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Atmospheric Measurement Techniques Discussions

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

Anonymous Referee #2

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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.

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.

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.

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.

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.

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.

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.

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.

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.

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