

Thank you to the reviewer for their insights on the paper and research. We believe the manuscript is stronger based on their comments and we are appreciative of the thoughtful advice included here. Each comment will be addressed point by point. The * will denote line numbers in the tracked-changes manuscript.

General Comments:

I know that it is common parlance in the community to refer to simulations that are run without the use of convective parameterizations as being “explicit” or that convection is “explicitly represented”. However, more recently there has been a shift towards the use of simulations of this type being referred to as “convection permitting”. This difference is subtle but I think is a better descriptor of what the models are actually doing. The model grid-scales involved are not so fine as to explicitly resolve individual updraughts and downdraughts but are sufficiently high to permit the development of convective storms that approximate those that we might observe in reality. I feel that it would be better to replace descriptions of simulations currently described as explicit with convection permitting.

This is a very good point. We agree that the term “convection permitting” is a more accurate description of the representation of convection in the model compared “explicit.” Like the community, we have made the mistake of equalizing the two terms in the manuscript, when really only “convection permitting” should be used. The manuscript has been updated to replace “explicit” where possible with the terms “convection-permitting” and “convection-allowing.”

Did you consider running a 15 km simulation with the convective parameterization switched off. I don’t think that you should do this as the work is already of a high standard, but think that you might well be surprised at how small the difference is between a 15 km grid-spaced convection permitting simulation and a 3 km grid-spaced convection permitting simulation

We actually did run a 15 km simulation without a convective parameterization, but decided not to include the results in the manuscript. The spatial and time averaged results from the no parameterization case are, in fact, similar in magnitude to running at 15 km with a cumulus parameterization. Differences do occur in the timing of the different local dust maxima throughout the day. This points again, to resolution being the dominate factor to control in this simulation rather than the choice of cumulus parameterization (or the choice to even employ a cumulus parameterization at that grid spacing at all). A short discussion of this has been added to the manuscript:

Ln 223-224* [A 15 km simulation with no cumulus parameterization was also tested, but the results were similar and within the spread of the 15 km simulations that employed cumulus parameterizations and are not included here.]

Specific Comments:

Ln 17-20 You need to be clear that the updraughts that are transporting dust vertically are part of the general circulation (eddies) in the dry atmosphere. At first I thought you were specifically talking about storm updraughts (which I assume are less important in the simulation for vertical dust transport due to washout).

It’s a combination of both, but yes, the storm updrafts are mediated by wet deposition, whereas the dry eddies are not. This point has been included:

Ln 67-69* [Current aerosol forecast and climate models are run at fine enough grid-spacing to simulate synoptic events but still typically employ cumulus parameterizations, which are incapable of resolving dry and moist mesoscale updrafts and downdrafts that can potentially loft and / or scavenge dust.]

Ln 45-47 I think it would be wise to indicate that in reality ingestion of this type is impossible. What you are hoping for is that the initialisation data and the representation of dust are good enough for your purposes. It is perfectly possible that that is true for this case study but that the same setup run for different case studies could provide different results due to the high dependency of models (even those that do not contain dust) on initial conditions.

We agree with the reviewer's point. The spread across models (and within the same model based on physics options) can be vast. More has been included in this section to emphasize the limitations here based on model and case study choice:

Ln 47-49* [Even the state-of-the-art models are currently incapable of this type of assimilation and rely on the quality of the dust model and initialization data, which models are known to be especially sensitive to and will vary depending on the specific region and case study.]

Ln 47-49 Is it the global and regional nature of models that causes these differences or is it the grid-spacing or other model differences? Please be clear.

The dust model inter-comparison studies listed in the text varied in terms of grid resolution (horizontal and vertical) and model physics (including the dust schemes), even for the same case study. However, the grid resolution of the models was consistent in that they were all at grid-spacings that would employ a cumulus parameterization. The literature referenced here was not comparing global versus regional, but if those studies exist we are interested to see the results. The text has been updated to reduce confusion here:

Ln 49-52* [As such, substantial discrepancies exist across global models of similar resolution (Huneus et al., 2011), and across regional models (Uno et al., 2006; Todd et al., 2008) in the magnitude of predicted dust flux from the surface to the atmosphere, as well as the models' overall representation of the dust cycle.]

Ln 50 I would get rid of "accurately" here. Generally in models dust processes are fairly simplistic and highly parameterised and so the idea that dust processes are accurately represented is a fallacy.

True, it's a stretch to say that the highly parameterized physics in the model could be thought of as "accurate". The word "accurately" has been removed from this and the next section.

Ln 53-59 This section needs rewording. The first sentence along with the word "Additionally" suggests that large-scale, synoptic-scale and meso-scale meteorology is separate from the phenomena listed below. Also why say large and synoptic scales? Instead I would suggest something like "Dust uplift events can be associated with meteorological processes across a broad range of scales. Synoptic scale uplift phenomena include monsoon troughs (Marsham et al, Beegum et al), Shamal winds (Yu et al.) and

frontal systems (Beegum et al). While dynamical effects on smaller (meso) scales can raise dust through the production of convective outflow boundaries (haboobs; Miller et al.) and the morning mixing of nocturnal low level jet (NLLJ) momentum to the surface (Fiedler et al)."

Thank you for the clarification. The wording suggested by the reviewer is a welcomed improvement and has been included in the text:

Ln 57-61* [Synoptic scale uplift phenomena include monsoon troughs (e.g. Marsham et al., 2008), Shamal winds (e.g. Yu et al., 2015) and frontal systems (e.g. Beegum et al. 2018), while dynamical effects on smaller (meso) scales can raise dust through the production of convective outflow boundaries, or haboobs, (e.g. Miller et al. 2008), daytime turbulence or dry convective processes (e.g. Klose and Shao, 2012), and the morning mixing of nocturnal low level jet (NLLJ) momentum to the surface (e.g. Fiedler et al. 2013).]

Ln 60 What other drivers of dust emission are there? There are prerequisite conditions (dry, unvegetated surface etc.) but wind is the only driver of surface dust emission that I can think of.

Possibly some anthropogenic activities can emit dust (e.g. plowing agricultural fields, construction, etc.), but ultimately, it's still then transported away from the source by the wind. This line was replaced to point out that wind is the only driver (albeit modulated by other conditions) and that we are only considering meteorological processes here:

Ln 61-62* [When considering only meteorological dust sources to the atmosphere, wind drives dust emissions...]

Ln 73 Heinold used offline emission which I think is a relevant point to mention here as it significantly differs from your approach. Another paper that discusses the grid-scale effects on online model dust and convective representation of dust in West Africa would be Roberts et al. 2018 (doi.org/10.5194/acp-18-9025-2018).

Yes, that is definitely worth mentioning and has been included. It's an important point for understanding the importance of the DUP parameter in the context of other studies. The Roberts et al. 2018 paper has also been added to the literature review to better place our results in the context of existing literature:

Ln 78-79* [Heinold et al. (2013) ran the UK Met Office Unified Model (UM) over West Africa with offline dust emissions, and found that...]

Ln 86--88* [Roberts et al. 2018 also used UM to investigate this relationship over the Sahara and Sahel and reported little change in the dust emissions when moving from parameterized to explicit convection, but also noted that the NLLJ maximum decreased as the convective maximum increased.]

Ln 82 One thing that you don't mention is that the thing that affects models the most is not the grid scale, or the microphysics and in some cases not even the whether simulations are convection permitting or parameterized. It is the initialisation data. This is one of the findings in Schepanski et al. 2015 (doi.org/10.1002/qj.2453) in West Africa.

Naturally, the model initialization data are going to be either a substantial source of error or accuracy in the output data. We have added this note and reference to the manuscript to remind readers that the findings here will be modulated by the initialization data:

Ln 70-73* [Schepanski et al. 2015 found that online dust models are likely to be most sensitive to the initialization data compared to other model options, model sensitivity to the representation of convection will be an additional source of uncertainty in dust forecasts.]

Ln 104-114 Roberts et al. 2016 (mentioned above) covers some of these points by using the Met Office Unified Model over West Africa. In the UM over summertime West Africa at least, the grid spacing does very little compared to representation of convection.

These findings have been added to the text (see above comment). But, despite the model and the region being different between these studies, we have found similar results.

Section 2.1 I find the ordering here a little odd. I would normally expect the model description to precede the description of the conditions that caused the dust uplift. It feels a little like you are skipping backwards and forwards between results and methods. I advise moving your current section 2.1 to either the end of section 2 or the start of section 3.

The case study description has been moved to the end of Section 2.

Ln 144-145 I don't think that Figs 1 and 2 show this. The first shows a number of different fields (not dust) and I wouldn't describe Figure 1 as the meteorological setup either. Figure 2 is actually 2 profiles which doesn't match the description either. Please be much clearer in you description. I cannot tell what you are referring to.

Thank you for pointing this out. We agree that it's more like a snapshot of the meteorology than an analysis of the meteorological setup. A more in-depth meteorological analysis of this case study and an attribution of the dust to different meteorological sources can be found in Miller et al. (2019) and we have directed readers there if they are interested:

Ln 258-262* [A meteorological analysis of this event, including an attribution of specific dust sources to meteorological features can be found in Miller et al., 2019 and will not be reiterated in detail here. Rather, a snapshot of the meteorology and dust fields from the WRF-Chem simulation on August 3rd at 15:00:00 UTC can be found in Fig. 1-2 as a reference to the typical meteorological setup for this case study.]

Ln 180-187 A very brief description of why these parameterizations were chosen would be welcome. For instance is this a replication of a setup used in a similar study? Is it similar to operational setups of WRF that are run for similarly arid regions? Or is there an individual reason for having chosen each of these options.

A reference was added to point out that similar WRF physics options have been used in dust studies in this region:

Ln 163-164* [The following model parameterizations were employed and kept constant across the simulations, with similar WRF physics options being utilized elsewhere to study dust effects (e.g. Alizadeh Choobari et al. 2013:)]

Ln 289 You should say why the soil moisture is more likely to fall below the threshold in the convection permitting simulations. This is very likely associated with the different way in which rainfall is generated in parameterised and convection permitting simulations. Parameterized simulations have much more widespread light rainfall while convection permitting simulations have rainfall over much smaller areas but at much higher rates. The smaller areal coverage of rainfall in the convection permitting simulations is most probably the cause of the soil moisture threshold not being exceeded as frequently.

The comment about rainfall affecting the soil moisture is on point. Thank you for raising it. We had similar ideas about this mechanism and have expanded this section to discuss these processes more.

Ln 308-313 [Rainfall is generated differently in parameterized versus convection-allowing simulations, and it has been well documented that parameterized simulations produce more widespread light rainfall, whereas more intense rainfall tends to develop over smaller areas in convection-allowing simulations (e.g. Sun et al., 2006; Stephens et al., 2010). From a domain average perspective, rainfall in the 3 km simulation will cover less area, leading to the soil moisture threshold not being exceeded as frequently compared to the parameterized cases.]

Ln 306 August 3rd

Typo has been corrected.

Ln 329 Given that Heinold and Marsham both use the UM (and I don't know what the others used but I suspect not the UM) I think you should comment on the possibility that this is a difference in model physics that is driving the different behaviour.

We have added this point throughout the manuscript to remind readers to be cognizant that the models are different and have different physics.

Ln 358-361* [...who used COSMO-MUSCAT and RAMS-DPM respectively. Considering each study used a different model and therefore physics, it is unsurprising that the results vary. However, it is not apparent how much of a role the region or specific case study plays in this difference, and is an area for future work.]

Ln 364 Once again you are not trying to explain the reason for this. In modelling of convective storms it is a well known phenomena that the radius of updraughts and downdraughts scales with the grid spacing. Could it not just be a similar effect you are seeing here. The same overall vertical motion occurs but not over such a large area (due to updraught and downdraught scaling with grid spacing) and therefore the average of grid points with non zero vertical wind speeds is relatively higher.

We agree that the scaling of the updraft / downdraft radius with grid spacing is well-known, and this is most definitely a factor here. But pushing this argument further, the finer grid spacing could permit points with higher, lower, or near-zero vertical velocities compared to the coarse spacing. The average does not necessarily have to skew higher and without testing we wouldn't know how that plays out. In

this case, the results skew to higher velocities, which is evident in the CFADs (Fig. 9). We are more likely to witness higher vertical velocities rather than lower or near-zero velocities in the 3 km simulation compared to the coarse simulations. These discussion points have been added to the section.

Ln 398-400* [It is known that in numerical models, the updraft radius scales with the grid spacing (e.g. Bryan and Morrison, 2012), with a compensating increase in updraft speed as the radius decreases. This relationship skews the frequency of vertical velocities to higher values.]

Ln 365-366 This needs to be reworded. At the moment it sounds like you are saying that the mean updraught speeds (throughout the depth of the model) are greater than the mean downdraught speeds near the surface. I suspect what you mean is that nearsurface updraughts are greater in magnitude than near-surface downdraughts (would also be nice to give a height blow which this is true).

Your interpretation is correct – the text has been updated to remove this confusion.

Ln 400-402* [Irrespective of resolution, the mean updraft speeds in the WRF-Chem simulations are slightly higher than the downdraft speeds, while at the surface mean downdraft speeds are higher than updraft speeds...]

Ln 395 “in the absence of any” ?

Discussion and recommendations and Conclusions. Do you really need both sections. There is a good deal of repetition between the two sections straight after one another. I would prefer a single Discussion and conclusions section (afterall, surely recommendations are a conclusion you arrive at from doing the work).

After considering this point, we decided to keep the sections as is and leave the result section more quantitative, with the discussion being more qualitative.