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Comment on acp-2021-134

Anonymous Referee #2

Referee comment on "Less atmospheric radiative heating by dust due to the synergy of coarser size and aspherical shape" by Akinori Ito et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-134-RC2, 2021

Review of "Less atmospheric radiative heating due to aspherical dust with coarser size" by Ito et al for publication in ACP.

What is presented here is a series of sensitivity analyses based on a global model simulation of atmospheric dust. The radiative effect of dust aerosols is computed under a number of different configurations, testing on different dust refractive index assumptions in shortwave and long wave and different particle size and shape assumptions. Sensitivity of the resulting radiative forcing efficiencies is demonstrated. The main conclusion of the paper is well stated in the title and in the abstract, introduction, and conclusions. The best simulation results from an adjustment of the default simulation toward the 3d, seasonal, and size constrained dust distributions from the DustCOMM database, non-spherical dust, and the combined lesser shortwave absorption in Di Biagio (2019) and the long wave assumptions of refractive index from the older Volz et al (1983) database.

The paper is compact and short in text, but there is a tremendous amount of information contained in 11 tables and 14 figures. The supplementary material (especially the tables) is so necessary I suggest it just be folded into the paper proper. It is actually a confusing and frustrating paper to navigate in how it is presently laid out and I think much to the detriment of the work, which could be significant if the presentation can be cleared up. I found this a difficult paper to read through, partly in need too reference several different figures and tables to get a point out and partly in some confusing choices I think the authors make in how to set this up. It needs more work to clear some of this up. I don't know how one could possibly read this on a computer or tablet (you'd need multiple copies open to see all the bits) and even printing it out it was a hard slog to keep it all organized.

I am not convinced of the central conclusion. In particular, Figure 4 and Table S3 seem not to justify the "Improved" simulation as better than "Default" for surface SW forcing efficiency, maybe "Coarse-mineral" is the best agreement at the surface though clearly worse at TOA. The various scatter plots do not seem to much different in correlation or even RMSE (e.g., Figure S2). It's a little hard to untangle though how this optimized solution was arrived at.

Major comments

First, I suggest the distinction of "fine" and "coarse" in the simulation titles is a little confusing at the outset, as those terms are also used in the paper to refer to particular sections of the particle size distribution. "Fine-global" might be titled "Default-aspherical" or just "aspherical" which makes the perturbation clear. I suppose "Coarse" is then clear enough, or else "DustCOMM" to discriminate the size and loading adjustment. Anyway, lines 90 - 95 don't really map to table 1 well ("denoted as 'fine'" — well, only one experiment is actually denoted as "fine", and "denoted as 'coarse'" evidently includes "Improved" which is not denoted as "coarse"). Please clean this up.

Second, I'm confused about how many simulations were actually run with IMPACT. Since I take that asphericity is included in the dust gravitational settling in the Default run then is there only a single simulation run and then everything else is handled in offline calculations performed with different look up tables? Or section 2.5 seems to suggest some other simulation was done where DustCOMM was used to adjust the emission fluxes, but I can't really tell which or if indeed there are actually seven independent simulations. Presuming this is a non-radiatively interactive CTM (which use of MERRA-2 meteorology suggests) then maybe there are two simulations, the "Default" and one performed with an adjust at emission particle size distribution? Or otherwise I don't understand: in any of experiments 2, 4-7 are you just using DustCOMM directly for the calculations? How does the model get relaxed to the information from DustCOMM to simultaneously adjust the loading, 3D distribution, and particle size distribution if it does not actually just become the DustCOMM result?

A lot of the discussion of refractive indices seems disorganized, with something covered in 2.2 and some at the end of section 2.5. Could this be consolidated in a single subsection that goes over the refractive indices? You would also make a useful contribution if you included a figure in the paper that showed the spectral refractive index for your different choices. For the mineralogical map in Figure 1 and used in experiments 1 and 6 are you just applying different look up tables in different regions? You are not actually tracking mineral composition in the forward simulation, are you?

There are three different regional numbering schemes at use in the paper (Figure 1, Figure 3, and Table 4). Most challenging is differences in Figure 3 and Table 4, which they are similar in number and similar in sense though not geographic layout. The reader must hold several pieces of paper to follow all of this.

I don't follow the discussion of Figure 9. I *think* what is being shown is something in the sense of differences between simulations as in Table 2, but this isn't clearly the case to me. There are missing symbols in (a) and (b) which are maybe because some of the simulations are degenerate with others, hard to tell. It seems stated that the differences from the default are shown, but confusingly the default simulation is still shown and I don't know what the "Baseline" simulation is.

Minor comments:

Line 99: Is this a CTM (i.e., there is no radiative feedback of the dust on the simulation itself)? Please state that clearly here.

Line 104: Is this a bulk aerosol scheme, modal or something else? The dust appears to be treated in a sectional scheme.

Line 110: If the default scaling factor for IMPACT dust emissions is determined from observations why is the AOD so low? Is this because that is a constraint on the source regions but not downstream?

Line 126-128: Sentence beginning "Consequently..." does not make sense to me. What ensemble of model results are you referring to? Are you just saying that even adding this asymmetric correction to the IMPACT simulation it still doesn't give a good dust lifetime versus DustCOMM? Maybe you mean "Nevertheless" instead of "Consequently?"

Lines 137 - 146: I think you are describing here the construction of the "Mineralogical map" refractive indices in the shortwave. The referencing to Table 1 simulation is again confusing: "default simulations (denoted as'mineral')" — no, default is not denoted as "mineral" it is denoted as "default." Similarly "Default" is also using "global" LW optics. I wonder if some other coding schemes for experiments would just make this a little less twisty to follow.

Line 148: You refer to internally mixing dust with other components in the radiative forcing calculations. How are those other species partitioned across the four dust size bins? How does this assumption project onto the calculations you are doing with the non-spherical optics? I would think that internal mixing drives particles toward sphericity, but clearly you are doing some calculations with asymmetric dust treatment. Do the non-dust portions also get this non-spherical treatment? None of this clear.

Line 150: Adjustment factors are noted to account for the missing treatment of LW scattering. But it is not stated *what* is adjusted? The overall fluxes? Please clarify this.

Line 155: Again to the internal mixing: how is the dust radiative effect separated out from internally mixing particles? Is it the difference of (dust+rest) internal mix and dust only?

Line 170: “to bias adjust an ensemble”

Line 283: Reference I think is to Figure S2f.

Figure 3: Caption references a non-existent panel (d).

*(check not a printer issue...) Figure 3: I am having a hard time reconciling the coloration of the regions comparing (a) with (b) and (c) with the numerical values in Table S1. For example, region 4 (Mali/Niger) has a stated semi-observation-based AOD of 0.462 in Table S1 which ought to be a red color, but the region is plotted yellow. This confuses the discussion about the “goodness” of the simulations. Similar I think for regions 2 and 9.

Figures 4 - 8, S3, S5: The units for the radiative effect efficiency (stated at bottom of each figure) should. properly be written (W m-2 AOD-1)

Figure 4: The SW TOA side is a bit misleading as the color bar ends at zero on the right, but from the tables clearly the values go to positive numbers. Might consider expanding color bar.

Figure 6 - 8: The word “Atmosphere” is misspelled in panel (c) in each.

Figure 9: Where is the “+” symbol in (a)? Where is the orange star symbol (Improved simulation) in (b)?

Tables S1, S2: Please add a leading column for the region number so the reader can associate the location described with the map.

Table S3: third line, should “69” be “-69”?