

Atmos. Chem. Phys. Discuss., referee comment RC3
<https://doi.org/10.5194/acp-2022-94-RC3>, 2022
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Comment on acp-2022-94

Anonymous Referee #2

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-RC3>, 2022

The authors apply the WRF-Chem model to simulate coarse and giant dust (besides the fine dust). For this purpose, they modified the dust transport bins in WRF-Chem, applied a modified (observations-derived) pre-defined particle-size distribution (PSD) to dust at emission and also modify the settling velocity to be applicable beyond the Stokes regime. With their modified model version, they conduct sensitivity runs with reduced settling velocities to test the impact of settling velocity compared to aircraft dust observations.

The study is timely and interesting, but I see two major weaknesses, one related to the comparison with observations and the other related with the transfer of the simulation results to processes other than settling velocity. I detail those aspects below besides other specific comments.

While the manuscript is overall well organized (although I suggest some changes, see below), grammar and orthography need to be improved throughout the manuscript.

Main comments:

The authors distribute the total emitted dust mass (all sizes) across their new bins using a prescribed PSD obtained from aircraft observations (FENNEC-PSD) at 1 km altitude. Given that particles settle when they are airborne (even if less than expected), the actual PSD at emission has to have been coarser than that observed at 1 km. It is still possible technically and no issue to apply this observed PSD at 1km to the emissions. However, in Fig. 5 / Section 3.2 / Section 3.4 / Discussion, the authors compare the modeled PSD at 1, 2, and 3 km height with the mean FENNEC PSD at 1km height and conclude that the model underestimates coarse dust, even when the settling velocity is reduced by 80 %. This is only natural as the FENNEC-PSD has been used at the PSD at emission, hence the model could only ever reproduce the observed PSD at 1km if all the emitted dust would be transported to 1 km in the model without any sedimentation. Otherwise the model has no

chance to do so. If this is the goal, then the PSD at emission would need to be described as coarser than the FENNEC-PSD, possibly by assuming a certain settling rate. In the context of the comparison between modeled PSDs and those observed in AER-D, I would like to see a specific comparison between the AER-D PSDs, the FENNEC-PSDs (this could in principle be seen from Figures in the paper, but a direct comparison would make this much easier): Are those PSDs, which have been measured above (FENNEC) and distant (AER-D) to dust source regions "sufficiently" distinct (i.e. is the FENNEC-PSD, which has been used for the emission, "sufficiently" [whatever this means] coarser than the AER-D PSD), such that the model has a chance to reproduce the after-transport AER-D PSD? I believe this aspect is critical, because it might well be that settling is one, but not the only key problem, but that particle sizes at emission are considerably underestimated, even if using the FENNEC-PSD. Besides this general discussion, I would like to ask how the part of the FENNEC-PSD, that extends beyond 100 microns has been dealt with when distributing the emissions. Was this fraction ignored and the remaining PSD re-normalized?

I understand that the sensitivity experiments on settling have been performed to mimic the effects of other processes. This is particularly applicable to effects of particle asphericity. However, the effects of other processes mentioned in the introduction, e.g. turbulence or vertical mixing in the Saharan Air Layer, are most likely much less homogeneous than settling and much more closely related to the meteorological conditions. I am not convinced that sensitivity experiments on the settling velocity are suitable to represent the effects of these processes. My recommendation is therefore to focus the manuscript on settling (which contains uncertainties as well, e.g. due to asphericity) and only discuss the other processes as possible additional contributors.

At several locations in the manuscript average PSDs or other quantities are discussed, but (some examples are mentioned below), but it was often not clear to me what averages those are (temporal, spatial, weighted?). I might have missed it, but I suggest to check this and clearly state how the shown and discussed quantities have been calculated.

L 14 Why is there a limit of dust particle sizes ($0.2 < D < 100 \text{ } \mu\text{m}$), in particular in the context of observations?

L 15 The formulation "extend the parameterization of mineral dust cycle" is not suitable. The parameterization of the mineral dust cycle (emission, transport [which includes itself several parameterizations], and deposition [again more than one parameterization]) was not extended, but some aspects of it were modified. The same applies for "our parameterization" (L 17).

L 21 - 22 Those additional processes have been proposed in the past, hence this statement is inaccurate. I suggest revising it and stating (after mentioning the sensitivity experiments) that those processes are discussed as candidates to cause such a reduced settling.

L 24 in the range

L 25 UR60 has not yet been introduced

L 30 Important to mention that dust only ranks first/second by mass.

L 32 Dust can be windblown, but I believe the emissions cannot.

L 34 Aren't all regions "spatially limited"? Perhaps use "Spatially more limited".

L 41 after their wet and dry deposition

L 46 I propose "cloud microphysical processes and their evolution" [omit the dissolution part] as I believe the processes do not stop.

L 51 Please give a reference for this diameter range. The lower limit seems relatively large to me.

L 65-66 gravitational settling

L 69 of all cases

L 70 Please give a (spatial) reference for "larger distances"

L 71 Stokes' theory is on settling, not on gravity.

L 112 The modified model version considers dust up to 100 microns, but airborne dust particles can also be larger, hence "the entire size range" is exaggerated.

L 131 Please add [in the default GOCART-AFWA] dust emission scheme [of (in) WRF]

L 152 Please introduce variables directly to Eqs. 3 and 4.

L 160 The Cunningham correction is missing in Eq. 5

L 172/176 I know the drag coefficient equation for the Stokes regime as $C_D = 24 / Re$ with $Re = U D / \nu$ with $\nu = \mu / \rho$. Is there any reason I am missing why the formulation shown here is different and contains the factor 2 in Re rather than C_D ? (The result is the same.)

L 178/192 The Kelvin scale has no degree symbol.

L 182 I don't think Equation 7 is meant here. Equation 4 maybe?

L 184 delete become

L 193 Eq. 13?

L 200 remove parenthesis around first reference

L 208 $Re < 1$?

L 218 Why did the authors choose to include so much ocean in their domain while omitting east N African dust sources? This seems not an ideal choice to me.

L 225-226 The authors state that "scaling of the dust source strength is chosen to best match the modeled DOD with the AERONET measurements". I would like to know more about this. What scaling do you refer to? Is this a universal scaling/tuning factor or a map scaling? Did you modify the Ginoux/GOCART erodibility function typically used in WRF or is a different scaling used? How has the modeled dust been compared with the observations to infer any kind of scaling? Please give more detail as this is an important aspect of the modeled dust fields.

L 228 A minimum DOD of 0.75 seems very high to me, even close to dust sources.

L 233 by up to 80 % with a step size...

L 234 "sensitivity experiment" instead of "artificial tuning"

L 235 Please revise "falling into the atmosphere"

L 236 "all real forces" is exaggeration. Gravitation and drag forces are real and considered.

L 240 What "fine resolution" are you referring to here? I would not consider 15 km a particularly fine resolution. Also, Table 3 does not contain any experiments on resolution (L 248).

L 243 Dunes are no meteorological condition.

L 270 The explanation is hard to understand, please revise it if possible. How did you handle missing values in the observations for the model comparison?

L 288 I suggest mentioning here again how the FENNEC PSD has been used. This will be as brief as mentioning that it is explained elsewhere (you can keep the reference to Sec. 2.1.1).

L 338/Fig. 5 Are the modeled PSDs for a particular time step or averaged?

L 360 I suggest showing deposition rates for bin 5 to see whether all particles have settled already over land.

L 363 – 375 The discussion about Flight b920 in the context of Fig. 7 is a bit confusing as Fig. 7 does not contain the PSD measured during the flight (but only the displaced dust plume). Why don't you show the PSD from b920 to provide a basis for the discussion?

L 393 The relative difference shown in Fig. 9b does not seem to vary systematically with height for bin 5. Shouldn't this be expected?

L 397 What kind of average is the "mean extinction coefficient"?

L 399-400 It has been discussed before how a few dust plumes were displaced, hence I do not agree with this general affirmation of simulation quality.

L 402 How can these mean (?) profiles be related to the night-time boundary layer? Was any more detailed analysis performed?

L 403-407 This discussion sounds like the observations are the main cause for model-observations discrepancies. I understand that this discussion is done to provide a justification why only Region II has been assessed. I suggest to revise the wording to avoid misinterpretation.

L 418 "acknowledged" is not the right word here, neither "transport code".

L 440 "(two times the particle major semi-axis)" seems out of place.

Discussion: I believe that much of the discussion around the different processes that might affect particle transport should go into the introduction. Only the discussion around the percentages in reduced settling these processes might account for should remain in the discussion.

L 482 losses instead of loses