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

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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.

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

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).

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.

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.

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., $\sim$0.6 MODIS vs. $\sim$0.4 OMI). So this seems to be a Second, I found the speculation that this difference is caused by "rapid cloud changes" not convincing. Note that the underlying clouds below AAC in SE Atlantic region are mostly boundary stratocumulus clouds. These clouds are pretty stable. It is hard to imagine cloud reflectance changes 50% in only 15 minutes. Min and Zhang (2014) studied the influence of cloud diurnal cycle on the DRE estimation on the basis of MODIS and SEVIRI observations. They found about 5% cloud fraction change in the SE Atlantic region between Terra (10:30 AM) and Aqua (1:30 AM). The two satellites are separated by 3 hours and the cloud fraction change is only 5%. I am not convinced that within 15 minutes the cloud reflectance can change 50% (for convective clouds maybe, but not for stratocumulus). So this issue/question needs to be addressed and clarified with substantial evidences. I'd suggest the author to use the high-frequency SEVIRI data ($\sim$15 minutes) to assess how much the cloud property changes within 15 minutes in the studied region.

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. 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?

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

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

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.

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?

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

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 overpassing 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.

Min, M., and Z. Zhang (2014), On the influence of cloud fraction diurnal cycle and sub-grid cloud optical thickness variability on all-sky direct aerosol radiative forcing, Journal of Quantitative Spectroscopy and Radiative Transfer, 142 IS -, 25–36, doi:10.1016/j.jqsrt.2014.03.014.

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