

Atmos. Chem. Phys. Discuss., referee comment RC1
<https://doi.org/10.5194/acp-2021-134-RC1>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on acp-2021-134

Anonymous Referee #1

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-RC1>, 2021

The present work aims at investigating the direct radiative effect of mineral dust aerosols and in particular it focuses on the effect of dust asphericity. This analysis takes advantage of a strong background of work performed in past years by the authors of the manuscript (work on size distribution and inclusion of non-spherical spectral optical properties calculations, Kok et al. 2017 Nat geosci ; DustCOMM dataset creation, Adebisi et al., ACP, 2020 ; study of the asphericity of dust and inclusion of the effect on reducing gravitational settling, Huang et al., GRL, 2020) to realise new model simulations with the IMPACT model coupled with the RRTMG radiation code. The IMPACT model has been improved in this study compared to its default configuration by accounting mainly of : a better soil moisture dataset from satellite observations to improve dust emissions, to include the effect of asphericity on gravitational settling velocity, by integrating with the RRTMG radiative code, and by using the DUSTCOMM dataset to constrain simulations. The paper provides a series of sensitivity simulations varying mainly the size distribution and the spectral optical properties of mineral dust aerosols and assuming or not the effect of asphericity in the simulations. The results are compared to field observations of the dust DREE (direct radiative effect efficiency, $Wm^{-2} AOD^{-1}$) to identify the simulation configuration that best reproduces field measured dust perturbations at the surface and at TOA. The main conclusion of the paper is that improving the simulation scheme (improved vs default simulations) does not significantly changes the TOA global annual net DRE, conversely both the surface cooling and the atmospheric heating are strongly reduced assuming coarse aspherical dusts.

The paper is potentially a nice contribution for the scientific community. It provides interesting insight on dust aerosol science and contributes to further advance in the modeling of the dust cycle and its direct effects. Despite, I find that the paper suffers from an unclear identification of the objectives and a poor contextualization compared to the recent literature. As well, the presentation of the modelling simulations and the description and discussion of the results should be improved. In the current form I find the paper a bit difficult to read and I suggest that some major revisions are applied for improving the organisation and the presentation/discussion of the results. I compiled

some comments below.

General comments

- One of my problem, especially at the very first reading, was to understand the main objective of the paper. I had the feeling by reading the title that asphericity was the main topic, then in the introduction it is discussed that asphericity is not so important (see lines 65 to 74) and then the paper introduces many simulations testing the dust DRE sensitivity and many test studies. The overall introduction and description of the work should be more incisive and clear in the objectives and scientific questions to test.
- Following the previous comment, I have also found a little bit tricky to follow/understand the scope of the many simulations performed despite the synthesis effort in Table 1 and 2. There are many things and concepts in these simulations and a non-expert reader could be lost. Probably be clearer in describing the strategy ?
- The paper refers too much to the supporting information, in particular in the Results section. While on one side I appreciate the effort of synthesis, I feel that it is quite difficult to follow the reasoning when being obliged to go back and forth from the main paper to SI. I suggest the authors to consider revising their strategy of presentation of the results in order to help the reader following the reasoning.
- Referring to lines 51-53 « On the other hand, model errors due to the underestimated coarse dust load and corresponding warming might be compensated for in models by using a refractive index that is too absorbing (Di Biagio et al., 2019), and which depends on the mineral composition of the dust ». Isn't it the same case here based on your results ? Here the size distribution is cut at 20µm despite field evidences that larger particles are efficiently retained during transport (see FENNEC or SALTRACE results) and the best agreement with observations is found then when a stronger absorption is assumed in particular in the LW range, where the contribution of the coarse dust component is more critical. Is this result just due to the missing coarse size in the model above 20 µm? I would expect such a discussion in the paper (I noticed a mention to this at the very end of the conclusions, but the issue is argued to be related only to « Godzilla » type events and not relevant elsewhere. Is this really true or this aspect deserve more discussion ?)

Specific comments

Abstract : the concept of default and improved simulation si given, but I am not sure it is fully clear at this stage. I would remove this nomenclature from the abstract text

Line 23 : please specify the temporal/spatial scale of these estimated effects (global annual I guess)

Line 27 : I would say « warming of the Earth-atmosphere system by trapping incident and outgoing radiation »

Line 27 : what do you mean with « climate feedback ». Is this the correct term here ?

Lines 54-64 : probably a word here also on the minerals affecting LW absorption would be good

Lines 65-74 : I have to admit that I am quite confused here. The scope of the paper is to include asphericity effects but here I understand that it is not a big issue for global modelling. Please be more clear and focused on the objective and contours of the work.

Lines 80-84 : it is quite unusual to draw the conclusions or the results in the introduction of the paper

Line 91 : what is the forward model referred here ?

Lines 104-112 : could be possible to give a bit more information or reference to the model capacity in reproducing dust mineralogy reasonably ?

Lines 123-128 : even by accounting for an « optimized » asphericity factor for dust in the model the lifetime of the coarsest bin is too low compared to pas literature. What is the impact of this on the results and overall sensitivity ? I guess this is a crucial point.

Line 134 : given that the focus of the paper is asphericity effects I am quite surprised to see that Mie theory is used here. However is then in section 2.4 that it is explained what it is done. Not sure the two things should not be merged together.

Section 2.3 : I have to say that it remains a bit unclear how the dustcomm database is used in practice until sect 2.4. This sect 2.3 is a bit confusing to me

Section 3.3 : should be taken in mind and reminded to the reader that the comparison in the LW range is more tricky than in the SW since there is a stronger dependence of the DRE on the vertical profile of dust, temperature and water vapour profiles therefore affecting the measured and modelled comparison

Section 4 and throughout the text : pay attention to refer to « spectral optical properties » when related to both the SW and LW spectra