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. <u>Is the aerosol type information from MATCH a2D or 3D field</u> (i.e<u>. is it resolved in the vertical</u>)? <u>Also are the MATCH simulations operational real-time forecasts or reanalysis?</u> 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?

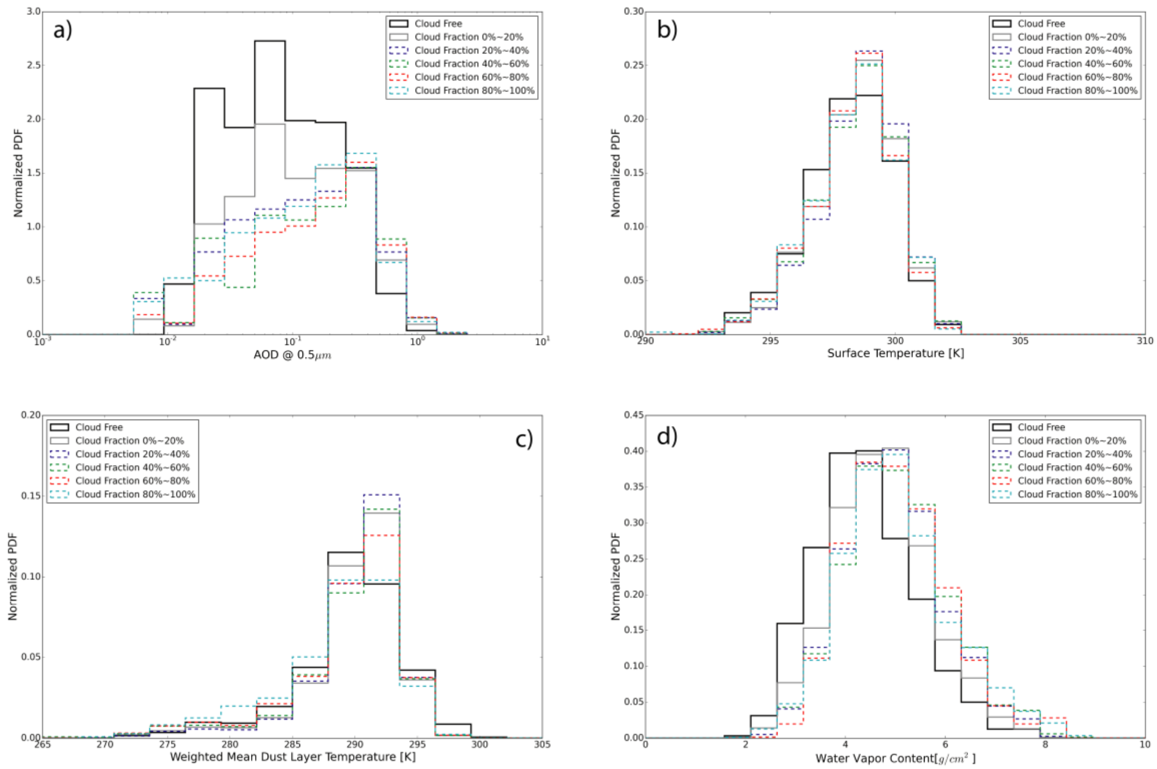
Reply: This is an excellent question.

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.