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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Convective distribution of dust over the Arabian Peninsula: the impact of model resolution" *by* Jennie Bukowski and Susan C. van den Heever

Anonymous Referee #2

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General comments

This paper aims to use the Weather Research and Forecasting model in its chemistry mode (WRF-Chem) to simulate the raising of dust for a specific case that occurred from 2nd to 5th August 2016 over the Arabian Peninsula. Simulations have been performed using a variety of grid spacings and convective parameterizations as well as explicitly representing convection in some cases.

Generally I think that this work is of a high standard and is well written. The reasoning and thoughts of the authors are clear and contextualised well in earlier literature. For this work to be published I would recommend only minor editorial changes.

I liked the of using different complexities of the DUP metric whereby different versions of DUP were generated and compared with one another. This method allows for each of the added levels of complexity to be assigned to a different physical property within the simulation and therefore the magnitude of the effects can be seen.

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 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.

Did you consider running a 15 km simulation with the convective parameterisation 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.

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 atmopshere. 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).

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.

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.

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.

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 dyamical 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)."

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.

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).

Ln 82 One thing that you don’t mention is that the thing that effects 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.

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.

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.

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.

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. 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 in 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.

Ln 306 August 3rd

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.

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 updarughts and downdraughts scales with the grid spacing. Could it not just be a similar effect you are seeing here. The same overall vertical motion occurrs 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.

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 near-surface updraughts are greater in magnitude than near-surface downdraughts (would also be nice to give a height blow which this is true).

Ln 395 "in the absence of any" ?

Discussion and reccomendations 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).

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