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Interactive comment

Interactive comment on "Toward an Observation-Based Estimate of Dust Net Radiative Effects in Tropical North Atlantic Through Integrating Satellite Observations and In Situ Measurements of Dust Properties" *by* Qianqian Song et al.

Anonymous Referee #2

Received and published: 9 April 2018

General comments

This is an interesting and useful paper providing some new constraints on dust radiative effects over the North Atlantic. The paper demonstrates a novel method using CCCM satellite products to estimate direct radiative effects of dust in both the SW and LW spectrum, for clear-skies over ocean. The dust radiative efficiency estimates so derived are also used to test a range of datasets for dust particle size distribution, shape and

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Discussion paper

refractive index. The paper assesses which combination of dust properties best match the satellite-observed DRE efficiencies.

The results shows the best agreement to both SW and LW radiative efficiency when using a relatively coarse particle size distribution based on the Fennec aircraft measurements, rather than size distributions based on AERONET retrievals. This adds growing support to the conclusion that dust particle sizes are underestimated in many current dust models and remote sensing retrievals (especially those based only on SW measurements). The study also finds it best to combine this PSD with a more up to date (less absorptive) refractive index dataset for the SW spectrum and either of two spheroid shape models. These are useful conclusions as the sensitivity tests include a range of recent in-situ and remote sensing measurements, along with older datasets that have been used in dust simulation models and remote sensing retrievals. The paper is well written and relatively easy to follow with only a few issues to address. I therefore recommend publication in ACP after minor corrections.

Specific comments

1. The title is rather long. It could be shortened for clarity, e.g.: "Radiative effects of dust in tropical north Atlantic from integrating satellite observations and in situ measurements of dust properties"

2. The abstract is also too long and carries to many details. I recommend trimming this down and reporting only the most crucial numerical estimates; there are currently 12 DRE or DRE efficiency estimates, which is too much to digest.

3. The introduction is very good, covering the issues very well but it would be good to add a sentence or two after line 65 explaining why it is important to quantify dust DRE as accurately as possible. For instance, dust radiative effects have an influence on global and regional climates, and changes in dust DREs play a role in anthropogenic climate change and climate feedbacks.

4. The manuscript uses the original terminology for aerosol effects (“direct radiative effect”, “indirect effect”, “semi-direct effect”). Although most readers will know what these mean it would be a good to refer to the new terminology, following IPCC AR5 conventions: “instantaneous radiative effect” for the direct effect, “aerosol-radiation interactions” for direct + semi-direct effects, and “aerosol-cloud interactions” for indirect effects (or refer to both if necessary). Please also make it clear in the methods if the DREs calculated in this study are indeed equivalent to the "instantaneous radiative effect".

5. In many places the sign of radiative effects are indicated by describing them as “cooling” or “warming” effects (e.g. Lines 81, 88, and many other places). Whilst it can be helpful to indicate the likely cooling or warming impact this way of explaining the sign can be mis-leading or even nonsensical. For instance, what does it mean to “yield a cooling effect at TOA”; there is no air at the TOA. Please give the sign of radiative changes explicitly in the text. The likely cooling/warming tendency can be given in addition, if desired, e.g. line 81 could read “. . .this leads to a negative DRE at the TOA that is likely to cool the climate system”. The same argument applies when expressing DREs at the surface; please state the sign rather than indicating this via the expected temperature change. Sometimes absorbing aerosols can reduce net radiation at the surface (yielding a negative DRE) yet cause surface temperatures to rise if the surface and absorbing aerosol layer are thermally coupled. When describing changes in OLR it is also not completely clear to say the OLR is “colder” or “warmer” (e.g. Lines 468 – 473, 514-515). Reducing OLR makes the planet look cooler from space but generally leads to a warming of the climate system. Please simply state if OLR is increased or reduced, or indicate the sign of the radiative effect.

6. Line 104. I do not agree that observations of dust PSD are “scarce”. There have been many measurement campaigns and long-term remote sensing observations during the past two decades. The problem is that dust PSDs are so variable and difficult to measure or retrieve so broader sampling and more accurate measurements are still

needed. Please could the text be clarified accordingly. Also, it would be good to add some further references here to indicate the breadth of measurements that are available, or if Mahowald et al. gives a good summary of these then the citation could be changed to ("see Mahowald et al., 2014 and references therein").

7. I am slightly surprised that the CERES-CALIOP and CERES-MODIS DRESW forcing efficiency estimate are so different, even when they are taken from the same subset of 153 pixels. How reliable are AODs from CALIPSO compared to those from MODIS? There is certainly a lot more scatter in the CALIPSO AODs and poorer regression against CERES SW flux. Are there problems detecting dust when loadings are low, does the CALIPSO retrieval fail to capture larger AODs > 1 due to saturation? These issues could potentially lead to biases in the inferred DRESW efficiency? Of course, MODIS is not perfect either. Could the authors comment on the relative accuracy or reliability of the CALIOP and MODIS AOD retrievals and any likely impacts / biases on the inferred DRESW efficiency estimates. It might be beyond the scope of the study to do a full evaluation, but to provide some comment is important.

8. A second point on the analysis of CERES SW fluxes. The dust AOD is surely not the only factor affecting the TOA SW flux. The main other factor would be solar zenith angle, but the sea state and marine BL aerosol loading could also be non-trivial factors. These factors may explain some of the scatter in Figure 5 but I think it is important to know if they could potentially bias the regression of SW flux against dust AOD. Can the authors demonstrate that these factors are not important? Otherwise I would recommend adding a comment to the text to caveat any potential biases or uncertainties that these factors may introduce when deriving the DRE efficiencies.

9. Line 192-195. I wasn't totally clear how the aerosol types were determined. Is the aerosol type information from MATCH a 2D or 3D field (i.e. is it resolved in the vertical)? Also are the MATCH simulations operational real-time forecasts or reanalysis? Further, when the dust type is set to dust how are the optical properties of dust determined? Are they specified based on an assumed PSD, refractive index and shape distribution,

or somehow constrained from the CALIOP retrievals.

10. Section 3.1. Could the low sampling rates (only 1.7% of CERES pixels are used) bias the results in any way? Dust properties and cloud cover (or lack of cloud) could well be related and co-vary as both are affected by the large-scale meteorological conditions. Would it be possible to check for any co-variation in the data?

11. Section 3.2. Why has the CERES-CALIOP DRE efficiency been calculated only from the 454 cases where MODIS is unavailable? Why not include all 607 pixels? Wouldn't this provide the "best" estimate from CERES-CALIOP. Limiting it to cases when MODIS was unavailable could introduce some sampling bias.

12. Line 319. The text isn't totally clear when it says "the other 454 cases. . . . . .are also included. . .". This might be read that all 607 cases were included but the caption for Figure 5c, states it includes only 454 cases. Please clarify.

13. The estimates of dust DRELW throughout the paper are given as "between 2.7+/- 0.32 to 3.4+/- 0.32 Wm-2". This is rather confusing. Does this mean that the DRELW estimate is between 2.38 – 3.72 Wm-2? The problem arises because no decision is made as to whether the 0.7Wm-2 discrepancy between CERES and RRTM should be subtracted from the DRELW estimate or not. It would be much clearer if this discrepancy was either: (i) treated as a bias and subtracted from the DRELW estimate, so that only the lower DRELW estimate was given, (ii) considered as a potential error and included when calculating the uncertainty range on the upper estimate.

14. Line 575. I am not familiar with the term "semi-observation-based". Has this terminology been used elsewhere in the literature? If not it might be better to define it or just explain what information was used, e.g. "we derive a set of DRELW estimates by comparing CERES observations with dust-free radiative transfer calculations from RRTM".

15. Line 589. It would be worth stating the years and months from which data was

included. The authors might also like to comment on the merit of extending the analysis to other season / regions / years in future studies.

Technical corrections

1. Line 61. It isn't necessary to draw the reader to Figure 1 at this point.

2. Line 90. Is there a reference to back up the statement that surface emissivity is an important factor in dust LW effects?

3. Line 273. It would be more useful to give the spectral bands in terms of wavelength intervals in units of microns (to be consistent with section 2.2).

4. Line 348. "In the analysis followed" probably means "In the following analysis".

5. Line 577. Please insert: "...we use the RRTM radiative transfer model".

6. Tables 4 and 5 could be combined.

7. The font size is too small on some of the tables.

8. Figure 5. The plots and their text are too small. Font sizes and/or overall figure size needs increasing.

9. Figure 8. The CERES and CCCM data need to be described in the figure caption. Also, please consider giving the CERES lines unique colours. On first glance one might assume that these are the linear regression lines for the blue and red squares.

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