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Interactive comment on "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.

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Answer to reviewer 1

We would like to thank the anonymous referee for his/her positive and constructive remarks on the manuscript.

You will find enclosed a point to point answer to those remarks. The pdf showing the modifications of the paper itself is also available for control.

The referee was suggesting that our results may be a bit oversold in terms of improvement of dust lifting representation. And we acknowledge that : 1) pieces of evidence were missing concerning the representativity of the comparisons which were focused in the first manuscript on observation stations which are on the South margin of the emission zone. Comparisons between the two model versions are now shown for the whole Sahara and for 2 consecutive years in terms of both distribution in the day of dust emission and diurnal cycle of near surface wind (Fig 10 in the new manuscript). This figure confirms the conclusions of the first version of the manuscript. 2) we may have given the impression that the LMDZ winds were better than ERAI to predict the total emission of dust for instance. It was not our intention. We just say that the diurnal cycle itself is much better represented with the NP version of LMDZ than with both the SP version and ERAI. It is true that for one station, the morning wind is closer to observation in ERAI than in LMDZ-NP, but it is clearly linked to a strong overestimation of wind in ERAI during the rest of the day. It may occur that, because of compensation errors, direct use of ERAI may give better results for dust lifting. And anyway for LMDZ, the problem is that we have to rely partly on ERAI (through nudging) to get the correct large-scale circulation in the simulations. We acknowledge however that we made too strong a conclusion, when suggesting that ERAI winds may be too strong in general. We have the wind observations on Sahelian stations only. So the statements on this particular point were removed from the manuscript, as detailed bellow.

We have to add in introduction to this answer that we found a small error in the computation of the Weibull distribution. A normalizing factor was missing, which was systematically lowering the emissions. We thus updated all the figures with the new simulations. No conclusion is affected especially because we are focusing on the sensitivity to parameterizations more than on the realism of the simulated dust distribution. Comparison with observation is now better for the dust (surface concentration and AOT)

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but we insist (as in the first draft) on the fact that this good agreement may be more a question of chance, since a number of parameters which were not explored here may affect emission. In particular, taking into account an a priori subgrid scale variability through Weibull distribution strongly enhances dust emission, and may be seen as a trick to compensate our inability to account for sub-grid scale turbulent or mesoscale processes. To simplify a little bit the discussion on this subgrid scale distribution, the W* term in the emission was omitted in the new set of simulations. All the figures were redone with those new simulations that rely on a somewhat upgraded version of the LMDZ model, which also marginally affects the wind but without changing any of the conclusion or comment.

The Reviewer comments are reproduce in "script" font together with the answers.

Hoping you will find our answer appropriate,

with best regards,

Frédéric Hourdin

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Reviewer #1: "Parametrization of convective
transport in the boundary layer and its impact on the representation of
diurnal cycle of wind and dust emissions``

General:

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.
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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.

We hope to have shown evidences that the NP version improves the representation of the
diurnal cycle of near surface wind. Since a salient feature of this diurnal cycle, corresponding
to the morning peak, is probably responsible of a large fraction of dust emission in this region,
we believe that it is step forward toward a more physical representation of dust lifting. As
said above, it is not possible to go farther in the comparison since the LMDZ simulations are
constrained through nudging by the ERAI wind. For the two stations we considered, ERAI
winds are stronger than observation and LMDZ. Because the overestimation by ERAI is much
larger at Cinzana than at Banizoumbou, it may give the impression that the LMDZ winds are
closer to observations at Cinzana. But we do not consider this result as such is a conclusive
evidence. On the other hand, the ratio of the maximum wind to the diurnal average, is very
close to observation at both sites as illustrated in the lower panels of Figure 4, which we take
as a quantification of the diurnal cycle representation.

 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 modeling. The effect of the new Weibull distribution relative
to the NP of plumes is not discussed.

Once again, we did not want to say that the dust was better represented but only that the
emissions were increased in NP compared to SP, due to the better representation of the diurnal

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cycle. It is true that we did not discuss the effect of Weibull, since it was not the purpose of the paper to discuss the various elements of the dust emission computation. In the new version, we give more details concerning the description of this Weibull parameterization and insist on its impact on the computation of dust in the conclusion.

```
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.
```

Mechanical turbulence may play a role in early morning, but we do not think that the turbulence in between 9:00 and 12:00 is predominantly shear-driven. Since we represent both shear- and thermally-driven turbulent diffusion plus the thermal plume model which is only thermally-driven in our model, we can at least confirm that thermal boundary layer convection dominates the momentum transport from the LLJ in our case. This point is made explicitly in the conclusion.

```
2. This might be not be an important point but
could the reduction of the time-step for the higher resolution runs have
an effect on the sub-grid scale parameterizations?  Did you reduce the
radiation timestep or leave it unchanged?
```

All the simulations shown here are done with the same grid and thus with the same time steps. But, we ran a specific simulation reducing the time-step for radiation and physics by a factor of 2 to check this point. It leads to a very small systematic reduction of the emission, by a few percent, probably due to a reduction of the numerical noise in the simulation of the boundary layer, but without altering any of the result shown here.

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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.
```

References are added: *" One of the important and uncertain dust related processes is emission which depends non linearly upon the friction velocity U* (Gillette, 1977; Nickling and Gillies , 1989, 1993; Gomes et al., 2003; Rajot et al., 2003; Sow et al. , 2009; Shao et al., 2011)."*

```
- 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.
```

Changed to: *"which coincides with the daily maximum wind speed in the observations in the Sahel."*

```
- 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.
```

This paragraph was concerning only the convective boundary layer, starting with : *"Various approaches have been proposed in the past decades to represent boundary layer convection."* We however rephrased the sentences above to avoid any possible confusion: *"[...] parameterizations of boundary layer turbulence that are based on eddy- or K-diffusion fail to represent the basics of boundary layer convection, which essentially transports heat upward from the surface. This transport is done upward the gradient of potential temperature since the atmosphere is generally neutral or even slightly stable in the so called "mixed layer" (typically several km thick in this region of the globe in the afternoon), above the unstable surface layer (typically a few-hundred-meter thick)."*

```
- 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.
```

We agree with the statement on the importance of wind shear-driven turbulence for the noctur-
nal boundary layer. The purpose here was more to insist on the the relative importance of the
two parameterizations at work in our model for typical diurnal cycle over land (not specifically
over Sahara) : turbulent diffusion and thermal plume model. However, we rephrased the sen-
tence to account for this remark : *" This approach was shown to capture well also the typical
diurnal cycle over land, contrasting a thin nocturnal boundary layer dominated by wind shear-
driven turbulent diffusion, and daily conditions in which the role of the parameterized turbulent
diffusion is confined to the unstable surface layer while the mass flux scheme accounts for most
of the turbulent transport in the mixed layer."*

```
- 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.
```

We are referring in both cases to the unstable surface layer, typically a few hundred meter thick.
We hope that the rephrasing done in reply to the two previous comments makes it clearer.

```
- 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.
```

Yes, of course. Added : *"primitive equations of meteorology (approximate form of the conserva-
tion laws for air mass, momentum and potential temperature, under hydrostatic and "thin layer"
approximation)"*

```
- Line 16/7: "large scale`` Ambient air is probably more appropriate
here.
- 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 $\alpha$th 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?

We tried to make all those details clearer in the revised manuscript. What we called "large-scale" variable was in fact the grid cell average $\overline{q}$, which is also the explicit state variable of the 3D model. In the approximation $\alpha \ll 1$ it is the same as the concentration of q in the environment of the plumes q_{env}. We now introduce those notations in the text to avoid ambiguities. This approximation is used in most mass flux parameterization. We added a reference to Tiedkte (1989) since he explicitly discusses the point when introducing the model (q_{env} being noted $\tilde{q}$). The approximation consists in replacing $e_{th}q_{env}$ by $e_{th}\overline{q}$ in the plume equation (Eq 1 of the revised manuscript). Details are now given in a footnote.

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

Done

– Lines 25–26/9: "the zoom was chosen so as to get a quasi uniform 1 $\times$ 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''?

We do not use nesting but a stretched grid. We tried to state this point more clearly in the sentences that precedes the above sentence : *"The zoom consists of a refinement of the global grid discretization in both longitude and latitude, here over West Africa and the tropical Atlantic ocean."*

– 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?

With the new simulations, the total emission is of 33 Mt for the SP version and 113 Mt for the NP version. It is still in the lower range of current estimates. We give more information on those estimates in a new footnote: *" Marticorena et al. (1997) report values of 163 and 101 Mt for 1990 and 1991 while considering only half of the Sahara. Laurent et al. (2008) compute mean emissions with ERA-40 winds for March (period 1996-2001) of the order of 80 Mt with a maximum value of 205 Mt while Schmechtig et al. (2011) compute emissions of the order of 300 Mt for March 2006 with ECMWF forecast winds."*

```
- 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.
```

As stated above, we do recognize that the extension of the discussion to the full emission region was missing in the manuscript. This is now done in the conclusions. The idea of selecting this particular location is given in the new version : *"We choose this particular point for illustration because it is located in the south of the emission zone, not too far from the latitude at which we show comparisons with in situ wind measurements in the following section. However, as shown later, the behavior observed at this particular grid point is representative of the whole emission zone."*

```
- 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?
```

As just said, we now show that the contrasted behavior of the SP and NP versions is similar everywhere on the region (new Fig10). As for the Weibull parameterization, it is activated

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exactly in the same way in both the SP and NP versions. So it can not explain differences. It is stated more clearly in section 2.3.

– 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.

Sorry if the message was not clear. We mean that the NP version improves the representation of the diurnal cycle. We propose to characterize this cycle with the ratio of the maximum to mean diurnal value, since we are especially interested by the maximum value for dust emission. As for the representation of the wind itself, it is difficult to draw any firm conclusion since the large-scale wind is for a large part constrained by the nudging term. Local effects may explain difference with observations in addition to that. However, because we have an explanation for it, and because of the consistency with observations, we think that the improvement in the representation of the diurnal cycle is similar on both sites. Of course, directly for emissions, it is the maximum value that is important and it can be better in one simulation or reanalysis due to a compensation between a poorly represented diurnal cycle and erroneous mean value.

– Line 26-27/13: "than the absolute mean value and mean field'' The meaning of this is not clear.

We do agree that the sentence was unclear. It was removed since the idea is much easier to get from the section on the mean diurnal cycle.

– 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.

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The change in concentration at M'Bour does not come from modification of the wind at M'Bour since their is no emission at M'Bour in this configuration. Once again it is difficult to fully assess the interplay between nudging and model physics. However, we can state that changing from SP to NP or, to a lesser extent, weakening the nudging (going from τ=3 to 48h) in both cases clearly enhances dust emission together with the amplitude of the wind diurnal cycle.

```
- 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.
```

We agree with the remark and do not think the sentence was adding much to the paper. It was thus removed in the new version.

```
- 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.
```

We made explicitly a reference as suggested : *"It is shown later that this good agreement is linked more generally to a much better representation of the mean diurnal cycle than in the SP version."*

```
- 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.
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As stated in the introduction of this answer, we do agree with this remark and removed these sentences and the corresponding statements in the conclusions.

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- 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.
```

It is true that the Richardson number is mentioned first in the description of the standard version of the model. It is clear also that the Richardson number could be used to characterize whether the turbulence is rather shear- or thermally-driven. We also agree that, during the night, turbulence below the jet is explained by mechanical production (shear-driven). However, at least in the model, the organized thermally-driven turbulence represented by the thermal plume model is clearly responsible for the downward transport of momentum that explains the wind maximum at surface in the morning. The turbulent diffusion, based on a prognostic equation for the turbulent kinetic energy that takes into account the shear-driven turbulence (as well as static stability) is acting the opposite way as visible in the lower panel of Fig9. The Richardson number used at this point in the paper is a different one. It is a so-called bulk Richardson number, the level 0.25 of which is used to identify the boundary layer height.

```
- 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.
```

Yes. We are aware that observations exist; but there were not available to us when we did this work. We intend to make some finer assessment/tuning of the parameterization in the future with such observations. However, we mention some indirect and qualitative comparison to published results: *" Note that a similar underestimation of the ERAI low level jet intensity is shown in Fig. 4 of Fiedler et al. (2013), when compared to observations in the Bodele region."*

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- 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
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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.
```

The explanations we gave were probably a little bit confusing. We rewrote this paragraph as follows. *" It is this peak of downward transport from the nocturnal jet which explains the morning peak in near surface wind. The mixing by thermals also rapidly reduces the jet intensity, reducing in turn the acceleration of surface winds by thermals subsidences. The near surface wind then decelerates slowly in the afternoon, under the effect of turbulent exchange with surface. The negative diffusive term (green curve in the second panel of Fig. 9) is almost compensated by the thermals tendency which accounts for convective exchanges between the surface layer and the mixed layer above. Both terms almost fall to zero after sunset, resulting in a decoupling that allows for the creation of the low level jet of the following night."* As already discussed above, we do not discuss this phenomenon in terms of Richardson number but rather in term of boundary layer convection versus turbulent diffusion, since it is mainly through this partitioning that our model distinguishes between shear-driven and thermally-driven turbulence. Even if the turbulent diffusion itself depends on the competition between wind shear and static stability through the TKE equation, the thermal plume model accounts for most of the vertical transport in the mixed layer in convective conditions. We think that going in this discussion would add more confusion to the explanation.

```
- 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.

We do not agree with this comment. Of course at a given time of day, ERAI can be closer to observations. But it is not to say that the diurnal cycle is better represented. Indeed, the diurnal cycle of surface wind speed is worse in ERAI during the dry season at both sites. A similarly poor behavior of ERAI during this season was also reported by Largeron et al. (2015) from the other sahelian more northern sites. We reformulate a little bit to specify what we mean by diurnal cycle : *"The mean diurnal cycle of the near surface wind is well captured in the NP version of the LMDZ model that includes these thermal plume processes, at the Sahelian stations considered here. It is much better represented in terms of mean value, phase and amplitude than in the reanalyzes used for nudging. "*

– 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.

We absolutely agree with this comment. So we change a little bit this conclusion to insist that we comment here first on the stations for which we have observations. We then added a new paragraph and figure (Fig10) that confirms that there is a small but systematic effect in the whole emission zone.

– 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 modeling. The credibility of the conclusions would benefit from a comparison in

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other seasons and years, which you say you have done but you do not
show.

We really did not want to say that the wind was better represented in the morning. Too strong a mean wind with a bad diurnal cycle, as is the case in ERAI, can produce a better wind and emission in the morning. We wanted to insist on the improvement of the diurnal cycle, that points to a much better representation of the boundary layer processes involved. Hoping that the modifications given to the text help avoid any confusion on this. Following the reviewer suggestion however, we added a new paragraph in the conclusion and the Fig10 that extends to other seasons and locations the findings of the paper.

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

We use K-DIFF for K-diffusion instead of TKE in the new version of Fig 9.

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.

Done.