Dear Dr. Chiu,

We would like to thank you for your effort on our paper. We'd also like to thank the three anonymous reviewers for their insightful and suggestive reviews, which have helped us improve the paper significantly.

Following the reviewers' comments, we have revised the manuscript significantly. Important changes include:

- We changed the title to "*Net Radiative Effects of Dust in Tropical North Atlantic Based on Integrated Satellite Observations and In Situ Measurements*"
- Several figures have been updated following the suggestions of the reviewers.
- A discussion on the representativeness of our results has been added to the end of the revised manuscript with a new Figure (Figure 11).

Below are our point-to-point replies to reviewers' comments.

Zhibo Zhang

Review 1

Q1. My main concern is that it is not clear how this study accounted for variation in the solar angle when calculating the DRE efficiencies. This is obviously a major factor affecting the outgoing SW fluxes, so the methodology for this should be clearly discussed.

Reply: Thanks for bringing up this point. The DRE efficiency is indeed dependent on SZA, which should be taken into account when estimating DRE efficiency when SZA has a significant variance.

However, in section 3, we only estimate the instantaneous DRE of dust in the selected region at the time of A-Train overpass which is about 1:30PM local time. Because the selected region is relatively small, the SZA at the A-Train overpass time in the domain only varies slightly among our selected cases, from 20 to 28 degree. We did some simple sensitivity test, in which we further divide the cases into two groups according the SZA value and we do not see significant differences in terms of DRE efficiency. Considering the limited sample size and the small SZA interval of selected cases, we therefore estimate DRE efficiency based on all the selected cases. Note in previous studies, such as Di Biagio et al. [2010], the DREE is compute for every 10 degree SZA interval.

Note that in Section 5, when computing the diurnally averaged DRE, we do consider the diurnal variation of SZA.

We added some discussion in the paper to clarify this.

Q2. The title is very long.
I'd recommending making it more concise to make it easier for readers to quickly comprehend what the study is about.
Reply: We change the title to "*Net Radiative Effects of Dust in Tropical North Atlantic Based on Integrated Satellite Observations and In Situ Measurements*" (Recommended by reviewer2).

Q3 It's a bit unclear to me why the authors did not use AOD retrievals from MISR, which have the advantage of also providing information the aerosol type?
Reply: In this paper, we use the CCCM product, which is a merged product of CERES, CALIPSO, CloudSat and MODIS from the A-Train satellite constellation. MISR is on board of Terra, not part of the A-Train. So, we didn't use its product.

Q4 I think the years over which the analysis is performed should be noted in the abstract for clarity.
Reply: We included from 2007 to 2010 in the abstract

Q5 The authors should explain their use of the term instantaneous DRE (first on line 163, I think).

Reply: The instantaneous dust DRE represents dust DRE derived under the conditions (e.g., solar position, atmospheric condition) at the measured/computed time to distinguish from the diurnally averaged DRE in section4.

Q6 Lines 334-335: Please be more specific here. Exactly which atmospheric profiles did you use? Ozone, water vapor, other greenhouse gases? Did this account for any fractional cloud cover of optically-thin clouds?

Reply: In the DRELW computations, we used the atmospheric profile and surface properties reported in the CCCM product, which are from the NASA GMAO GEOS system [Kato et al. 2011].

In this study, as explained in Section 3.1 we only select the cloud-free cases based on the cloud mask product from both CALIPSO and MODIS. The CALIOP lidar is very sensitive to thin clouds, which gives the confidence that the selected case should be free of optically thin clouds. Of course, the CALIOP lidar also has its detection limitation, but it is the best we can get at the moment.

Q7 The errors are alternately reported as 1 sigma and 2 sigma intervals. I recommend the authors choose one and keep this consistent to avoid confusion.

Reply: We consistently report DRE efficiency with 1-sigma error in this paper.

Q8: I understand and appreciate that you report both the MODIS and the CALIPSO-based estimates of the DRE and the DRE efficiency. However, it's clear that the MODIS estimate is likely to be more accurate. I think your paper would therefore have more impact if you combine these estimates into a single number, either by using error propagation to weigh each estimate proportional to the inverse squared of their error; this will weigh the estimate towards the lower-error MODIS-based estimate.

Reply: Thanks for the suggestion. It is easy to combine the two observation-based DREE_SW based on MODIS and CALIPSO observations to get an averaged value. However, in our opinion, this averaged value does not seem to have much physical meaning. Neither does it provide any additional insight into the uncertainty in the observations. So, we hope to keep our original estimate of the uncertainty range.

Q9 Line 378: Could you include the exact definition of the extinction efficiency here, which differs somewhat between different sources?
Is this the extinction cross section normalized by the projected surface area of the irregular dust particle, or normalized by the projected surface area of the volume-equivalent sphere? Additionally, please clarify how the extinction efficiency is actually calculated for the mixed size distributions of Fennec and AERONET.

Reply: Thanks for bringing up this point. Indeed, the computation of the bulk scattering properties of nonspherical dust is complicated, which is explained below.

First of all, as we mentioned in Section 2.2, we assume volume equivalent radius for the AERONET-PSD to be consistent with Dubovik et al. [2006] and the maximum dimension for Fennec-SAL PSD to be consistent with Ryder et al. [2013b].

Secondly, the single-scattering properties of spheroid dust particles are from the database described in Meng et al. [2010]. In the database, particles are assumed to be randomly oriented. For each spheroid particle with the volume V and aspect ratio ϵ, the database reports its single scattering properties, such as extinction efficiency (Qe), single scattering albedo (ω) and scattering phase matrix, as well as the maximum dimension of the particle and the projected area averaged over random orientations.

Ideally, the bulk scattering properties of nonspherical dust (i.e., spheroid in this study) should be computed by averaging the single scattering properties of dust properties over a **joint** probability density function $n(r,\epsilon)$ that takes into account of the distribution over both dust size and shape. For example, the bulk scattering extinction efficiency should be computed from the following equation:

$$< Q_e(\lambda) > \; = \; \frac{\int_0^\infty \int_0^\infty Qe(\lambda, r_X, \epsilon) \cdot A(r_X, \epsilon) \cdot n(r_X, \epsilon) \cdot d\epsilon \cdot dr_X}{\int_0^\infty \int_0^\infty A(r_X, \epsilon) \cdot n(r_X, \epsilon) \cdot d\epsilon \cdot dr_X},$$

where, r_X could be the volume- equivalent radius (i.e., $r_X = r_V$) in case of the AERONET-PSD or the radius corresponding to the maximum dimension ($r_X = D_{max}/2$) in case of the Fennec-SAL PSD; ϵ is the aspect ratio of spheroid particle; $A(r_X, \epsilon)$ is the averaged projected area of randomly-oriented spheroid particle with the dimension r_X and the aspect ratio ϵ, which can be obtained from the Meng et al. 2010 database.

However, there is no such joint PDF in the literature, probably because it is difficult to measure the size and shape at the same time.

The aspect ratio distribution from Dubovik et al. [2006] in Figure 4 a is size-independent. In other words, $n(r,\epsilon) = n(r)n(\epsilon)$ in this case. As such, the bulk scattering properties can be easily computed from

$$< Q_e(\lambda) > \; = \; \frac{\int_0^\infty \int_0^\infty Qe(\lambda, r_X, \epsilon) \cdot A(r_X, \epsilon) \cdot n(r_X) \cdot n(\epsilon) \cdot d\epsilon \cdot dr_X}{\int_0^\infty \int_0^\infty A(r_X, \epsilon) \cdot n(r_X) \cdot n(\epsilon) \cdot d\epsilon \cdot dr_X},$$

where $\int_0^\infty n(r_X) dr_X = 1$ and $\int_0^\infty n(\epsilon) d\epsilon = 1$ are the normalized PSD and shape distribution, respectively.

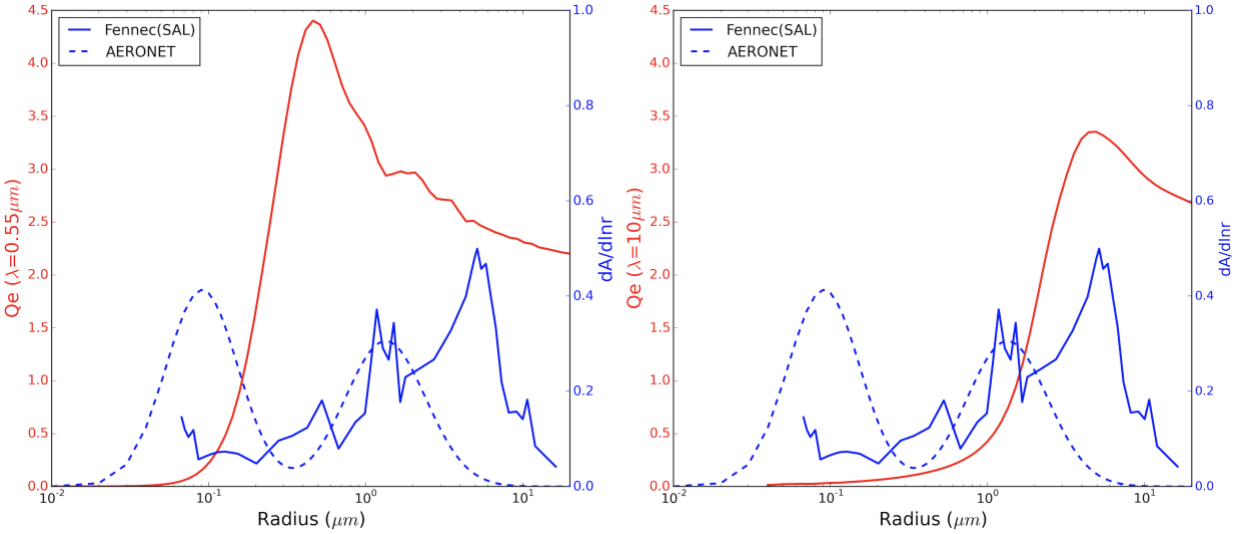
In contrast, the aspect ratio distribution from Kandler [2009] in Figure 4 b is size-dependent. In this case, we assume that the size and shape are independent such that $n(r,\epsilon) = n(r)n_i(\epsilon)$ in each size interval (i.e., <0.25 μm, 0.25μm ~ 0.5 μm and >0.5μm). Accordingly, the bulk scattering properties are computed from

$$< Q_e(\lambda) > \; = \; \frac{\sum_i \left\{ \int_{r_{X,min},}^{r_{X,min}} \int_0^\infty Qe(\lambda, r_X, \epsilon) \cdot A(r_X, \epsilon) \cdot n(r_X) \cdot n_i(\epsilon) \cdot d\epsilon \cdot dr_X \right\}}{\sum_i \left\{ \int_{r_{X,min},}^{r_{X,min}} \int_0^\infty A(r_X, \epsilon) \cdot n(r_X) \cdot n_i(\epsilon) \cdot d\epsilon \cdot dr_X \right\}},$$

Where $n_i(\epsilon)$ is normalized in each size interval $\int_0^\infty n_i(\epsilon) d\epsilon = 1$ in each size interval. The PSD is normzlied as $\sum_i \left\{ \int_{r_{X,min},}^{r_{X,min}} \int_0^\infty n(r_X) \cdot n_i(\epsilon) \cdot d\epsilon \cdot dr_X \right\} = 1$

Q10. Please clarify what the physical reason is that causes a higher extinction efficiency for the Fennec size distribution.

Reply: To explain this, we made the figure below. Here we assumed the Dubovik et al. 2006 size-independent aspect ratio distribution. The two plots are *Qe* as a function dust size at 0.55μm and 10μm (red), respectively, overlaid with the two PSDs, i.e., Fennec (solid blue) and AERONET (dashed blue). Note that we have converted both PSDs to dAdlnr because the bulk scattering extinction efficiency averaging is weighted by the area. Evidently, the AERONET PSD has a peak around r~ 0.1 μm where the *Qe* is very small. In contrast, most of *the Fennec PSD* is in the region where *Qe* is large. This explains why the bulk scattering *Qe* based on the Fennec PSD is significant larger than that based on AERONET PSD.



Q11 Table 3: Please include here the LW DRE efficiency (based on 0.5 um AOD), as you did for your SW results, which is easier to compare between studies.

Reply: Following your suggestions, we have added the following table to the revised manuscript as Table 3.

PSD	Refractive Index	Shape	TOA DRE_{LW} efficiency ($W/m^2/AOD_{0.5\mu m}$)	TOA DRE_{LW} (W/m^2)	Surface DRE_{LW} efficiency ($W/m^2/AOD_{0.5\mu m}$)	Surface DRE_{LW} (W/m^2)
Fennec-SAL	OPAC-LW	Dubovik	10.5	3.0	26.9	7.7
AERONET	OPAC-LW	Dubovik	6.3	1.8	16.4	4.7
Fennec-SAL	Di-Biagio-LW	Dubovik	8.4	2.4	18.9	5.4
Fennec-SAL	OPAC-LW	Sphere	12.6	3.6	32.9	9.4

Q12 Lines 591 – 612: These two paragraphs compare their results to other studies. As such, this really belongs in your discussion section, not your conclusion section.
Reply: In the revised manuscript, we combine summary and discussion into one section 'Section 6 Summary and Discussions'

Q13 Figure 3: Please include a, b, c, d labels. Also, the reference is Di Biagio et al., 2017 (not 2016).

Reply: Done

Q14 Figure 4a: Please include labels on your horizontal axis. Currently, the numbers are not clear.
Reply: Done.

Q15 Figure 8: It's confusing here that the red and blue dashed lines, which denote model calculations with particular microphysics, have the same color as the observation- based lines. Please adjust.
Reply: Done

Review 2

Q1. 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"

Reply: We change the title to "*Net Radiative Effects of Dust in Tropical North Atlantic Based on Integrated Satellite Observations and In Situ Measurements*" (Recommended by reviewer2).

Q2. 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.

Reply: We have revised the abstract accordingly.

Q3. 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.

Reply: We added some brief discussion with relevant references.

Q4. 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".

Reply: Indeed, our definition of DRE is same as the "**instantaneous radiative effect**" in AR5. Note that in the paper, we calculated both *instantaneous DRE* and *diurnally averaged DRE*, where instantaneous DRE is defined as dust DRE derived under the condition (solar position, atmospheric condition) at the measured/computed time to distinguish from the diurnally averaged DRE in section4. To avoid confusion, we added some clarifications to the terminologies we used in the paper.

Q5. In many places the sign of radiative effects is 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 misleading 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.
Reply: Very good suggestion. We have revised the text accordingly.

Q6. 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").
Reply: Thanks for raising this point and the reference. They are added to the revised manuscript.

Q7. 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.

Reply: First of all, although the difference between CERES-CALIOP and CERES-MODIS DRESW forcing is significant, it is still relative small if compared with the range of the DRE based on radiative transfer simulations (i.e., Figure 8). So, this uncertainty does not affect our conclusion.

Second, as you pointed out the difference between CERES-CALIOP and CERES-MODIS DRESW forcing is mainly due to the AOD retrieval difference between CALIOP and MODIS. The potential reasons for the difference may be complicated and are beyond the scope of this study. Since we haven't really studied this topic, we don't think we are able to offer any helpful insight. So, we refer the readers to two recent comparison studies between the MODIS and CALIPSO AOD retrievals by Kim et al. [2013] and Ma et al. [2013],

Q8. 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.

Reply: Thanks for raising this point.
Indeed, in reality the TOA flux is influenced not only by AOD, but also many other factors, such as surface reflectance variation, meteorological conditions, boundary layer aerosols that are undetected by satellite, uncertainty in satellite retrieval algorithm. Although we account for some of these factors (e.g., meteorological conditions), the variability captured by radiation transfer simulation can be expected to be smaller than reality, which explains why the TOA flux vs. AOD relations based on the radiative transfer computations are much less scattered than those based on observations. The R^2 value for the computation-based regressions all exceed 0.95, much higher than the observation-based results in Figure 5.

We do not know whether these uncertainties would lead to a bias in our DRE estimation, because we do not know the variability of these factors. To quantify this the uncertainty caused by this, we reported the uncertainty based on the 1-σ rage. We added some discussion on this point after the discussion of Figure 8.

Q9. Line 192-195. I wasn't totally clear how the aerosol types were determined. Is the aerosol type information from MATCH a2D 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.

Reply: In the CCCM product, the default aerosol typing is from the MATCH simulation, except when CALIPSO detects dust; then dust is used.

In this study, we select cloud-free, dust-dominant cases (at least 90% AOD are attributed to dust aerosols) based on the aerosol type provided in the CCCM product, which, as explained above, is from the CALIPSO observation.

Other than aerosol typing, the CCCM product does *not* provide any dust physical or optical property. In our radiative transfer simulations, we have to assume the dust PSD, refractive index and shape distribution as explained in Section 4.

Q10. 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 covariation in the data?
Reply: This is an excellent question.

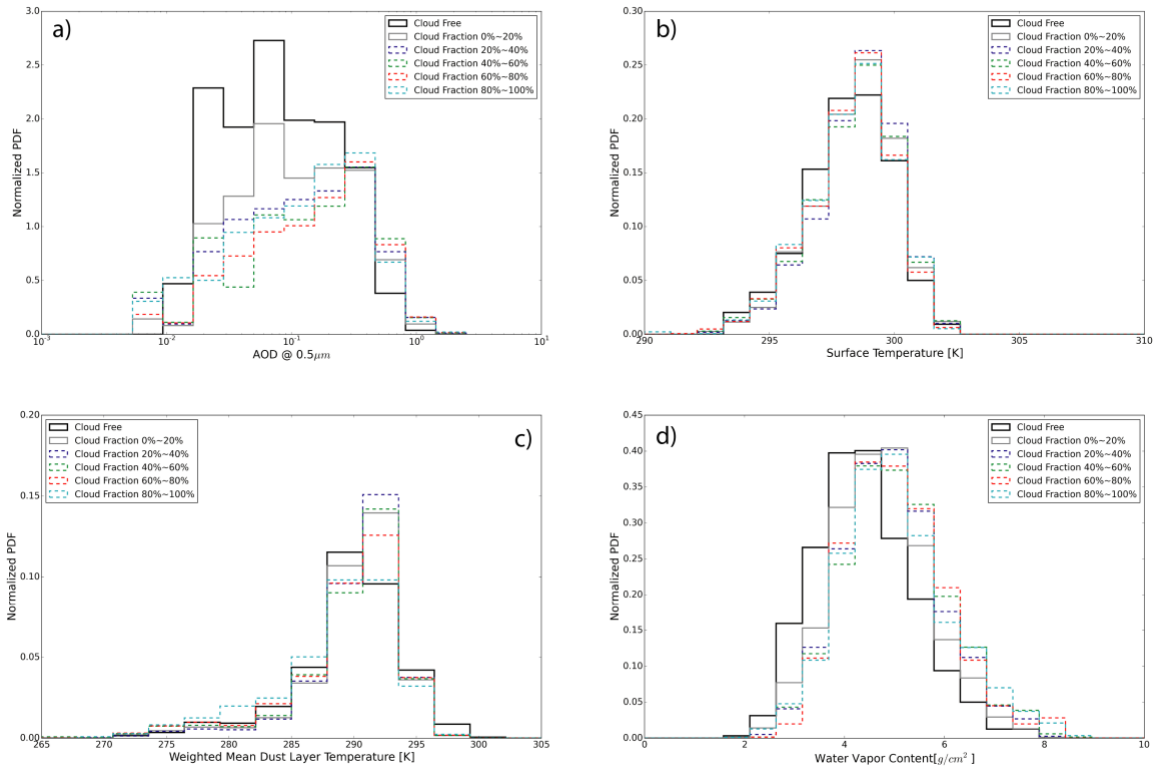
First of all, the sampling rate of cloud-free dust dominant cases also seems surprisingly low to us. As explained in the paper, it is because this region is pretty cloudy making it difficult to find cloud-free CERES pixels. We certainly agree that our observational bases DRE efficiency estimations

would be more reliable if we have more samples. However, CCCM product only provides 5 years' data at present and we have used them all. Therefore, this is the best we could do at the moment.

We appreciate your concern and question regarding the representativeness of our results. We think a key question is *whether the results derived from our cloud-free cases can be applied to the clear-sky part of those cloudy CERES pixels.* To address this question, we investigated if the dust properties (e.g., AOD) and meteorological conditions (e.g., surface temperature and precipitable water) have any correlation with the cloud fraction. If it turned out that the statistics of the dust properties and meteorological conditions from the clear-sky cases are similar to those from the cloudy cases, then we can argue that our results are representative of not only the clear-sky dust dominant CERES pixels, but also the clear-sky part of cloudy and dust dominant CERES pixels.

We first checked the AOD. This time we identify the dust-dominant cases based on CALIPSO observations regardless of the cloud fraction. Then, we divide the selected cases into 5 groups according to the cloud fraction within the CERES pixel, i.e., 0~20%; 20~40% 40~60% 60~80% and >80%. The figure a) below shows the dust AOD histogram of each group. Apparently, the AOD from our cloud-free cases tend to be smaller than those from the cloudy group. If the DRE efficiency remains the same, then the DRE of our cloud-free cases would be smaller than those of the cloudy group.

We don't know whether and to what extent other dust properties, such as size, shape and refractive index, co-vary with the cloud cover. Investigating this is extremely challenging, if not impossible, using satellite observations. We have to leave this for future studies using other types of measurements (e.g., in situ).



After AOD, we also checked the surface temperature, the dust layer temperature (weighted by the dust extinction coefficient from CALIPSO) and the total amount of water vapor in the column. These quantities are potentially important for the DRE_{LW}. As shown in the figure above, in terms of the surface temperature (figure b)) and dust layer temperature (figure c)), the cloud-free cases are very similar to those cloudy-cases.

However, not surprisingly, we found that the cloud-free cases are drier than the cloudy cases (figure d). Note that, given the same dust properties, an increasing of water vapor increases the atmospheric opacity in the LW, which tends to reduce the dust DRE_{LW}.

In summary, if the dust particles properties (i.e., dust size, shape and refractive index) remain the same, then the DRE_{SW} of dust in the clear-sky part of cloudy CERES pixels would be slightly larger than that based on our results because of the larger AOD. In the LW, the larger AOD of the clear-sky part of cloudy CERES pixels would lead to a larger DRE_{LW}, but on the other hand, they are also more humid which would counteract the effect of larger AOD. The net result is dependent on the relative importance of these two competing factors.

We hope these analyzes address your questions. We have added the figure above to the revised manuscript as the new Figure 11 and also discussed the representativeness of our results.

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.

Reply: to Q11 & Q12:
Sorry for the confusion. The text was correct, and the figure caption was a mistake.

We think that the confusion might partly be caused by the fact that, all selected cases have CALIPSO AOD retrievals but only a fraction has MODIS AOD retrievals. In the revised manuscript, we simplified the discussion in this part, by getting rid of the original Figure 5b, because it does not seem to provide any additional insights but might cause confusion.

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.

Reply: Thanks for this good suggestion. To avoid confusion, we have decided to consider 0.7 W/m^2 as a bias in our clear-sky flux computation. We have revised the text and updated the Table 4 accordingly. It does not affect our conclusion, but it indeed simplifies the discussion.

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".
Reply: "semi-observation-based" is now defined at the end of section 3.

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.

Reply: Now we include from 2007 to 2010 in that sentence and add some comments on the merit of extending this study.

Technical corrections:

1. Line 61. It isn't necessary to draw the reader to Figure 1 at this point.
Reply: We delete Figure 1 in line 61.

2. Line 90. Is there a reference to back up the statement that surface emissivity is an important factor in dust LW effects?
Reply: We add a reference to back up the statement. [Yang et al. 2009 'Net radiative effect of dust aerosols from satellite measurements over Sahara']

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).
Reply: We convert wavenumber to wavelength intervals in units of microns.

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".
Reply: revised accordingly.

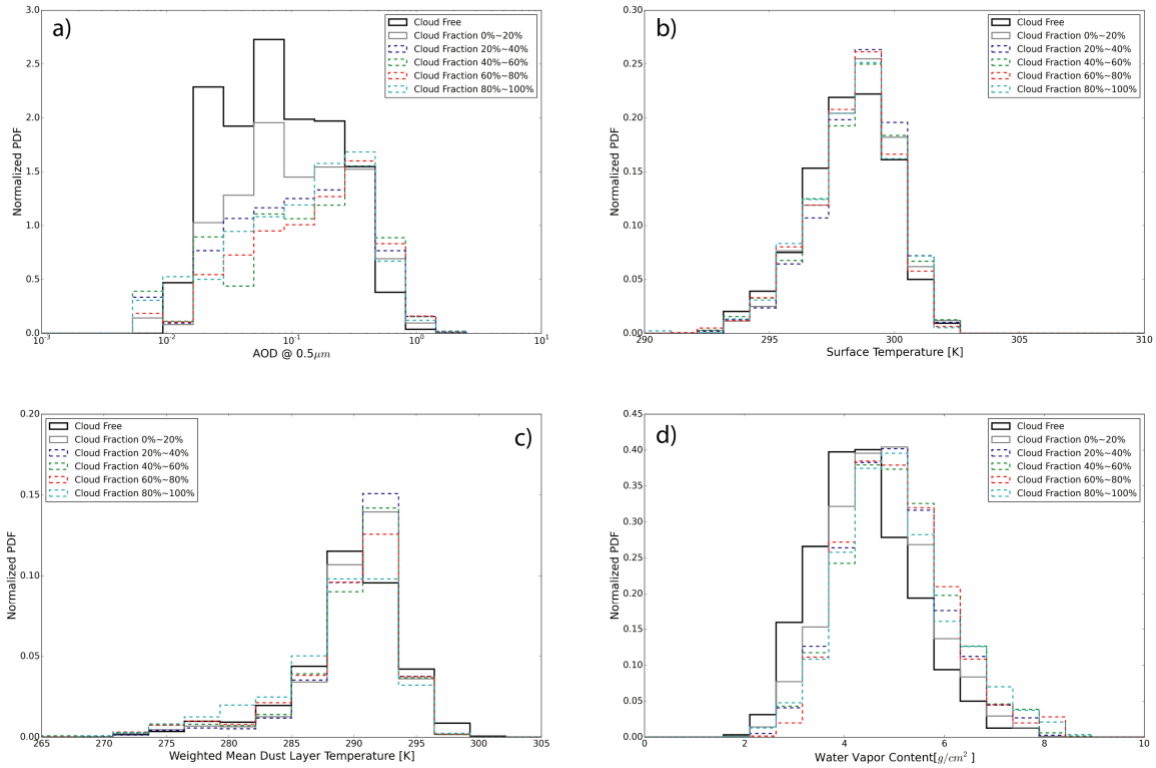
Review 3

1. Only 5% pixels are cloud free in the analysis region and season. Such a small occurrence would make readers wonder to what extent the dust DRE calculated in this study contribute to the dust all-sky radiative forcing in this region. I can see that dust radiative forcing in cloudy sky is complicated and beyond the scope of this study, but it is still helpful to discuss the possible influence of different types of clouds at different levels on the dust radiative forcing at both TOA and surface.

Reply We appreciate your concern and question regarding the representativeness of our results. We think a key question is *whether the results derived from our cloud-free cases can be applied to the clear-sky part of those cloudy CERES pixels.* To address this question, we investigated if the dust properties (e.g., AOD) and meteorological conditions (e.g., surface temperature and precipitable water) have any correlation with the cloud fraction. If it turned out that the statistics of the dust properties and meteorological conditions from the clear-sky cases are similar to those from the cloudy cases, then we can argue that our results are representative of not only the clear-sky dust dominant CERES pixels, but also the clear-sky part of cloudy and dust dominant CERES pixels.

We first checked the AOD. This time we identify the dust-dominant cases based on CALIPSO observations regardless of the cloud fraction. Then, we divide the selected cases into 5 groups according to the cloud fraction within the CERES pixel, i.e., 0~20%; 20~40% 40~60% 60~80% and >80%. The figure a) below shows the dust AOD histogram of each group. Apparently, the AOD from our cloud-free cases tend to be smaller than those from the cloudy group. If the DRE efficiency remains the same, then the DRE of our cloud-free cases would be smaller than those of the cloudy group.

We don't know whether and to what extent other dust properties, such as size, shape and refractive index, co-vary with the cloud cover. Investigating this is extremely challenging, if not impossible, using satellite observations. We have to leave this for future studies using other types of measurements (e.g., in situ).



After AOD, we also checked the surface temperature, the dust layer temperature (weighted by the dust extinction coefficient from CALIPSO) and the total amount of water vapor in the column. These quantities are potentially important for the DRE_{LW}. As shown in the figure above, in terms of the surface temperature (figure b)) and dust layer temperature (figure c)), the cloud-free cases are very similar to those cloudy-cases.

However, not surprisingly, we found that the cloud-free cases are drier than the cloudy cases (figure d). Note that, given the same dust properties, an increasing of water vapor increases the atmospheric opacity in the LW, which tends to reduce the dust DRE_{LW}.

In summary, if the dust particles properties (i.e., dust size, shape and refractive index) remain the same, then the DRE_{SW} of dust in the clear-sky part of cloudy CERES pixels would be slightly larger than that based on our results because of the larger AOD. In the LW, the larger AOD of the clear-sky part of cloudy CERES pixels would lead to a larger DRE_{LW}, but on the other hand, they are also more humid which would counteract the effect of larger AOD. The net result is dependent on the relative importance of these two competing factors.

We hope these analyzes address your questions. We have added the figure above to the revised manuscript as the new Figure 11 and also discussed the representativeness of our results.

2. How is the dust DRE sensitive to the altitude of dust layer? It is a non-trivial question for the longwave radiation. Also, the analysis region is away from the source region, so there should be some variability of the dust layer height.

Reply: Generally, under clear sky conditions, as dust layer altitude increasing, LW dust DRE at TOA increases but surface LW dust DRE decreases. (Under clear sky conditions, SW dust DRE is not sensitive to altitude of dust layer). In our study, we take into account the dust layer height variability by specifying dust aerosol extinction coefficient profile for each case based on CALIPSO retrieval. In future study, we will analyze how the dust layer height influence the DRE_{LW}.

3. Would dust outflows over the North Pacific exhibit similar DRE as the values reported in this study?
Reply: Dust DRE depends on dust aerosol optical depth and dust physical properties such as dust size distribution, refractive index and particle shape. Those physical properties are highly dependent on dust source region and dust aerosol transport processes. Considering the difference in dust AOD and dust source region between dust aerosol over North Pacific and North Atlantic, we would not say they have the similar or different DRE values. We plan to investigate this in future studies.

4) Fig. 7 and 9. Hard to distinguish lines in those figures. Please consider using color plots.
Reply: We changed them to color plots.

5) L84, is it supposed to be [Zhang et al., 2016]? Also Xu et al. [2017, AE] is relevant here and should be cited as well.
Reply: Thanks for suggestion, we fixed our citation and cited the paper Xu et al., 2017.

6) The title is a little bit wordy. Suggest to remove "Through Integrating Satellite Observations and In Situ Measurements of Dust Properties"
Reply: We changed the title to "Radiative effects of dust in tropical north Atlantic based on integrated satellite observations and in situ measurements"