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Comment on acp-2020-1113

Anonymous Referee #1

Referee comment on "Estimating radiative forcing efficiency of dust aerosol based on direct satellite observations: case studies over the Sahara and Taklimakan Desert" by Lin Tian et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-1113-RC1>, 2021

This paper determines the TOA direct radiative effect (DRE) and the so-called direct radiative forcing efficiency (DRFE) of dust for two dust storms by using CERES SSF data. One of the dust storms occurs near an AERONET site in Tamanrasset (Africa) and the other occurs near Kashi, India. The authors note that the mineralogy of dust is different for these two regions. The authors would like to study the impact of mineralogy on the dust radiative effect, so they constrain their analysis to a single land surface albedo (LSA, from MODIS) and a single solar zenith angle (SZA). Since the authors are looking at multiple locations with the same surface albedos, they call this the equi-albedo method. The authors also focus on DRFE instead of DRE in order to eliminate the effect of column loading. Since the authors have constrained most of the parameters that affect TOA DRFE, they attribute DRFE differences between the two storms to differences in dust mineralogy. Thus, the authors investigate further by analyzing the differences in microphysical dust properties inferred at the AERONET sites during the dust storms. This is an interesting idea, but the analysis is not terribly convincing.

The paper is telling a pretty reasonable story until the arrival of Figure 7. Here, the data of the earlier maps is reduced to a few 1x1 deg areas. Undaunted, the authors discuss how "The high dust aerosol loading regions show significant negative radiative forcing..." (lines 267-268). I guess that the reader is supposed to scroll between Figs 5 & 7 to confirm this, but requiring a reader to scroll between two figures does not generally convince anyone of anything.

The frustrating part is that there is no need for the maps in Fig 7 to be so sparse -- the data is available. Now, I realize that the authors want to focus on 1x1 regions that are constrained by LSA and SZA, but there are other ways of dealing with this. For instance, one can include data for the entire maps in Fig 7, and then put a black border around the few 1x1 grids of interest. This will allow the authors to discuss the whole map (which they often do for these sparse maps) as well as the regions of interest. Furthermore, include

the same borders in Figs 3,4,5,6. This will help the reader to understand the cloud fields, AOD, and TOA SW radiative flux in the 1x1 regions of interest. Since these regions of interest are only constrained by surface albedo and SZA, they can be easily introduced in Fig 3. If you do it this way, this will allow the reader to see the patterns of DRE in Fig 7 and make the paper a whole lot more interesting. Same thing with Figs 9&10 -- show the IWV and SBDART fluxes everywhere, but outline the 1x1 regions of interest with borders. Finally, include the Tamaarasset and Kashi sites in all of the maps (including figures 4, 6, 7, 9, and 10). The authors are trying to link surface measurements at these two sites to the dust storms observed in the satellite data, and the link is very weak because they do not show the location of these sites on many of these maps.

Is it also very odd that the 1x1 regions with data vary from day to day. Perhaps more odd, sometimes some of the boxes vary, but others do not. Since these regions are selected based upon LSA and SZA constraints, why don't the same regions show up on Mar 9, 11, and 14 at Tamaarasset and on Apr 9, 23, and 25 at Kashi? I haven't kept up on the MODIS albedo products, but it used to be produced every 2 weeks. Thus, if LSA is the same, the only parameter that will move these boxes around is the SZA. An explanation about why the SZA apparently varies so much on the different days would be helpful.

The other component of this paper is computing TOA DRE and DRFE from the microphysical properties at two AERONET sites. The purpose is to link the two techniques together (satellite and surface retrieval computations), but the link is weak since the reader does not even know if the AERONET sites are located within any of the regions with data in Figs 7,9, and 10, or how DRFE varies across the maps. It would have been interesting to see a map of DRFE for the entire maps.

Putting collocation aside, the details of the microphysical calculations are missing. The authors use SBDART for broadband computations, but they only have optical properties at four wavelengths. How do they extrapolate the AERONET refractive indices throughout the SW?

The methodology is sprinkled throughout the paper, and is sometimes inconsistent. For instance, the authors state that SSA and ASY are calculated using spherical and non-spherical methods -- how? Do the authors do this, or does SBDART take care of this? On lines 240-242 the authors say that AERONET computes SSA and ASY. Later (on line 153) they say that the "NASA-GISS code is used to calculate the optical properties of the spherical particles and the ellipsoidal particles." On line 375 we're back to AERONET.

The authors do not discuss details of their datasets. For instance, do they use AERONET Version 2 or Version 3? Level 1.5 or Level 2? Version 2 is no longer available, but the authors may be using previously-downloaded data. Also, the Version 3 retrievals at Tamaarasset never made it to Level 2, indicating that the data did not make it through the new cloud screening process. This is important because cloud contamination could easily confound their conclusions about this part of the paper.

The authors seem to have some misconceptions about SSA. On line 382, they state: "A high SSA is correlated with low real parts of the complex refractive index, while a strong absorption is correlated with a high imaginary part of the complex refractive index. Together with the size distribution, real parts of the complex refractive index can determine the magnitude of the SSA." <--- This is incorrect; the SSA is largely determined by the *imaginary* refractive index and the size distribution.

Story does not flow and jumps around. The writing is pretty sloppy, as evidenced by the long list of issues below.

MAJORS

Line 154: Why is the particle aspect ratio set to 0.8? Is this based upon a literature value? Need to tell the reader.

Line 193: There is no Version 3 Level 2 data in March at the Tamanrasset site. So it is possible (probable?) that this data is contaminated by clouds. This could contribute significantly to the retrieval differences between the two sites. An explanation is necessary.

Line 254: "The TOA SW radiative flux distribution shows the highest value over cloud conditions" ...How can I tell this from Fig 6?... Tell the reader that they can find the clouds in Fig 3. Better yet, design the paper so that you can combine these flux figures with the Fig 3 images. So, one figure would contain the left panels of Figs 3 & 6 and another figure would contain the right panels of Figs 3 & 6. That way the cloud images are side by side with the flux figures.

Line 256: Sentence unclear.

Line 257: "Thus, dust aerosols have a negative radiative effect in the SW spectrum." ...Here again -- how do I get this from Fig 6, where all numbers are positive? If you want to discuss radiative effect, why not show radiative effect in the figure? ...I see you have rad effect in Fig 7. Why not delay this discussion until then?

Line 267: "The high dust aerosol loading regions show significant negative radiative forcing" ...a little difficult to conclude this with so sparse data in Fig 7.

Line 308: "The integrated water vapor varies little over research areas,..." -- here again, how can the reader know when you only show a few points in Fig 9? Surely ECMWF provides more H₂O_v than this?

Lines 382-384: Incorrect.

Line 415: "As shown in Fig. 16, with higher aerosol scattering (higher SSA) and higher backward scattering coefficients (lower ASY), the negative DRF_{dust} from Kashi is more significant." <-- I don't see how this follows from Fig 16, as SSA is not even mentioned in the Figure.

MINORS

Throughout the paper the authors assume that all aerosols are dust, both for the satellite data set and for the surface measurements. That's ok, but it needs to be stated.

Line 45: Anderson (2005) does seem to claim ownership of this idea, but forcing efficiency dates back to at least Satheesh (2000) papers. There are many others, but at least Satheesh (2000) needs to be added.

Line 193: This is confusing. The authors provide two dates here, but figs 3&4 provide 3 dates for each site. They have already covered Fig 2 in the previous paragraph -- why are

these first two sentences even located in this paragraph?? This should really be a lead-in for the previous paragraph.

Line 220: Retried?

Line 236: need to cite original AERONET paper; Holben et al, 1998.

Line 281: "According to the definition, the DRFE_{dust} represents the DRE_{dust} of 281 a certain AOD at per unit area..." -- not just any AOD, but at AOD = 1, right? Should state that.

Line 285: AOD wavelength should be mentioned in caption.

Line 289: Two numbers, one location.

Line 285: Is the regression forced through the origin? If not, state the offset so we know. Early papers often had an offset.

Line 311: Unclear sentence.

Line 324: This figure would be more intuitive if the independent variable was put on the x-axis.

Line 327: "...which is little than CERES observation errors." <----- nonsensical

Lines 330-332: again, nonsensical.

Line 349: why the name change from tamanrasset and kashi to Sahara and Takliman?

Line 349: why is Kashi associated with the Sahara desert in panel d?

Line 359: "The volume size distribution of dust aerosols clearly shows the particle size difference between dusty and clear-sky days." Authors need to point to a figure when making a statement like this. can not conclude this from the most recent figure (fig 12).

Line 359-362: these first 3 sentences make no sense b/c you have not told the reader what you are talking about!

Line 365: units

Lines 359-367: What is the point of this paragraph?

Figure 6: Tell us in the caption that this is CERES. Ideally, include some details about the particular CERES product. Most folks won't read more than the captions in your paper.

Figure 10: Again -- why not the whole map?

Fig 11: independent variable should be on x-axis.

Satheesh, S., and V. Ramanathan (2000), Large differences in tropical aerosol forcing at the top of the atmosphere and Earth's surface, *Nature*, 405, 60–63.