Our responses to the comments from Reviewer 2 are included in the followings.

Reviewer #2

Comments:

The paper presents a model-based sensitivity analysis of various factors that affect the calculation of dust direct radiative forcing in both SW and LW parts of the spectrum. Some of the model simulations are partially constrained by prior results from a semi-observationally-based dust climatology, which in particular modifies the simulated fine- and coarse-mode apportioning of the dust, as well as the overall loading.

The paper is improved in its revision, and I remain convinced there is a kernel of something really useful here, but it is still confusing in its organization and layout with a disturbing number of typographical errors that need to be cleaned up. I cannot recommend the publication of this paper at this point. It also seems to have lost its main thrust or conclusion, with no clear recommendation of a path forward evident. It seems squarely stuck in model sensitivity analysis land.

Response:

We would like to thank the reviewer for his or her constructive comments. The reviewer understands the simulations correctly that we did not use climate model (coupled IMPACT/CAM) but used CTM (coupled IMPACT/RRTMG) as in the initial submission for the revised manuscript. Accordingly, we did not change the main conclusion from the initial submission but revised the readability of our manuscript following suggestions made by the reviewers and the editor.

Main points

Comment 1:

Methods and modeling

I still don't understand how many different CTM simulations were performed.

Line 106 says two IMPACT simulations were performed "with the finer dust size", while line 112 refers to two CTM simulations with "IMPACT" and "DustCOMM" configurations. I suspect it is the latter. Elsewhere it is variously referring to eight experiments and five sensitivity experiments. I suspect actually there are two independent simulations (E1 and E2) that have different a priori presumptions of the dust particle size distribution and emission fluxes and that result in a database of three dimensional, time varying dust distributions (I justify this also by seeing only E1 and E2 report masses in Table 3). Further, I infer that both simulations actually account for dust asphericity in calculation of settling velocities and that the distinction between spherical and non-spherical dust is only in the a posteriori radiative fluxes and AOD. Please clarify.

Response:

The reviewer understands the simulations correctly. We stated chemical transport (IMPACT) model and radiative transfer (RRTMG) module simulations more clearly on p.4, l.104.

"We examined the dust radiative effects using ten combinations of different numerical experiments that varied (1) the simulated dust concentration and their size distribution, (2) particle shape, and (3) mineralogical composition (Tables 1 and 2). Two RRTMG calculations used the hourly averaged aerosol concentrations calculated from one IMPACT model simulation (E1 and E3) (denoted as "IMPACT"). The two sensitivity experiments were handled in the RRTMG calculations performed with the distinction between spherical and non-spherical dust and different refractive indices. We denoted "Sphere" when the RRTMG calculations used the spherical assumption on the particle shape, while the IMPACT model considered asphericity in calculation of gravitational settling velocities. On the other hand, we denoted "Asphere" when the dust asphericity was also considered in the RRTMG calculations."

Comment 2:

These are then used in the offline/separate radiative transfer calculator (line 128). I suspect the remaining 8 sensitivity experiments are using the distributions from E1 and E2 and just applying the different optical property assumptions. This needs to be clarified in the paper. (Also, line 128 states: "the radiative feedback of the dust on the climate model simulations can be predicted…" what climate model simulation? I think you just mean you have an offline tool for doing the radiative effect calculation. These are CTM experiments and there is no feedback on the simulated dust distributions. If I've got that wrong then I am thoroughly confused about what you actually did in this study. I think you are only diagnosing radiative effects, not looking at feedbacks.

Response:

The reviewer is right that the NCAR Community Atmosphere Model (CAM) was not used in this study. We revised the sentence to avoid the confusion on p.4, l.111 and p.5, l.135.

"Subsequently, the simulated dust concentration and the size distribution were adjusted to the semi-observationally-based concentrations (Adebiyi and Kok, 2020) in another chemical transport model simulation, which was performed with the five RRTMG calculations (E4, E5, E6, E8, and E9) (denoted as "DustCOMM"). The term "semi-observationally-based" is used for DustCOMM, $DAOD_{550}$, and dust radiative effect efficiency when the estimates are based on the combination of observations and models. We examined different refractive indices for the dust mineralogy to represent the regional variations in refractive indices (denoted as "Mineral", "DB17", "DB19", "V83", "Less SW", "More LW", "More SW", and "Less LW"). Thus, the other three experiments (E2, E7, and E10) were calculated from the model output with a post-processor. DustCOMM-Asphere-DB19-V83 (E2) was obtained from combination of DustCOMM-Asphere-DB19-DB17 (E4) for SW and DustCOMM-Asphere-Mineral-V83 (E6) for LW. DustCOMM-Asphere-Less-More (E7) was obtained from combination of DustCOMM-Asphere-Less-Less (E8) for SW and DustCOMM-Asphere-More-More (E9) for LW. DustCOMM-Asphere-More-Less (E10) was obtained from combination of DustCOMM-Asphere-More-More (E9) for SW and DustCOMM-Asphere-Less-Less (E8) for LW."

"Thus, the radiative feedback of the dust aerosol on the climate was not considered in this study."

Comment 3:

For clarity I suggest you move line 142 and following text “Dust emissions were dynamically simulated…” to following line 132 “….their precursor gases.” This would clarify the model description.

Response:

This is done.

Comment 4:

Consistent usage of reference to Liu et al. 2015 throughout paper I think s/b Liu et al. 2005. Please correct or else provide citation.

Response:

This is corrected.

Comment 5:

There is further some inconsistency in description of optical properties. Five aerosol species simulated are externally mixed for computation of spectral optical properties (line 141) but are later said to be internally mixed (line 182). I’m also confused about pulling out the dust-only signal on the computed fluxes. If the particles are internally mixed I would expect the radiative effect of a coated particle to not simply be the additive effect of uncoated dust + residual other particle contribution (line 185). Rather, would it make more sense to differentiate the dust-only properties (as the core) from the total coated particle? I think this is different than what is stated. In any case, if the point of the paper is to evaluate the dust radiative effect only (which it seems to be) then why make this distinction at all since everything appears to be calculated offline. I’m frustrated I still don’t seem to understand what is being done in this paper.

Response:

This is mainly because the SW scattering effect of coating materials on aerosol cores is considered. The aerosol core species are externally mixed with each other, whereas each core is internally mixed with coating materials on each aerosol. Thus, the coated dust is externally mixed with the other coated aerosol core species in the RRTMG calculations. We did not change this treatment from previous studies (Xu and Penner, 2012), but revised the description only. The sentence is revised on p.7, l.188 and p.7, l.191.

“These coating materials on aerosol cores were treated as internally mixed with each aerosol core in each size bin. Thus, the coating materials on dust only can reduce solar absorption of mineral dust.”

“The dust RE was estimated as the difference in the calculated radiative fluxes with all aerosols and with all aerosols except the dust coated with sulfate, nitrate, ammonium, and water for each bin.”

Comment 6:

Line 247: should refer to “Aspherical” for consistency with Table 1.

Response:

Table 1 is corrected for consistency.

Comments on Results section

Comment 7:

Figure 1: the (b) and (d) figures render in pretty poor quality. There is no explanation in the caption for the presence of (b) and (d) and generally none of the parts are referred to adequately in the caption.

Response:

Fig.1 and its caption are revised.

“Imaginary part of the refractive index at (a) 0.52 µm, (b) SW, (c) 9.7 µm, and (d) LW.”

Comment 8:

Figure 2: the lengthy caption could be greatly reduced as the regional boundaries are all present in Table S1. Also the caption and Table S1 refer to the regions as “A1” etc. but in the (a) panel they are labeled as “S1” etc.

Response:

Fig.2 and its caption are revised.

“The coordinates and the values of $DAOD_{550}$ at the 15 regions (marked in Fig. 2a) in summer were listed in Table S1.”

Comment 9:

Figure 4: the caption does not describe the figure as presented. There are panels (a) through (j) and the captioning breaks down by the time you get to the description of panel (g). Also could label shown regions as in Figure 2 with reference to the regions in tables S2-S6

Response:

This is revised.

Comment 10:

Figure 5: same comment as for Figure 4

Response:

This is revised.

Comment 11:

Figure 10: the clarity of this figure would be enhanced if in addition to the legend of symbols you wrote explicitly the experiments being differenced as in Table 2. I.e., the black hexagon could state next to it (E3-E4)

Response:

This is revised.

Comment 12:

Line 320: this is unclear or incorrect. As presented in Figure 2 and Table 3 I expect that E2 has > dust AOD than E1. What's written seems to say opposite.

Response:

This sentence and Fig. 2(c) are revised and combined with the sentence to represent the higher $DAOD_{550}$ from E2 than E1 (E2 – E1) on p.12, l.341.

"We also found higher $DAOD_{550}$ from E2 than E1 over East Asia and Bodele/Sudan in winter (Fig. 2, Table S2)."

Comment 13:

Line 335: The statement about comparisons for other regions does not appear to refer to Figure 4 actually. No idea what is meant here.

Response:

This is deleted.

Comment 14:

Line 385: Why is E4 going forward the baseline simulation to compare against? Not sure it's a wrong choice, but it just seems out of context when E2 seems the preferred simulation.

Response:

We thank both the reviewers for this insightful comment. We choose E4 as a reference case in figure 10 to show the model sensitivity to LW RI used in other studies in Fig. 9, as was stated on p.11, l.311 and suggested by the reviewer #1 (https://doi.org/10.5194/acp-2021-134-RC1 and https://doi.org/10.5194/acp-2021-134-AC1). Specifically, both Di Biagio et al. (2020) and Balkanski et al. (2021) used the 'DB17' and considered dust with diameters in excess of 20 µm. We revised the sentences to clarify it on p.14, l.393, p.14, l.401, and p.14, l.429:

"To elucidate the differences in dust radiative effects between the IMPACT-Sphere-Mineral-V83 (E1) and DustCOMM-Asphere-DB19-V83 (E2) simulations and to explore the variability in different previous model estimates (Fig. 9), the differences in annually averaged radiative effects of mineral dust from DustCOMM-Asphere-DB19-DB17 (E4) simulation were shown in Fig. 10."

"This revision can be divided into (1) the size-resolved abundance (black hexagons, E3 – E4, in Fig. 10), (2) SW refractive index (red diamonds, E6 – E4, in Fig. 10), and (3) particle shape (red circles, E5– E4, in Fig. 10). Additionally, we show the sensitivity of dust RE to LW refractive index (DB17), which was used by both Di Biagio et al. (2020) and Balkanski et al. (2021)."

"At the same time, both Di Biagio et al. (2020) and Balkanski et al. (2021) used DB17 and considered dust with diameters more than 20 μm. Thus, the more absorptive LW dust refractive index (V83, E6 for LW: 1.00 W·m^{-2}) than DB17 (E4 for LW: 0.58 W·m^{-2}) (E6 – E4 for LW: 0.42 W·m^{-2} at the horizontal axis of red diamond in Fig. 10b) could also contribute to the less surface cooling, which might be partially compensated for in our model by the omission of dust with diameters more than 20 μm."

Comment 15:

Line 393: Statement here is also unclear. I think E6 is mineralogical map referred to here which I think is more absorbing than E4, not less.

Response:

The reviewer is right that E6 is more absorbing than E4, but the positive (negative) value means less (more) cooling. To avoid the confusion, the sentence is revised on p.14, l.407.

"Second, this less SW cooling effect with coarser dust (E3 – E4) was partially compensated for by more SW cooling with the use of the less absorptive SW refractive index (E4: –0.32 W·m^{-2}) than E6 (0.02 W·m^{-2})."

Comment 16:

Line 442: Where are these numbers from? They don't appear in Table 5.

Response:

The numbers obtained from Balkanski et al. (in review) were updated to the final publication (Balkanski et al., 2021) in the revised paper.

Comment 17:

Line 447: Abstract refers to -0.60 W m-2 where here you say -0.88 W m-2. Abstract agrees with Table 5.

Response:

This is corrected.

References

Balkanski, Y., Bonnet, R., Boucher, O., Checa-Garcia, R., and Servonnat, J.: Dust induced atmospheric absorption improves tropical precipitations in climate models, Atmos. Chem. Phys. Discuss. [preprint], https://doi.org/10.5194/acp-2021-12, in review, 2021.

Balkanski, Y., Bonnet, R., Boucher, O., Checa-Garcia, R., and Servonnat, J.: Better representation of dust can improve climate models with too weak an African monsoon, Atmos. Chem. Phys., 21, 11423–11435, https://doi.org/10.5194/acp-21-11423-2021, 2021.

Di Biagio, C., Balkanski, Y., Albani, S., Boucher, O., and Formenti, P.: Direct radiative effect by mineral dust aerosols constrained by new microphysical and spectral optical data. Geophys. Res. Lett., 47, e2019GL086186, https://doi.org/10.1029/2019GL086186, 2020.

Xu, L. and Penner, J. E.: Global simulations of nitrate and ammonium aerosols and their radiative effects, Atmos. Chem. Phys., 12, 9479–9504, https://doi.org/10.5194/acp-12-9479-2012, 2012.

Our responses to the comments from Reviewer 3 are included in the followings.

Reviewer #3

Comments:

This study uses the observation-based size-resolved dust concentration, asphericity factor and spectral refractive to improve the simulated dust radiative effect at TOA, surface and in the atmosphere. The adjustment improves the agreement of simulated globally and annually averaged DAOD with the semi-observational based estimate. Several experiments are implemented to investigate the sensitivity of dust radiative effect to dust size, shape, and refractive index. This study finds that a less absorptive SW dust refractive index (RI) and more absorptive LW RI are required for coarse aspherical dust to achieve a better agreement against a semi-observational-based radiative effect efficiency at TOA. The combination of coarse aspherical dust with less absorptive SW refractive index induces a less heating effect in the atmosphere. Overall, this study is interesting and comprehensive, it provides new insights to the research community. I have several minor comments listed below.

Response:

We would like to thank the reviewer for his or her constructive comments which helped us to improve the readability of our manuscript substantially. We revised the manuscript following suggestions made by the reviewer.

Comment 1:

1. Line220: In this section, I think it would be helpful to add some equations to show how exactly the emission flux and emitted dust mass fraction are scaled.

Response:

Three equations are added for the bias correction factor (Eq. 1), mass fraction for each size bin (Eq. 2), and dust emission flux after the adjustment (Eq. 3).

Comment 2:

2. Line267: Should ‘DB17’ be ‘DB19’? Please correct

Response:

This is corrected.

Comment 3:

3. Line272: Should ‘DB19’ be ‘DB17’? Please correct

Response:

This is corrected.

Comment 4:

4. Line320: In 'The lower DAOD550 from E2 to E1', lower should be higher, right?

Response:

This sentence and Fig. 2(c) are revised and combined with the sentence to represent the higher $DAOD_{550}$ from E2 than E1 (E2 – E1) on p.12, l.341.

"We also found higher $DAOD_{550}$ from E2 than E1 over East Asia and Bodele/Sudan in winter (Fig. 2, Table S2)."

Comment 5:

5. Line376: Please change 'V83 simulation' to 'IMPACT-Sphere-Mineral-V83' to be specific, 'V83 simulation' is confusing, since both E1 and E2 use V83.

Response:

This is changed.

Comment 6:

6. Section 3.5. Figure 9 shows the comparison between E2 and E1, the difference between E2 and E1 could be divided into 3 aspects: dust size, shape, and RI (SW-RI). Then each aspect is investigated in figure 10. However, I wonder why to choose E4 as a reference case in figure 10? We could see that LW-RI (volz83) is the same for E2 and E1, there is not an experiment (or marker) in figure 10 could directly illustrate the contribution of RI difference (only SW-RI is different in this case) to the radiative effects difference between E1 and E2. Would it be better if change the reference case to be E2? Then set up three experiments to change size, shape, SW-RI respectively?

Response:

We thank both the reviewers for this insightful comment. We choose E4 as a reference case in figure 10 to show the model sensitivity to LW RI used in other studies in Fig. 9, as was stated on p.11, l.311 and suggested by the reviewer #1 (https://doi.org/10.5194/acp-2021-134-RC1 and https://doi.org/10.5194/acp-2021-134-AC1). Specifically, both Di Biagio et al. (2020) and Balkanski et al. (2021) used the 'DB17' and considered dust with diameters in excess of 20 µm. We revised the sentences to clarify it on p.14, l.393, p.14, l.401, and p.14, l.429:

"To elucidate the differences in dust radiative effects between the IMPACT-Sphere-Mineral-V83 (E1) and DustCOMM-Asphere-DB19-V83 (E2) simulations and to explore the variability in different previous model estimates (Fig. 9), the differences in annually averaged radiative effects of mineral dust from DustCOMM-Asphere-DB19-DB17 (E4) simulation were shown in Fig. 10."

"This revision can be divided into (1) the size-resolved abundance (black hexagons, E3 – E4, in Fig. 10), (2) SW refractive index (red diamonds, E6 – E4, in Fig. 10), and (3) particle shape (red circles, E5– E4, in Fig. 10). Additionally, we show the sensitivity of dust RE to LW refractive index (DB17), which was used by both Di Biagio et al. (2020) and Balkanski et al. (2021)."

"At the same time, both Di Biagio et al. (2020) and Balkanski et al. (2021) used DB17 and considered dust with diameters more than 20 μm. Thus, the omission of dust with diameters more than 20 μm could also contribute to the less surface warming in our model, which might be partially compensated for with the use of the more absorptive LW dust refractive index V83 (E6: 1.00 $W \cdot m^{-2}$) than DB17 (E4: 0.58 $W \cdot m^{-2}$) (E6 – E4: 0.42 $W \cdot m^{-2}$ at the horizontal axis of red diamond in Fig. 10b)."

Comment 7:

7. The caption of Figure 4 and Figure 5 is not consistent with the figure, please correct.

Response:

This is corrected.

Comment 8:

8. I think the 'Sphericity' in Figure 10 is the 'Aspherical shape' experiment (E5-E4) in table2. It would be easier to understand if the names are consistent. In addition, in figure10, it would be clearer if add the experiment number (from Table 2) for each marker.

Response:

This is done.

References

Balkanski, Y., Bonnet, R., Boucher, O., Checa-Garcia, R., and Servonnat, J.: Better representation of dust can improve climate models with too weak an African monsoon, Atmos. Chem. Phys., 21, 11423–11435, https://doi.org/10.5194/acp-21-11423-2021, 2021.

Di Biagio, C., Balkanski, Y., Albani, S., Boucher, O., and Formenti, P.: Direct radiative effect by mineral dust aerosols constrained by new microphysical and spectral optical data. Geophys. Res. Lett., 47, e2019GL086186, https://doi.org/10.1029/2019GL086186, 2020.

Our response to the comment from Reviewer 4 is included in the following.

Reviewer #4

Comments:

The authors did a great work in revising the manuscript. I have no further comments.

Response:

We would like to thank the reviewer for his or her constructive comments.