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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 #1

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General comments:

The authors study the emissions and transport of mineral dust aerosol in the region of the Arabian Peninsula. In this region, along the coast, dust emissions may be huge, espcially during convective events. With a regional model (here WRFchem), emissions are primarily sensitive to the near-surface wind speed (in general the 10m wind speed, due to the parameterizations used). The authors made a sensitivity experiment by comapring dust emissions and concentrations, their impact on radiation, by using the same model but with different convection schemes. The 'reference' case is a simulation over the same region/period but with a resolution high enough to explicitely treat the

convection.

Detailed comments:

l.175: What is the meaning of 'coupled'? Probably only the use of the mineral dust emissions module (but not the transport, mizing, deposition etc.). Please better explain.

l.177 More details are needed about the schemes used. The paper is a sensitivity study about these schemes and they are not explained. In particular, the way to treat the aerosol for the indirect effects is completely different (the Grell scheme is aerosol aware compared to the others).

l.178: for a mineral dust study "no chemistry" why not. But no initial and boundary conditions for a simulation of 3 days, it is not possible to have realistic results.

l.182 "kept constant" meaning remain the same during the whole simulation?

l.214: The 'Dust Uplift Potential' is a calculation already done in a large majority of dust emissions schemes, by principle of the mechanism to evaluate. Unfortunately, it represents only a small part of the problem and is not really useful. It describes only the link between the friction velocity treshold (using the aeolian roughness length) and the current friction velocity. But other important parameters are not taken into account: the vegetation, the erodibility, the soil humidity, the recent precipitation etc. In addition, the fact to use a constant Ut is not realistic (eq.3): the aeolian roughness length is far to be constant over erodible region. It is the most important varying parameter in mineral dust emissions modelling. The use of three different kind of DUP has a large interest. The message is already contained in one. If the authors really want to use this criteria, only one is enough.

l.239: The reference simulation has an horizontal resolution of 3km to enable explicit convection calculation. This simulation has boundary conditions and this is a good point. But these boundary conditions are from the BMJ simulation, i.e one of the studied case. Thus, we can think that the reference case will be very influenced by this case, no? To have a more realistic comparison between scheme, the 'reference' has to be done for each scheme and a first spread can be calculated between all 'high resolution' cases.

Figure 4: this figure clearly shows that the most important choice is the horizontal resolution and not the convection scheme.

l.263: for long-range transport, 24h of spin-up is not enough. For the time averaged results, it is only th elast two days. But for the time series, it is the 3 days? why this difference?

l.272: why not use directly the mineral dust emissions fluxes? Please explain this important point.

l.280: why the simulation with the coarsest resolution (and not simulation) overestimates the wind speed? Please explain (and I imagine it is the "10-m wind speed", please correct).

l.293: Yes, it is right. And obvious. Of course, a key point in modelling is to try to have a model not sensitive to the spatial resolution. And it seems it is the problem with WRF-chem. In WRF, the principle is to use, for each grid cell, the dominant soil type and landuse. Thus, by principle, the result is very sensitive to the resolution. Some other models are using subgrid scale variability and Weibull distribution for the 10-m wind speed, for example, to avoid this problem. Please see bibliography and replace WRF-chem in the context of all currently used regional dust models.

l.331: it is not sure that there is an interest to have a conclusion such as "resolution increases or decreases the mineral dust emission fluxes". In fact it depends on the studied area, the variability of the orography, aeolian roughness length, soil humidity, vegetation. And, of course, the way to well take into account or not all these processes and their variability.

l.335: I don't understand the discussion with "The rates of gravitational settling are

higher in the explicit simulation compared to the coarse simulations, yet Fig. 6.a suggests that this is not enough to offset the higher dust emissions, or the integrated dust quantities would be similar across all the simulations." The dry deposition is proportional to the concentrations, being a velocity applied to the concentrations. How is it possible to have 'enough' settling to 'offset' the higher dust emissions?

Figure 6: the fact to have difference sbetween resolution is understandable but a factor 2 has to be better explained. Mineral dust emissions mass maps for the common domain (the one with 3km horizontal resolution). The caption is not easy to understand: "Domain averaged integrated dust mass". Please correct with Spatially averaged, vertically integrated.

l.346: "the vertical dust profile follows a generally exponentially decreasing function" is it a conclusion of this study? or coming from a reference? These is no reason to have an exponential decrease in the troposphere. Many cases of thin but concentrated dust plumes transports are observed and modelled...

l.369: "The implications for dust transport based on vertical velocities is convoluted." This sentence is difficult to understand.

l.421: The impact on radiation, with potential heating and cooling, is a process needing more than 2 days of simulation to be significative.

l.428: there is a sign change. Could you explain why?

Conclusion of the comments:

The study suffers of several issues, mainly methodological.

1. There is no data used in this work: the simulations are compared between them but we have no idea of the realism of the simulations (there is only one reference for a comparison to Aeronet AOD in another paper, under discussion, and no guarantee this is exactly the same model set-up, and which one?). At least, the reference case (dx=3km) should be compared to available data (surface networks such as MIDAS,

AERONET, satellite, other data).

2. The studied case extended from 2 to 5 August 2016: there is no spin-up time, important when studying transport of aerosol such as mineral dust. Time series are presented for the three days, but some average are done only for the last two days, explaing that the first day is spin-up. But, viewing the domain size, the minimum spin-up time should be at least one week.

3. There is no boundary or initial conditions. These missing background values may have a large impact on the results, in particular knowing that the model couples the meteorology and the aerosol concentrations: direct and indirect aerosol effect may be long-term and it is required to have correct boundary conditions to have realistic effect of aerosol on meteorology. For the 'reference' domain, the boundary conditions are extracted from one of the studied case, biasing the results.

4. The convection schemes used are not explained. The paper is a sensitivity study about these schemes but there is no explanations about their real differences, how they take into account aerosol or not, thus no conclusion about why results may be different depending on the scheme.

5. The paper deals with the sensitivity to the model resolution. But since the schemes are not well implemented (no wind speed distribution, no subgrid scale variability), there is a large sensitivity but not for realistic and physical reasons: the differences are not due to the convection schemes in general but just to the fact that the problem of the resolution is not well designed in this model: it is not possible to describe a treshold problem (such as mineral dust emissions) without taken into account disstributions of input parameters. Results are linked to this model only and are not useful for other modellers.

In conclusion, I recommend 'rejection' to give a chance to the authors to really re-design the paper. It is obvious that all calculations have to be reprocess in order to have a minimum of confidence in the results. More important, the main scientific goal has

also to be revised: it is not possible to conclude for a sensitivity study, by running only 3 days over a large domain, with a coupled model and for mineral dust aerosol (long-range transported species, high SW impact), and without boundary/initial conditions.

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