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*".

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? <u>Did this account for any fractional cloud cover of optically-thin clouds?</u>

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.

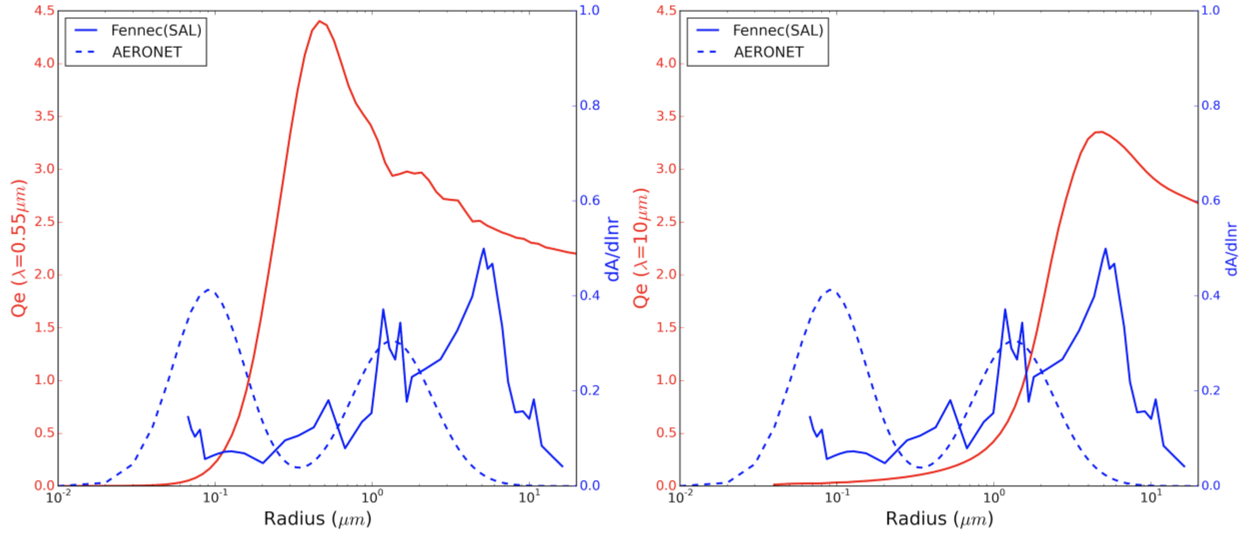
In contrast, the aspect ratio distribution from Kandler [2009] in Figure 4 b is size-dependent. In this case, we assume that the size and shape are independent such that $n(r,\epsilon) = n(r)n_i(\epsilon)$ in each size interval (i.e., <0.25 µm, 0.25µm ~ 0.5 µm and >0.5µm). Accordingly, the bulk scattering 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 $\sum_i \left\{\int_{r_{X,min,}}^{r_{X,min}} \int_0^\infty n(r_X)\cdot n_i(\epsilon)\cdot d\epsilon\cdot dr_X\right\} = 1$

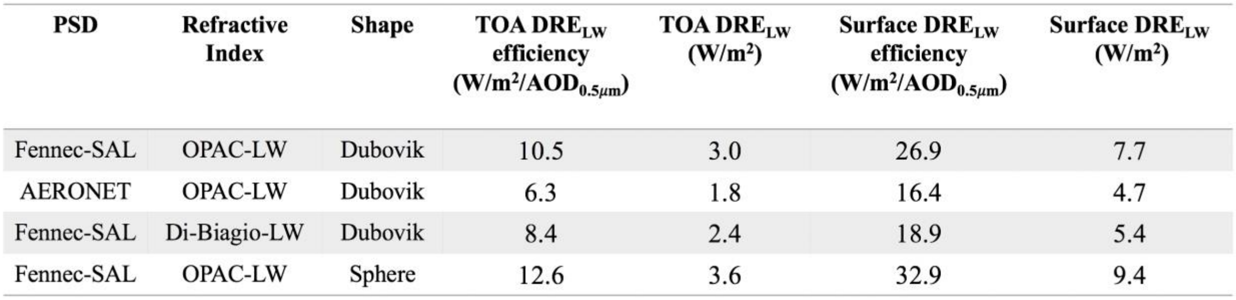
Q10. Please clarify what the physical reason is that causes a higher extinction efficiency for the Fennec size distribution.

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