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Atmospheric Measurement Techniques Discussions

Interactive comment on "A novel method for estimating shortwave direct radiative effect of above-cloud aerosols using CALIOP and MODIS data" *by* Z. Zhang et al.

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Review of Zhang et al. AMTD

This is a very good paper that studies the direct radiative effect of partially absorbing aerosol above underlying clouds. The paper provides results that are very similar to the pixel-by-pixel approach of Meyer et al. (2013), but is significantly faster and therefore provides a potential basis for deriving the above cloud direct radiative effect which can be compared/contrasted to those derived by models.

However, the authors should consider the following points:-

Clarity:

Section 2.1. In thick biomass burning regions, there is some evidence that the AOD derived from the CALIOP 532nm channel may be attenuated to an extent where it does not penetrate down to the bottom of the aerosol layer leading to too small AODs being retrieved (AGU meeting, Jethva pers. Comm..). This may lead to a low bias in the AOD derived from CALIOP which may bias the results presented in this paper.... However, this work has not yet to my knowledge been published. I leave it to the authors whether they want to follow up on this and include a caveat.

P10000, l5. Section 3.1., and section 3.2, l17. Having had a look at the Omar et al (2009) paper, and the Meyer et al (2013) paper, I'm a little confused about the aerosol model. It appears that you are interested in the two most absorbing aerosol types, (1) smoke and (2) polluted dust, as evidenced by the imaginary part of the refractive index in Omar et al (2009), Table 1. You state that the Mie scattering calculations are based on "the aerosol model in Meyer et al (2013)". The Meyer model appears to be based on the assumption that the overlying aerosol is the CALIOP determined smoke aerosol (based on the imaginary part of the refractive index that is assumed). Is there a different model used for polluted dust or do you just use a single assumed model for the polluted dust and the smoke aerosols classified by CALIOP? I would suggest that aerosol off the coast of the Sahel/Sahara where dust is mixed with BB during DJF (see e.g. schematic diagram and results from the AMMA/DABEX field campaign, Haywood et al., JGR, doi:10.1029/2008JD010077, 2009, Fig 12) that the column BBA/dust single scattering albedo may be greater than 0.9 as the dust is essentially non-absorbing (Omar's paper suggests this).

P 10004, l22-26. I would be tempted to swap the results round: you've gone to the trouble of correcting for the underlying COD underestimate, so why not show these?

Fig 3. You show comparisons against the pixel-pixel approach. Why not show a scatter plot i.e. a vs c and b vs d. Then you could do a short statistical analysis to see if the

relationship is indeed linear.

Section 4, p10004, l 28. I like that you've gone to RFE. I don't know whether you have enough statistics to go slightly further – to RFE/COD in Wm-2/ACA AOD/COD. This would mean that you can actually start to make comparisons against models which underestimate the stratus cloud deck AOD for whatever reason. It could be a useful number to have.....

Section 5.

#1. I'm not sure whether the SSA of $\sim$0.9 is used for both smoke and polluted dust (see comments above). The negative DREs that you refer to at the top of p10007 are presumably because the underlying cloud is so thin that it doesn't impact the effective surface reflectance enough to tip over the balance point from negative to positive forcing. You may want to clarify this point with a few more words.

#2. I think that something should be said about how best to make use of the data that is presented in the paper. You should highlight that the measurements that you make are valid for a certain time of the day (A-train overpass). The diurnal cycle of cloud in the SE Atlantic is quite high, with less cloud at the time of the AQUA overpass than e.g. earlier on in the day (stratocumulus burn off) so models should endeavour to compare against a similar time period.

#3. I think that you are missing an important reference – that of de Graaf et al. (2011) who use SCIAMACHY to show the spectral distribution of the DRE derived above clouds. It would be even more interesting to see what you're modelled spectral dependence of the DRE over cloud is and whether your spectral dependence derived from an assumed model looks like what is derived from SCIMACHY (that doesn't assume an aerosol model). My suspicion is that the constant refractive index assumption that is used in the model (particularly the imaginary part), may lead to not enough absorption in the UV (absorption of brown organic carbon likely has a role in this), and hence the DREs and RFEs may be rather smaller than if a wavelength dependent refractive index

were used.

Typos/minor comments:

P9996, l21. I would remove most recently as you've already discussed this paper in the preceding paragraph.

P9998, l7. assumption -> assumptions

P10001, l3. While Constantino and Breon find no correlation between above-cloud AOD and below-aerosol COD, I think it worth stating that they do find a relationship between cloud fraction and aerosol index. Otherwise the reader may infer that there is no correlation and/impact at all.

P10003, l5. (e.g. .....) should be just after the first bracket.

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