Review of "Satellite-based estimate of aerosol direct radiative effect over the South-East Atlantic" by L. Constantino and F.-M. Bréon, submitted to ACP

October 15, 2013

In this paper, the authors present a very detailed quantification of the direct radiative effect (DRE) of aerosols over the region of the South-East Atlantic. This region is one of the most intensely studied ones in terms of aerosol-cloud interactions on the globe, yielding ideal conditions for quantifying the effect of aerosol on semi-permanent oceanic stratocumulus cloud decks. Plumes of absorbing aerosol stemming from biomass burning over the African continent are frequently advected over these cloud decks, making the quantification of the DRE of absorbing aerosol above clouds feasible.

The authors base their analysis of the DRE on co-located measurements of aerosol- and cloud properties obtained from CALIPSO and MODIS. The dataset is compiled by the same author team and is presented in Costantino and Bréon (2013) and spans a time period of six years (2005 – 2010). The observations are used to drive a radiative transfer model (RTM) to estimate the DRE of the aerosol. Here, CALIPSO provides information on the vertical distribution of clouds and aerosols in the atmospheric column. Thus, aerosol can be quantified as residing either below, above or at the same level as the ambient cloud. Cloud and aerosol optical properties derive from MODIS observations. In contrast to previous studies using RTMs to estimate the DRE of aerosols in this region, the authors parameterise the single-scattering albedo (SSA) of the aerosol on a retrieval-by-retrieval basis, thus accounting for the expected spatial heterogeneity of the SSA. The observations are aggregated to achieve a $1^\circ \times 1^\circ$ spatial and monthly temporal resolution, thus reducing uncertainty from previous studies who used either coarser spatial or longer temporal averaging (or both) to derive the DRE. Overall, the DRE of aerosol over the South-East Atlantic derived in the present study compares well with previous studies. Because the present study also accounts for cases of aerosol below cloud and aerosol mixing with cloud, the DRE estimates presented here provide a more complete picture compared to previous studies.

The paper constitutes an important contribution to the field, is well structured, some parts of the manuscript lack preciseness, most uncertainties of the method are addressed and discussed, and the level of English is appropriate in most parts of the manuscript.

My main issue with the present manuscript is that the authors have failed to adequately address one very important uncertainty of their method which derives from using MODIS retrieved values of cloud optical thickness (COT) in conditions of overlying absorbing aerosols (see discussion below).

I recommend publication of the manuscript in ACP after the following issues have been adequately addressed.

Using COT from VIS/NIR measurements to run the RTM

As mentioned above, the authors fail to acknowledge and discuss the uncertainties associated with the retrieval of cloud microphysical properties from satellite observations in the VIS/NIR spectral range when absorbing aerosol overlies clouds. Absorbing aerosol residing above a cloud can lead to substantially biased retrievals of COT, with errors on the order of 10-20% (e.g. Haywood et al., 2004; Meyer et al., 2013). The MODIS observations of COT (and cloud droplet effective radii) used here rely on retrievals in this spectral range, thus possibly biasing the results presented in the submitted manscript.

What deranges me most is that the authors are very well aware of this issue, but choose to ignore it for the present study. The issue was mentioned by anonymous referee #2 in the review process to Costantino and Bréon (2013), which is viewable on the ACP homepage. In their reply to the reviewer's concern, the authors mention that based on the findings of Haywood et al. (2004), "...aerosol can be responsible of an error on retrieved COT up to 10-20% in the case of smoke... ". In the final published paper, it says that "...cloud retrievals based on the 0.86/2.1 μm combination are thought to be little affected by the presence of biomass burning and dust aerosols...". In my view, an error of 10-20% is not a "little" effect and cannot just be neglected and MUST be discussed in the present study, especially because COT is used to drive the RTM to derive the DRE.

I understand that accounting for such retrieval errors is very difficult to achieve. However, I would like to see the authors discuss their methodology and results in light of the work by Meyer et al. (2013). As that paper is very recent, it may have skipped the author's attention. Meyer et al. (2013) quantify the DRE of absorbing aerosol above cloud over the region of the South-East Atlantic Ocean by using co-located CALIPSO and MODIS observations. Additionally, they account for the MODIS retrieval errors in cases of overlying aerosol by applying a research version of MODIS Collection 6 retrieval algorithms. They show that neglecting MODIS COT retrieval errors leads to a non-negligible underestimation of the positive DRE of absorbing aerosol above clouds.

Using the findings of Meyer et al. (2013), the authors could substantially increase the foundations of their methodology. I propose they try to obtain some of the corrected data used in Meyer et al. (2013) to perform a sensitivity using the corrected COT values. I do not know if the author team of Meyer et al. (2013) is willing to cooperate, but I think it is definitely worth a try. If this fails, the authors should present at least a back-of-the-envelope type assessment of the effect the biased retrievals of MODIS COT have on their results.

In any case, a discussion of the effect is needed.

Minor points

- P23296, line 19 (and elsewhere in the manuscript): The authors often speak of their results in terms of mean and standard deviation. However, this may lead to substantial confusion because it seems that the calculated distributions of DRE shown in Fig. 4 of the manuscript are heavily skewed towards values of large positive DRE. Perhaps this is just an artefact of the log-linear presentation. How close to Gaussian are the distributions of calculated DRE ?

  If the distributions are considerably non-Gaussian, it would make more sense to speak of the variability in calculated DRE in terms of percentiles or quartiles.

  As mentioned, this applies to every instance where the authors mention the alleged standard deviation of DRE and I will not mention it further.

- P23297, lines 3–4: The reference to Bréon et al. (2002) is not listed at the end of the manuscript. Furthermore, the authors should cite at least one of the more general references to global aerosol indirect effects (e.g. Haywood and Boucher, 2000; Lohmann and Feichter, 2005; Quaas et al., 2009).

- P23297, line 8: I am not sure what the authors mean by this. Conventionally, one refers to the "Earth's outgoing radiation" as the outgoing longwave radiation (OLR). However, aerosols have a negligible effect on longwave radiation. Maybe the authors mean the change in outgoing shortwave radiation due to changes in local planetary albedo ?

- P23297, lines 9–13: Please cite appropriate literature describing direct and indirect aerosol effects here and perhaps give a bit more detail on the processes involved.
- P23297, lines 13–15: True. However, in a strict sense, radiative forcing can only be diagnosed after stratospheric temperatures have adjusted to a change in tropospheric temperatures. In terms of diagnosing aerosol radiative effects which act on substantially shorter timescales than needed for stratospheric adjustment, it is better to adopt the nomenclature "radiative flux perturbation" as described in Lohmann et al. (2010).
- P23297, line 19: By definition, the term "radiative forcing" is only applicable to the radiative effect brought about by anthropogenic substances in the atmosphere. True, both natural and anthropogenic aerosols may have a cooling effect on the Earth System, but only the anthropogenic ones can be considered as "forcing". The authors often confuse this throughout the manuscript. In fact, because their methodology generally considers all aerosols, the derived DRE should not be termed as "forcing" anywhere in the paper.

  This referes to all occurrences in the manuscript and I will not mention it further.
- P23297, line 20: Please mention that absorbing particles have a SSA<1.
- P23299, lines 1–5: The review of studies focusing on the DRE of absorbing aerosols above clouds from observations is incomplete. Numerous studies have produced such estimates and their contribution to the field should at least be mentioned if not shortly reviewed and contrasted (Peters et al., 2011; Wilcox, 2012; de Graaf et al., 2012; Meyer et al., 2013).
- P23300, line 10: Please insert an appropriate reference here.
- P23300, line 15: I partly disagree. Yes, dust generally has a higher SSA than biomass burning aerosol. However, dust still yields substantial absorption at visible wavelengths with SSA values spanning $\approx$0.82–0.97 in the range of $\approx$400–800 nm (PRIDE campaign, Bergstrom et al., 2007). This is confirmed by the results of Peters et al. (2011), who find a reduction of local planetary albedo (shortwave) in overcast scenes (positive direct radiative effect) everywhere over the tropical- and subtropical Atlantic Ocean. Over the tropical north-east Atlantic, this is attributable to dust aerosol.
- P23300, lines 22–23: This applies only to those studies which applied radiative transfer modelling to derive the DRE. Observationally based studies that rely on measured radiances do not need to make such approximations (Peters et al., 2011; Wilcox, 2012; de Graaf et al., 2012).
- P23301, lines 4–6: The description of the data is confusing. The sentence suggests that the authors use the operationally derived Level3 product by the MODIS science team. However, when going through the publication describing the dataset (Costantino and Bréon, 2013), it is clear that the authors produce their own Level3 data from Level2 cloud retrievals. This should be made more clear at this point.
- P23302, line 15: If retrievals with COT<5 are excluded from the analysis, how can the authors produce the plots in Fig. 6 of the manuscript. These plots span the COT range down to values of 2.
- P23302, line 21: Although used in almost every study using satellite data to quantify aerosol-cloud interaction, it should be mentioned that it is of course an approximation to assume that

AOD measured in cloud-free scenes is also representative for the AOD in cloudy scenes (Anderson et al., 2003). This could be overcome by using AOD retrievals in cloudy scenes from e.g. OMI (Torres et al., 2012).

- P23303, line 4: does "optical depth" refer to AOD ? Make sure the same nomenclature is used throughout the paper. Otherwise the reader gets confused.
- P23303, lines 15–25: I find the description of the model setup a bit too short. Especially, the authors do not go into detail on how cloud optical properties are specified.
- P23304, lines 18–22: It should be noted that similar results are provided in Devasthale and Thomas (2011). Please compare shortly.
- P23305, lines 6–7: Compare to the results of Peters et al. (2011) who find the by far strongest positive DRE in the South-East Atlantic for the time period June – November.
- P23305, line 7: As mentioned above, the authors do not quantify a "forcing" in this study. Please change to "effect" or similar.
- P23307, line 6: see comment above
- P23309, lines 9–11: How much smaller is the AOD derived from CALIPSO compared to MODIS ? How large would the effect on the derived DRE actually be ?
- P23309, lines 12–29: I suggest the authors shift these three paragraphs upwards to directly follow the paragraph ending on "...clouds get involved". This way, a clear separation between results and the contrasting to previous studies would be achieved. This would make reading the paper easier.
- P23309, line 20: What is meant by"global mean value" in this context ? As far as I can see, only regionally mean values are calculated. However, it would be interesting to see how these estimates scale globally, i.e. by setting all other regions across the globe to zero, and see how the values compare to the global estimates presented in Peters et al. (2011).
- P23309, lines28–29: this is trivial and can be deleted. See also again the confusing use of the term "standard deviation" for the most likely non-Gaussian distributions of DRE presented in the paper
- P23311, lines 10–12: I fully comprehended the basis for this sentence after reading it three times and looking at the plots whilst doing so. Perhaps the authors could phrase this differently so that it can be understood when reading it for the first time ?
- P23311, lines 22–23: "...Terra...instead of with Aqua..." ??? This must be a typo.
- P23311–P23312: The authors go very much into detail with a number of shortcomings, which is very nice. However, the possibly most critical one (Haywood et al., 2004; Meyer et al., 2013) is not mentioned (see above).
- Summary and Conclusions: Maybe this is just me, but I like to see acronyms redefined in the summary section. Memory for acronyms may get lost along the way. Furthermore, some readers may want to look at the summary first before reading the whole paper. I leave it up to the authors to decide what to do.

Spelling, grammar, etc

There are a few mistakes due to wrong spelling and grammar. The manuscript is however very readable and the below list is not exhaustive.

- P23296, line 22: larger $\rightarrow$ smaller
- P23307, line 7: necessary $\rightarrow$ necessarily
- P23308, line 6: may be $\rightarrow$ is
- P23308, line 14: different errors $\rightarrow$ differences
- P23310, line 6: Chan $\rightarrow$ Chand
- P23310, line 21: 50 $\rightarrow$ 500 or 550 ?
- P23310, line 24: filed $\rightarrow$ field
- P23313, line 15: hypotheses $\rightarrow$ assumptions/simplifications
- P23313, line 23: see above
- P23313, line 27: gasses $\rightarrow$ gases

References

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