Reviewer #1

Interactive comment on **Aerosol direct radiative effect over clouds from synergy of OMI and MODIS reflectance** *by Martin de Graaf et al.*

Summary: This paper documents a method called differential aerosol absorption (DAA) to estimate the direct radiative effect (DRE) by the smoke aerosols above cloud (AAC) in the SE Atlantic region using the combination of OMI and MODIS. In this paper, the physical basis of this method is illustrated using selected cases, the uncertainties are analyzed. Applying this method to Aug. 2006 yields an "average aerosol DRE" of 31.5 Wm-2. The topic of this paper is a good match of AMT. The DAA method described in this paper is unique and interesting, although most aspects of this method have already described in early studies i.e., de Graaf et al. (2012, 2014). Overall, I think this paper can eventually be accepted for publication in AMT, but not without significant revision. Below is a list of my major questions and comments. They need to be addressed carefully and thoroughly so the revised paper can meet the standards for publication in AMT.

The reviewer is thanked for the careful evaluation of the paper. The reviewer raises many questions with respect to the quality of the method and the associated uncertainties. We have tried to address all of the questions of the reviewer as best as possible, and clarified the text to more clearly show the strength and weaknesses of the DAA method. We feel the paper has benefited greatly from the improvements in the text and the added analyses and we thank the reviewer for the feedback.

Major concerns/questions:

1) Uncertainties associated with the anisotropy factor B: Eq. (3) is the main theoretical foundation of the DAA method. I think it needs to be explained better than what is in the current manuscript. A main uncertainty I can see is the anisotropy factor B, which is basically the angular distribution model (ADM) used by the CERES to convert the directional radiance to hemispheric flux. In this study it is assumed that the anisotropy factor for AAC is the same as that for clean clouds. But this assumption is not justified or discussed in depth in the paper. It is simply stated that the uncertainty associated with this factor was investigated in de Graaf et al. (2012). Of course, this is not satisfying and sufficient. This uncertainty needs to be carefully addressed in the present study. In particular, the following questions need to be clarified with proper figures, data and/or references.

The uncertainty associated with the anisotropy factor has been raised a number of times before by Dr. Zhang in the past. It is not an issue unique to DAA. E.g. CERES measurements also use the assumption of an unchanging anisotropy factor in their forcing computation. The aerosol direct effect is a function of a scene with and without aerosols, and can by definition never be determined by measurements alone, because both scenes do not exist at the same time. Therefore, RTM calculations have to be performed, with assumptions on either cloud properties, aerosol properties or both. The problem here is that (i) cloud properties can be biased by the presence of aerosols and

(ii) aerosol properties by clouds. The DAA method makes markedly different assumptions than methods that derive COT and AOT and compute DRE using RTM, providing independent validation measurements for these methods. Therefore, these assumptions have been described thoroughly in De Graaf et al (2012). The anisotropy factor was addressed as well, for one representative case, and found to be small.

A more complete and extensive study was performed in 2016 by R.E. Prouty, a master student under the supervision of Dr. Zhang. His master thesis work was complemented with SCIAMACHY DRE analyses and described rather completely the uncertainties associated with the anisotropy factor. Unfortunately, this work was never published in the peer-reviewed literature. However, the master thesis is still publicly available (Prouty, 2016). To address the concerns raised by Dr. Zhang, the analyses in Prouty (2016) have been repeated and the main conclusions added to the manuscript in a separate section. The separate questions are answered below.

a. Anisotropy factor is a strong function of solar-satellite viewing geometry. The uncertainty can be especially large over the special scattering angles, such as rainbow directions. A figure is needed to show the difference between the anisotropy factors for AAC and for clean clouds as a function of satellite viewing direction (i.e., polar contour). This figure can be plotted using the typical solar zenith angle in July or August in the SE Atlantic region at the A-Train crossing time (i.e., 1:30 PM).

This is correct. Figures have been added following Prouty (2016) to show the change of BRDF of a cloud scene for overlying aerosols. They show that the largest change can be found in the cloud bow (single scattering angles around 140°) for optically thin clouds and (obviously) thick aerosol plumes. The largest change in associated DRE was about 11 Wm^{-2}, which is within the error estimate for the OMI/MODIS DRE. However, since the DRE for this case is small, the change due to the anisotropy factor changes the sign of the DRE. Therefore, the assumption on anisotropy factor clearly determines the critical albedo for which the aerosol direct effect changes sign, when estimated using DAA.

b. Moreover, the anisotropy factor for AAC is also dependent on the scattering properties of the aerosol. It has to be explained why simply assuming the same anisotropy factor B for all types of AAC is sufficient.

Obviously, this is true for every assumption on aerosol properties. All methods of deriving aerosol DRE assume an aerosol model, mostly fixed based on location, and in the best case varying the SSA. Assuming a wrong aerosol model (e.g. a dust model where smoke is appropriate) may be as disastrous as assuming no aerosol effect at all.

However, in our papers we assume smoke aerosols, and restrict our analysis to the south-east Atlantic during the biomass burning season in Africa, because we show that smoke aerosols have the smallest bias on the retrieved cloud parameters in our method, and smoke also has a small effect on the cloud BRDF. This would be quite different for e.g. dust, and therefore dust is explicitly excluded in the papers.

c. Similarly, the anisotropy factor for AAC is also dependent special wavelength. The *spectral difference between the B for AAC and clean clouds also needs to be addressed and the uncertainty assessed.*

The analysis by Prouty (2016) showed that the largest effect can be expected at UV-vis wavelengths, where the angular effect of aerosol scattering is largest. At SWIR wavelengths the effect of aerosols is much smaller and more smooth, largely canceling the BRDF change. In the revised manuscript the effects at 555 nm and 2130 nm are compared.

2) The difference between OMI and MODIS cloud reflectance: On page 9 line 20, it is found that "Clearly there is a mismatch between OMI and MODIS for the broken cloud scene, which is caused by rapid cloud changes. The averaged reflectance of the scene has changed during the 15 minutes between overpasses Aura and Aqua." First, I found the difference between the OMI and MODIS cloud reflectance surprisingly large (i.e., 0.6 MODIS vs. 0.4 OMI). So this seems to be a Second, I found the speculation that this difference is caused by "rapid cloud changes" not convincing. Note that the underlying clouds below AAC in SE Atlantic region are mostly boundary stratocumulus clouds. These clouds are pretty stable. It is hard to imagine cloud reflectance changes 50% in only 15 minutes. Min and Zhang (2014) studied the influence of cloud diurnal cycle on the DRE estimation on the basis of MODIS and SEVIRI observations. They found about 5% cloud fraction change in the SE Atlantic region between Terra (10:30 AM) and Aqua (1:30 AM). The two satellites are separated by 3 hours and the cloud fraction change is only 5%. I am not convinced that within 15 minutes the cloud reflectance can change 50% (for convective clouds maybe, but not for stratocumulus).

So this issue/question needs to be addressed and clarified with substantial evidences. I'd suggest the author to use the high-frequency SEVIRI data (15 minutes) to assess how much the cloud property changes within 15 minutes in the studied region.

This question is slightly surprising. Surely, the reflectance in an OMI pixel can change substantially in 15 minutes. The change in reflectance in a pixel due to cloud contamination depends on the wind speed and the size of a pixel. An OMI nadir pixel is 13x24 km^2. To completely fill a cloud-free pixel in 15 minutes, the clouds only have to move in the along-track direction at a speed of $13 \times 4 = 52$ km/h. A cloud fraction of 0.2 is already more than enough to change the reflectance by more than 50% over the dark ocean background, so a mere 10 km/h would suffice. The presented change of 0.4 to 0.6 should not be surprising.

The reviewer probably mixes average values with individual ones. Min and Zhang (2014) present analyses of the cloud heterogeneity, using histograms which are based on a large number of MODIS/Aqua and MODIS/Terra pixels, with many compensating effects. Even then, CF change is significant. Min and Zhang (2014) conclude that marine boundary layer clouds have significant small-scale heterogeneity. However, these numbers are quite different from individual cases, for which the reflectance can change easily, as explained above. Our example of one OMI pixel in which the reflectance change was large due to moving broken cloud fields merely illustrates our strategy of combining OMI and MODIS reflectances, for measurements that are 15 minutes apart. This is relevant for understanding the method described in the manuscript. Differences

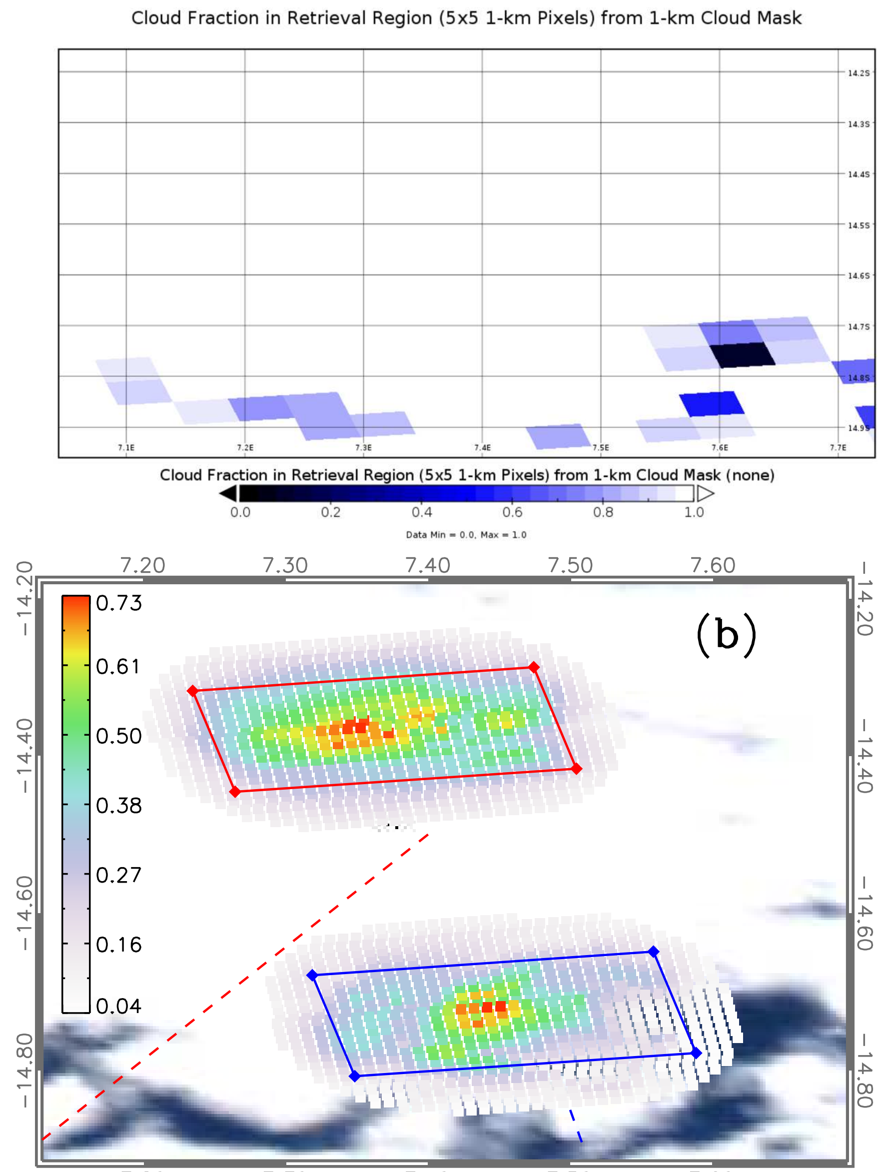

Figure 1: MODIS cloud fraction on 1 August 2006 13:14:09 and 13:14:15 UTC. The OMI pixels were acquired at 13:30:15 and 13:30:21 UTC, respectively

between the OMI and MODIS reflectances occur often, but our strategy to combine them works very well for the derivation of DRE, as explained in the manuscript. The issue was also addressed in (de Graaf et al., 2016).

The OMI FRESCO effective cloud fractions for the pixels in Fig.2 in the manuscript were given in the panels with the spectra. They were 0.69 and 0.35, respectively. Effective cloud fractions are generally smaller than geometric cloud fractions. I tried to determine the MODIS geometric cloud fractions of the pixels using the L2 data cloud data from the MODIS MYD06 dataset. Fig 1 shows the L2 5x5 km2 cloud fraction from 1x1 data in the same area as in Fig.2b of the manuscript, at the time of MODIS overpass, which is 15 minutes before OMI. It shows the open cloud fields just at the edge of the lowest OMI pixel. The most common wind in this area at the surface is from the southeast, and this would have moved the cloud edge over the blue OMI pixel, lowering the FRESCO eff. CF for this pixel to 0.35.

A better way of determining the geometric cloud fraction in the OMI footprint would be to count MODIS pixels with cloud mask on and off, but this was not further attempted.

"Rapid cloud changes" is a misleading term though. It should be "significant reflectance changes due to changes in cloud fraction". We have removed the term from the manuscript, and rephrased the sentence more carefully.

3) Sampling rate of the DRE needs to be provided: In my opinion, the DRE values are only meaningful and useful when the corresponding sampling rate is given side by side. It seems to me that the DAA method described here is only applicable to certain portion of the total cloud fraction. But the paper provides no data or analysis of the sampling rate. As shown in Zhang et al. (2016) as well as many previous studies, the DRE is dependent on both AOT and COT (See their Figure 9a). If a method only samples, say large COT and large AOT, then the DRE from such method would yield larger DRE than another method that can sample all COT and AOT. But the results from the two methods are not directly comparable. Because of the lack of sampling rate information, it will be difficult for other researchers to compare or use the DRE results from this study.

Indeed, polar orbiting satellites only sample the atmosphere at one particular time. And DAA is only applicable to a certain portion of the total cloud fraction, i.e. for scenes that are sufficiently cloudy. Therefore, the title states explicitly that only cloud scenes are considered, and the DRE will be (mostly) positive. Furthermore, in this section 4.2 and Figs 5 and 6 is was explicitly stated that only scenes with a CF > 0.3 were selected. However, the statements were absent from the conclusions and in the abstract, and the explicit CF sampling has been added there as well.

To address this issue, I believe the following sampling rates need to be provided with proper figures or tables:

a. What is the total cloud fraction identified by the collocated OMI-MODIS observations?

A minimum of CF=0.3 is always adopted, as stated in section 4.2 and Figs 5 and 6. In Fig 2. it was shown that the CF for the two scenes were quite different, 0.69 and 0.35 respectively, as was indicated in the figures.

b. What is the fraction of cloud with detectable AAC, e.g., UV-AI >2?

There is no filter on any aerosol or reflectance conditions. All cloud scenes with CF>0.3 and CP<800 hPa were processed, see section 4.2. Scenes without aerosols will yield zero DRE (ideally, see Fig. 6).

c. What is the fraction of the cloud with valid DRE estimation using the DAA method?

Valid cloud retrievals are possible for scenes with a minimum CF of about 0.15. The exact number was not analysed. This is, however, irrelevant, since only scenes with a CF >0.3 are considered. Cloud information is included in the dataset, though.

d. The above information should be provided for all DRE results, for example, Figure 5. I am wondering to what extent the inter-annual difference in Figure 5 is due to real cloud or aerosol and to what extent it is actually due to year-to-year sampling rate difference.

All of it, since SCIAMACHY and OMI/MODIS DRE were compared for only those scene that had a CF > 0.3. As was explicitly stated in the text, and the caption of Fig.5.

e. Another relevant question is: What are the DREs for the cloud with detectable AAC but the DAA method cannot be used for any reasons?

Obviously, it is difficult to present the DRE using DAA for those scenes that the DAA fails.

The cloud retrievals fail for CF around 0.15 or smaller. Scenes with 0.15 < CF < 0.3 are filtered for the analyses. The aerosol DRE for cloud-free scenes (down from CF < 0.3) can and have been analyzed with different techniques than DAA, e.g. Chand et al. (2009); Jethva et al. (2013); Meyer et al. (2015). The current study is not suitable nor intended to answer this question.

The sampling rates need to be provided whenever the DRE values are given, i.e., in the abstract and conclusion.

Indeed, the sampling rates were not repeated in the abstract and conclusions. This omission has been corrected.

4) The DRE results need to be presented more carefully: It needs to be empathized in the abstract and conclusion that the DRE from this study is the instantaneous DRE only at the A-Train over passing time. When talking about the "daily averaged" DRE, the following questions need to be clarified: a. Is it a diurnal average, i.e., including nighttime, or only daytime average? See Zhang et al. (2016) about diurnal average. b. Does the daily average account for the diurnal cycle cloud clouds? See Min and Zhang (2014) about the impact for cloud diurnal cycle on DRE estimation.

This has been corrected. Indeed, the average values were area-averaged only, for each day. "Daily area averaged values" is ambiguous, "Daily, area-averaged values" was intended. However, the term has been dropped entirely, to unambiguously state that "area-averaged instantaneous DRE values" are presented for each day. This has been changed throughout the manuscript.

Reviewer #2
Interactive comment on **Aerosol direct radiative effect over clouds from synergy of OMI and MODIS reflectance** *by Martin de Graaf et al.*

This paper describes a method to estimate the direct radiative effect (DRE) of aerosols above clouds using OMI and MODIS measurements. The technique (differential aerosol absorption, DAA) is somewhat different from related algorithms as is in essence a retrieval of the radiative effect itself rather than being focused on the optical/microphysical quantities of the aerosols and clouds, which gives it a different set of strengths and weaknesses from other above-cloud aerosol algorithms. The DAA retrieval is an extension of an algorithm published by the authors previously, which used SCIAMACHY instead. The SCIAMACHY record ended in 2012, while OMI/MODIS are still flying, and other sensors with similar capabilities also fly now and are planned for the future (e.g. OMPS/VIIRS, and PACE OCI). SCIAMACHY was a spectrometer with a coarse footprint, while OMI is a UV-vis spectroradiometer and MODIS is multispectral. MODIS and OMI also have different footprints (both finer than SCIAMACHY) and fly on different platforms. So, the adaptation of the algorithm from SCIAMACHY to these other sensors is of scientific interest and sufficiently non-trivial and novel. This work is well in scope for AMT.
The quality of language is good. With the exception of Section 5, which was a let-down, the paper is pretty good. Overall I recommend publication after minor revisions; some points in the text need expanding and I have a few concerns with the error budget, as well as the lack of use of ORACLES data. I would be happy to review the revision.

Comments/Corrections *Page 2, lines 23-27: While POLDER is probably the most informative, there are several techniques to estimate above-cloud AOT and COT from MODIS and/or OMI alone. See e.g. work by Meyer, Sayer, Jethva for various algorithms. I'm not saying that the authors have to cite each paper in this field, but a brief acknowledgment/discussion of the fact that there are several MODIS or OMI techniques which have been developed and used successfully already, and it's not only POLDER and CALIOP which have these capabilities, would be welcome.*

This is very correct observation. All the references to other methods ended up in the accompanying paper about the OMI/MODIS - POLDER comparison. The references have been added to this manuscript as well, it was no intention to disregard the work done by other authors.

Page 2, line 26: This mentions that a comparison with POLDER results is presented elsewhere. I went to the bibliography and this is listed as a study in preparation for submission to GRL. If this work has already been done, it would be good to briefly summarize the results. This is relevant because the POLDER technique is quite different from DAA. Otherwise, I'd just say that the comparison will be performed and remove the citation. I suppose the progress of both papers can be assessed at the time that this manuscript is revised. Given this paper is cited again on page 14, I think it's important that we get to see the results, which we can't because the paper being cited *hasn't even been submitted yet. Basically, either give us the information or remove the citation.*

The information has been added and the citation removed.

Page 3, line 5: I'd add a brief discussion of and references in support of the assumption of negligible aerosol effects in the longwave. While agree it is probably the case for smoke, it may not be for dust. I know there are various papers looking at shortwave vs. radiative effects of dust under various conditions (e.g. over land, ocean, daytime, nightime, cloud). I think it's important to acknowledge when/where this assumption is reasonable and the magnitude of the error from assuming it is negligible. Some readers might otherwise assume it is always negligible. This is mentioned later on page 5, but I'd state it here too.

Correct, the application to dust aerosols will fail. The restriction to smoke aerosols was added.

Equation 3: this is the core of the method; the most questionable assumption here seems to me to be that the anisotropy factor B is the same for an aerosol-laden and an aerosol-free cloud. Intuitively one would expect the aerosol-laden scene to be less anisotropic. Page 4 directs the reader to de Graaf (2012), and I found that their Section 6.2 addresses this. I realise that these errors are often AOT-dependent but to give the reader a rough idea of expected performance for the SCIAMACHY case (as a reference for the present MODIS/OMI), I suggest summarizing this information here (either the total figure of 8 Wm-2 given in section 6.3 of the 2012 paper, or a brief quantification of the individual components) so the reader does not need to dig out the previous paper.

This section has been extended with a thorough analysis of the uncertainty due to anisotropy factor, following the suggestion of reviewer #1. In addition, the reference to SCIAMACHY results have been removed, and an error estimate for OMI/MODIS measurements only has been added.

Section 4.3: If I understand this correctly, the biggest contribution to the retrieval error is estimated as the calculated forcing for pixels where the UV aerosol index (UVAI) is less than 0. This has mean and standard deviation 7 and 12 Wm-2 respectively. This is fine in theory but I have some questions in practice. UVAI is a semi-quantitative detection since it depends on not only aerosol absorption but also on factors including solar/view geometry, altitude, cloud properties, underlying surface (in cases of broken cloud) etc. The threshold value of 0 is not supported by radiative transfer arguments as far as I can tell, but rather seems a hand-waving threshold that is a nice round number. While sensible as a first approximation it is certainly possible to get negative UVAI when there is some absorption (this is even shown in the de Graaf 2005 paper the authors cite at this point), or positive when the aerosols are only scattering; while one might argue that this would contribute to the scatter in Figure 6, there is no reason to assume that it would lead to an unbiased estimate. Thus the reported systematic bias of 7 W m-2 might be true, or might be the result of choosing

0 as the UVAI threshold when another one would be more appropriate. It is not clear which UVAI the authors are using (there are several definitions and data versions). I believe the latest OMI standard product version includes a new definition and calculation which reduces the dependence on factors like geometry (see Torres et al 2018, https://doi.org/10.5194/amt-11-2701-2018). If this was not what was used, I recommend repeating this analysis with it. The new OMI UVAI will reduce some of these confounding effects such that it is a better proxy for aerosol absorption. It should make the authors' assessment of systematic/random errors here more realistic. So, my suggestions are: (1) Ensure that the latest OMI UVAI data set is used for this calculation, to decrease the confounding non-aerosol effects, and (2) acknowledge that UVAI=0 as a threshold is arbitrary and mention (or even better), estimate the additional uncertainty this is contributing to the error analysis in section 4.3. Perhaps a better threshold than UVAI=0 could be determined and adopted.

This is a very much appreciated observation by the reviewer. The used verion of the AI was the OMAERO AI v1.2.3.1 developed and maintained at KNMI, using the 354/388 nm wavelength pair. A small analysis on the dependence on the threshold showed a decrease in bias with decreasing AI threshold, see the table below. The average DRE is 1–2 for UVAI down to -1.5 and -1.0, below which too few pixels remain. This is lower than the mean of 7 that we found for the indeed arbitrary threshold of 0.0. Increasing the threshold further increases the mean, as expected, as more and more pixels with absorbing aerosols are incorporated. So, indeed the assumed bias seems

Table 1: UVAI threshold analysis results

OMAERO v 1.1.1			
AI	bias	std. dev.	number of scenes
-1.5	2	10	44
-1.0	1	12	368
-0.5	4	11	2740
0	7	12	12471
0.5	10	13	32067
1.0	13	15	59564
1.5	15	16	89021
2.0	17	17	109480
OMAERUV 1.8.9.1 (2017)			
UVAI	bias	std. dev.	number of scenes
-1.5	17	15	9
-1.0	12	13	87
-0.5	13	11	899
0	15	12	10579
0.5	18	12	46454
1.0	21	13	103513
1.5	24	14	152008
2.0	24	14	180830

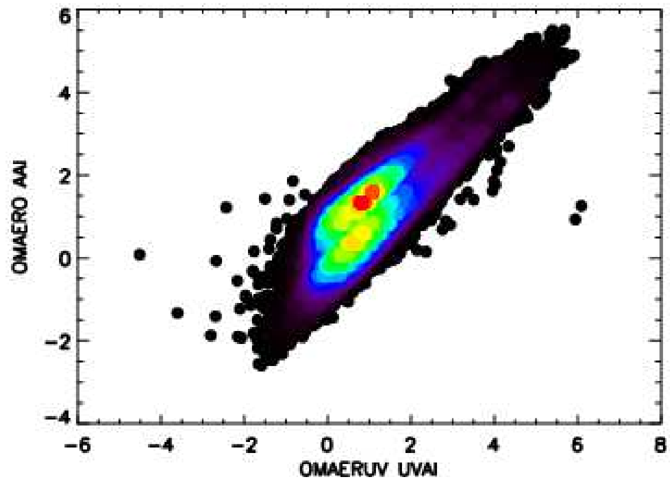

Figure 2: Scatterplot of OMAERUV UVAI v1.8.9.1 vs OMAERO AI v1.2.3.1

to disappear with more stringent filtering on AI. Interestingly, the standard deviation does not change much with AI threshold, suggesting that the standard deviation is a good estimate of the random error, i.e. the ability to correctly simulate a cloud scene spectrum and estimate the DRE from that.

The analysis was repeated with the new OMAERUV UV-AI developed at NASA. The definition for this aerosol index is very different than the OMAERO AI. The influences from cloud scattering is included in the index using simple scattering layers in the RTM-generated LUTs, using Mie or HG clouds. The OMAERO AI and OMAERUV UV-AI were compared in Fig. 2 for all 2006 scenes in this study (with and without aerosols). As the figure shows, their is a strong correlation between the products, but there are also very clear differences.

A repetition of the analysis above showed that the OMAERUV UVAI seems unsuitable for removing absorbing aerosols in cloud scenes, see table 1. With different UVAI thresholds the average DRE is always significantly higher than 0. The reason is unclear, but maybe for fully clouded scenes the effects of simulating cloud reflectances in the LUTs is so large that the aerosol effects are not significant anymore.

Section 5: Honestly this section is a bit of a let down and missed opportunity. The authors show time series of radiative effect during the CLARIFY, ORACLES, and LASIC campaigns, and give citations about them. However the analysis amounts to plotting back-trajectories and showing tmie series of AERONET AOT against DRE. None of the actual data from the field campaigns appears to have been actually used. The ORACLES data are already freely available from https://espoarchive.nasa.gov/archive/browse/oracles. This includes a large number of relevant observations including e.g. irradiance/flux which could be used to evaluate the algorithm's output more quantitatively, rather than *just showing that AOT at Ascension Island is correlated with DRE over the southern Atlantic Ocean. I strongly urge the authors to look at these data as there are bound to be some matches close in space/time to the A-Train overpass. It would help give a sense of whether the DRE magnitudes are reasonable, as right now all we can say is that temporal variation seems reasonable. As-is, the paper's introduction and section 5 state these plots are presented "in support of" these campaigns, but there's really no linkage demonstrated in what's actually contained in the paper.*

We fully agree with the reviewer, and a comparison with aircraft would be very valuable. We have tried to add comparisons with ORACLES data, which are indeed freely available. We also contacted individual researchers in the ORACLES community. Unfortunately, it was not possible to add anything significant within the time frame of the manuscript review period. The analysis of aircraft data is specialized work, and a thorough comparison deserve more time than was available here. A separate publication would be more suitable for this.

The suggestion of the editor was followed to compare with satellite AOT from OMI and MODIS, to at least present some more evidence of correctness of the DRE magnitude. Also, all references to the support of the aircraft campaigns were removed. The manuscript now states the existence of the campaigns and the data, and merely illustrates the DRE data during this period, as it was intended.

Conclusions: this quotes mean and standard deviations of DRE. I'd be interested to see some pdfs somewhere in the paper, to see what the distributions look like at different scales. If they are skewed then mean and standard deviation might not be the best summary metrics, perhaps median and interquartile range would be better. This could also be something to add to the SCIAMACHY comparison section, for example: show whether the pdfs of DRE are similar to within the expected level of consistency for e.g. a season's worth of data over the south Atlantic. This would complement the existing instantaneous consistency assessment with a more climatological consistency assessment, which is after all important if the end goal is to move toward a long-term post-SCIAMACHY record.

A figure of histograms of OMI/MODIS DRE and SCIAMACHY DRE has been added.

I was also surprised not to see any mention of VIIRS/OMPS in the paper. These sensors fly on SNPP (since 2011– there's even a brief overlap with the SCIAMACHY record) and NOAA20 (since 2017), and have similar capabilities overall to MODIS/OMI. In some senses they would even be a better choice than the MODIS/OMI pair, because they fly on the same satellite, which simplifies some of the collocation/time difference issues. Again, I don't expect the authors to demonstrate the algorithm with VIIRS/OMPS, but a brief mention that this sensor combination exists and the relative merits of the sensor pair would be welcome.

True. This has been added to the conclusions.

We thank the reviewers for the helpful comments to improve the manuscript.

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