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Comment on acp-2022-94

Anonymous Referee #3

Referee comment on "Modeling coarse and giant desert dust particles" by Eleni Drakaki et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-94-RC2, 2022

This paper addresses the by now seemingly well established underestimation of coarse and giant dust particles by large-scale models. This is an important topic, as these particles are much more abundant than previously thought, such that models could be missing important effects on radiation, clouds, and biogeochemistry. The present paper tries to address this issue by using in situ measurements of dust size distribution over the North African source regions to parameterize the sizes of emitted dust in the WRF-Chem model and then comparing the results against. They find that the deposition velocity of particles must be greatly reduced in order for the model to match measurements further from source regions, which further confirms previous findings in the literature that coarse dust deposits too quickly in models.

Overall, this is a useful contribution to the literature. I did find a series of issues with the description of the methods and results. None of them are serious enough to preclude publication and I'm hopeful that a next version would be suitable for publication. Nonetheless, major revisions are required.

Specific comments:

- I think the paper should be clearer about the actual objective of the paper is or the scientific question it addresses. If this is just to "extend the parameterization the mineral dust cycle in the GOCART-AFWA dust scheme of WRF4.2.1 to include also coarse and giant particles" then this is pretty narrow and perhaps better suited for GMD or a similar journal. But it seems that the authors also investigate the reasons for why coarse and giant dust is underestimated by models, finding that particles settle much too fast in the model. I would suggest making this objective of the paper clearer, especially in the abstract and the end of the introduction.

- I'm puzzled by the lengthy discussion of the inclusion of a new drag coefficient in section 2.1.2. I understand that a drag coefficient parameterization that is valid for larger Re number must be implemented since you're treating coarse and giant dust (with Re up to 10 or so), but I think the drag law you use (Eq. 14) is fairly standard. So rather than taking up the reader's finite attention with this lengthy description, I recommend you just state you implemented the drag coefficient law from Clift et al. (2005). Additionally, you should show that implementing this new drag coefficient law is actually important by including a plot of the new and old drag coefficients versus particle size.
- This paper was posted online a few days before the publication of a rather similar paper by Meng et al. in GRL that also found that the settling speed needs to be greatly reduced for a large-scale model to match measurements of coarse and giant dust particles. A brief comparison between the results in the two papers should be included.
- Lines 135-140 and Fig. 2: Here and elsewhere in the paper (section 2.2.2, Figure 5), not enough detail is provided on the used in situ measurements. Please describe exactly which runs were used for this data, how measurements were averaged over different runs and any other processing. Which instruments of the FENNEC and AER-D data did you use and how did you treat data that overlapped in the particle size range? And please include the measurement uncertainties and describe what's included in them.
- 240-241: "The fine resolution increases the accuracy of the dust simulations and provides a good estimate of the missing mechanism." Please include either citations or original results that support this statement. Also, how does the fine resolution affect the numerical diffusion in the model? And please include a discussion in this section of the numerical diffusion in WRF-Chem as Ginoux (2003) hypothesized this to be a main factor in why coarse dust particles deposit too quickly in models. Currently, there's only a brief mention of this in the last paragraph of the paper but not really any discussion of how big a problem numerical diffusion is in WRF-Chem and thus of whether it can explain your results.
- Section 2.1.4: here the effect of asphericity on dust extinction is neglected, which could be substantial. I think that's fine as the focus is on the size distribution, but please note that simplification.
- (16): here the units for dust mass concentration, particle density, and diameter don't match (they all use different length scales). Please correct.
- Line 273: please elaborate on how you are "taking into account the absolute difference between WRF forecast time and Aqua overpass time"
- I find Figure 5 hard to interpret and I think a lot more information is needed here. The text notes (L. 347) that this result is for "an emission point in Mali" - could you indicate exactly what location? And are the model results here for the closest grid box? Did the model include emissions only from that grid box or from the entire domain? And see comments above on more details needed for the experimental data. Is this the same data as shown in Fig. 2a, except sorted into the five bins? And could you also include uncertainties on the measurements? I also recommend including your parameterized size distribution at emission to help interpret the model results.
- L377-380: Why do you average over the eight neighboring grid points when you're already interpolating the measurements? Some more explanation is needed here.
- Figure 8: Please describe what exactly the error bars represent. Is this derived from the counting uncertainty in a given run? Or the standard deviation (or standard error?) over several measurements?
- Also for Figure 8: I find the results in Fig. 8a puzzling. The measurements shown here are at the very lowest level, only 38m above the ground. So presumably, these measurements were part of the data used in Fig. 2 to parameterize the emitted size distribution, is that correct? Then why does the model do so poorly in reproducing these measurements so close to the surface? Please show the emitted size distribution in this plot to help the reader interpret your model results. Please also discuss why the

model does not capture the measurements so close to the ground, where errors in deposition would presumably have not as much impact on the results.

- Figure 10: What are the grey, yellow, and blue shading here?
- Discussion and conclusion section: As written, this is really only a discussion section. I recommend the authors add a summary of the results of their study for the reader.
- 441: The gravitational force acts on the center of mass and thus does not create a torque. Perhaps you mean that the aerodynamic force creates a torque? Please correct.
- 438-455: This is an interesting discussion of the effects of shape and particle orientation on settling speed. It left me confused on a few points though. The text states that "prolate spheroids fall faster than their spherical counterparts" even though their surface area is larger. How is that possible as more surface area would create more drag? This conclusion is also opposite of results in, for instance, Ginoux (2003). Do you perhaps mean that for this statement to apply to the special case when the prolate spheroid is aligned with its longest axis in the vertical direction, such that its cross-sectional area is smallest? If not, wouldn't the drag of the spheroid relative to an equal-volume sphere depend on the orientation, which itself is unknown as it depends on a variety of factors including the electric field (per Mallios et al. 2021)?
- Later in this same section you seem to state the opposite conclusion (L. 452-5), that prolate spheroids do fall slower than spheres. But I think here the difference is that you're comparing it to spheres of the same max dimension (rather than volume)? I think this is quite confusing to the reader and I recommend you focus on the comparison that could actually explain that particles settle slower than your model simulations predict. And these measurements are presumably for volume-equivalent spheres? Or are these optical diameters, so it depends on particle index of refraction and the shape of real dust particles? That should also be discussed in section 2.2.1 for the discussion here to add value. In general, I think the discussion on the effects of asphericity on settling should be presented more clearly for the statement on L. 476 ("the particle asphericity seems to be a strong candidate for the suggested corrections") to make sense to the reader.
- I think the author contribution sections requires more detail. There are a large number of authors with only a generic description of their contributions, with only the descriptions for ED, VA, AT, EP, and AG more specific. I think the contributions of each individual author should probably be spelled out more.

Technical corrections:

- Can you provide a reference for Eq. 10?
- 138: "upwelling" is probably not the right word here
- 184: "become is" à "becomes"
- Line 448: I think ellipsoids here should be spheroids