Response to Anonymous Referee #1 interactive comments on “Can explicit convection improve modelled dust in summertime West Africa?”

Reviewers comments are given in black and responses are in green.

**General Comments**

The paper focuses on the improvement in modelled dust that could be achieved when the convection is explicitly represented. A total of 8 simulations run with the UM model over a warm season (May-September 2011) are assessed in terms of dust AOD and near-surface wind speed. These simulations differ with the horizontal grid spacing (40, 12 and 4 km) and the use of a convection parameterization. Against expectation, no significant improvement in modelled dust is achieved with the convection permitting simulation. Whatever the grid spacing, the dust AOD is poorly reproduced by the UM model. The study points out two major drawbacks: a too low wind speed and a wrongly fixed bare soil parameterization. While there is no attempt to solve these issues, the study is though of interest in the way the assessment is performed.

We would like to thank the anonymous reviewer for their helpful comments on this manuscript. We agree with the reviewer that there was nearly no change in the lifting of dust in simulations despite significant changes to the simulation methodology. Instead the lack of change is investigated in this work. We conclude that changes in wind speed associated with the representation of convection are insufficient to increase the dust raising potential overall. But, even under a regime with significantly higher winds the uplift would be strongly controlled (limited) by the seasonally static bare soil fraction and the depth of the shallowest soil moisture level in the Met Office Unified Model (UM).

**Specific Comments**

**Abstract**

Page 1, line 12, the cold pool outflows can have an important role in raising dust. However, it is not well established that their contribution can be over 50%. For example, Chaboureau et al. (2016) estimated the role of harmattan to 80% of dust emission (over the western Sahara and in June 2011).

We apologise for the misunderstanding created by this statement and **have changed it to reflect the local and seasonal nature of haboob uplift.** This statement is also later qualified later in the paper, saying that “cold-pool outflows contribute over 50% of the total uplift in some areas of the Sahara in summer (Marsham et al., 2013; Allen et al., 2013 & 2014; Heinold et al., 2013)”. The evidence for this claim is based upon observations of dust and wind speed in the Sahara (Marsham et al., 2013; Allen et al., 2013 & 2014) and simulations of convection permitting simulations with offline dust emission (not limited by restrictive surface characteristics; Heinold et al., 2013).
We would also like to highlight that the estimation of 80 % of emission associated with the Harmattan in Chaboureau et al., (2016) is for the “western Sahara” which excludes regions close to the Aïr and Adrar des Iforas where emission is “dominated” by afternoon emission in high resolution convection permitting simulations (haboobs). Therefore, the findings of Chaboureau et al. (2016) are not in disagreement with the fact that haboobs can (for specific regions and seasons) be responsible for over 50% of dust uplift.
We think that it is also important to recognise that simulations capable of producing organised convective storms will not automatically be able to represent the near-surface winds of real cold pools in terms of size, duration or wind strength. This creates a cold pool, dust-uplift, grey-zone where cold pools are present (allowing for convective organisation) but

are not resolved well enough to represent their full impact on dust uplift. **A statement to this affect has been added for clarity.**

**Introduction**

Page 2, line 30, Chaboureau et al. (2016) evaluated not only dust, but also 10-m wind speed. Even if the evaluation was done between models only (not against observations), this is of great interest as it differs much from your results (see comment below on Figure 7).

The statement indicating the difference between this work and Chaboureau et al., (2016) is to highlight the novel nature of the work and point out the differences between this study and the similar work already conducted. We agree that the difference in results is interesting and indicates that what is true for one model is not necessarily the case for another (albeit similar) model. That is why when making statements about the relevance of this work for climate modelling, we have been careful to indicate the similarity between dust fields in the SWAMMA simulations and those in CMIP5. It is also the case that work of this type will be of interest to the UM model development community given the importance of dust modelling for different stakeholders on a wide variety of timescales. **We are sorry for this mistake and have clarified that the analysis in Chaboureau does include analysis of 10 m winds.**

**Section 2.1**

Page 4, line 18, please give the height of the first model level. How is the calculation of the surface friction velocity U* sensitive to the model level?

**The height of the first model levels of simulations has been included for clarity (model levels are the same for the 40 and 12 km grid spaced simulations, with lowest at 10 m and are finer for the 4 km simulations, lowest level at 2.5 m).** The calculation of u* is internal to the model (boundary layer scheme) and is given as a model diagnostic.

More generally u* can be defined as

$$u^* = \sqrt{\frac{\tau}{\rho}}$$

Where $\tau$ is the shear stress and $\rho$ is the air density. $\tau$ can be defined as

$$\tau = \mu \frac{du}{dz}$$

Where du is change in wind speed, dz is height difference and $\mu$ is the dynamic viscosity. Given the insensitivity of air density, the shear stress is the dominant term. Assuming that the lowest model level is within the turbulent boundary near the surface, (which is a reasonable assumption with lowest model levels at 10 m and 2.5 m) then u* should be fairly insensitive to changes in the height of the lowest model level. This seems to be the case in the SWAMMA simulations with there being no clear relationship between the friction velocity values present in the 12 km simulations and those in the 4 km simulations, which have different level spacing and might have the potential to impact on friction velocity values.

Page 5, line 16, the issue is not to make give a fair comparison. Instead, it is to simulate AOD well for the right reasons. The question is thus on the scale awareness of the dust scheme. As the dust scheme depends on the surface friction velocity, which depends itself on the grid spacing, this requires an adjustment of the model values. So does the mixing length of the 3D scheme (page 5, line 30).

Wherever possible the simulations have been kept as similar to one another as is feasible. That way we can be as confident as possible that the differences in the results are due to the very small number of differences between each simulation's setup. Our results are consistent with past studies (Marsham et al 2011, Heinold et al 2013) that for the UM at these grid-spacings total dust uplift potential is almost independent of horizontal grid spacing and therefore there was no need to retune. This is in contrast to coarser grid spaced simulations (eg the Ridley et al., 2012). Allowing for a more straightforward comparison unmuddled by retuning that maybe needed for lower resolution models.

Page 10, Figure 2, does the MODIS AOD provide a reliable reference for model assessment? It should be of interest to show as well the other product you use, the SEVIRI AERUS-GEO AOD. This is valid also for Figure 6.

The MODIS combined product utilises both Dark Target and Deep Blue algorithms to be able to retrieve aerosol optical depth over both ocean and land (including arid, high albedo surfaces). Sayer et al., (2014) gives a detailed analysis of the performance of both algorithms with the MODIS AOD comparing well globally with AERONET AOD retrievals (R = 0.92) with the MODIS and AERONET AOD distribution being centred around the 1:1 line. In dusty regions there is a tendency for MODIS to underestimate AOD values. However, the spatial distribution of dust will be well represented and allow for a good comparison with the spatial distribution of modelled dust. Also, given the very low values of AOD in the simulations compared to retrievals, the absolute value of AOD in MODIS becomes less important for analysis.
While there will always be differences between different retrieval products such as MODIS and SEVIRI AERUS-GEO these differences are small compared to the model-retrieval differences. MODIS is a much more widely used and trusted product and it is a simple procedure to download and process large amounts of data. Its use in figures 2 and 6 reflects this as regional-monthly averages can be computed. The SEVIRI AERUS product is available through the icare website (Univeristy of Lille). I had hoped that I would be able to fulfil your request to add the SEVIRI AERUS-GEO data to figures 2 and 6, however, downloading data (as opposed to imagery) for SEVIRI AERUS-GEO appears to be more complicated than anticipated. I have contacted icare but have yet to hear back about the availability of AOD data.
The SEVIRI AERUS-GEO AOD imagery remains the preferred option for figure 12 due to its better spatial coverage allowing us to show the retrieved dust distribution for a single case study event. It serves to show the absence of dust AOD associated with the MCS cold pool in the 4 km simulation and indicates that even when we might consider the simulation to have done a good job in representing deep convection, the cold pool produced is not able to raise anywhere near enough dust compared to observations.

Page 10, Figure 3, why the models are quite good in May and very poor the other months? This seems to be due to a decrease in u*. So, why does u* decrease with month?

In May there is a lot more dust than in other months, however their AOD maxima is too far north, it is not in a latitudinal band as seen in MODIS retrievals, and the well-known major hotspot of the Bodele depression is much too weak.
There is generally a decrease in u* in the Sahara and Northern Sahara from May to September. However, the pattern in the Sahel is less clear as shown in Figure 6. Given that emission is a cubic function of u* associated with exceedance of a u* threshold, it is no surprise that with dropping u* values (possibly associated with a weakening Harmattan after the start of the monsoon onset and developing Saharan Heat Low) that AOD values would also fall. We see that there is only a weak growth in afternoon and evening winds and an unrealistic drop in nocturnal low level jet winds in convection permitting simulations. As discussed in the paper, this can be attributed to cold pools that are too small and too weak. Cold pools are known to be key to dust uplift in the Sahel and southern Sahara from observations (Marsham etal., 2013, Allen et al., 2013, 2014) and given the recirculation of

dust around the Saharan Heat Low would go a long way to explaining the peak dust location growing through June, July and August in MODIS AOD.

Page 12, Figure 7, Chaboureau et al. (2016) compared the 10-m wind speed over western Sahara for models with parameterized convection and without and found wind speed up to 15 m/s. This is the case for the ALADIN model run with a 24-km grid spacing for which cold pools were not expected to be simulated. This result strongly contrasts with the one shown for the UM model. This suggests a drawback in the UM model wind speed that would be not specifically related to the representation of cold pools. Further, the convection-permitting models in Chaboureau et al. (2016) show wind speed up to 25 to 30 m/s (due to cold pools), a much larger value than obtained from the UM model.

The model wind-speeds plotted in figure 7 have been height adjusted for comparison against observed winds at 2 m. This means that the 10 m model winds will be higher than those shown in Figure 7. **We have added information about this technique and indicated that this brings modelled winds speeds close to those simulated in parameterised models in Chaboureau et al., (2016).**
The indication that the convection permitting models in Chaboureau et al., (2016) have an increase in frequency of strong winds associated with haboobs does suggest that they are doing a better job that the UM in representing the effects of cold pools. The work presented here indicates that (for the UM at least) that the cold pools produced are too small, too weak and do not raise dust in a way that is consistent with in situ observations or satellite retrievals, it is also possible that the increased windiness shown in Chaboureau et al. (2016) is also too weak. **We understand that this result does contrast with that of Chaboureau et al., (2016) and have added some detail about the differences. We have also indicated that the conclusions made are primarily relevant for the UM and simulations that share similarly poor AOD distributions, such as those in CMIP5.**