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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Satellite-based estimate of aerosol direct radiative effect over the South-East Atlantic" *by* L. Costantino and F.-M. Bréon

Anonymous Referee #2

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This study provides a new estimate of the Direct Aerosol Radiative Effect (DARE) over the southeast Atlantic Ocean, which is an important region where aerosol-cloud radiative effects cause large discrepancies in models, mainly due our lack of understanding of the fundamental relationships between aerosols and clouds, which is in turn due to the difficulty of obtaining observations of cloud and aerosol properties. The paper adds an interesting new estimate of the DARE, over a larger time span than most existing estimates, with an variable SSA, based on MODIS retrieved Angstrom coefficient (ANG) and presents estimates of the relative position of the aerosols and cloud, which is important for the DARE.

The main criticism is that the paper is not very comprehensive. A new estimate of the aerosol effect is given and compared with existing estimates. It is well embedded in existing literature, but it is correctly noted that it is hard to compare because of the many different assumptions in the various estimates. However, for a complete review of the various DARE estimates and methods the comparison is too brief and a judgement of the improvement of the DARE estimates is not possible from the paper. And this is probably not the purpose of the authors. The authors claim that the purpose is to introduce a better estimate of the DARE using an improved representation of the SSA based on retrieved ANG and better relative position of clouds and aerosols. This would be a real contribution to the field. However, instead of a comprehensive study of the sensitivity of the DARE to the new assumptions (especially SSA), the mere result of the DARE is given and compared to a few different results. The paper would be greatly improved if a study of the sensitivity of the DARE to SSA were given, and a clear description of the MODIS retrieval of the ANG, which is missing at the moment. A brief discussion of the parameters of MODIS is necessary, because the DARE estimates will be rather dependent on the correct MODIS retrievals, which are not guaranteed. E.g. the MODIS COT retrieval will be biased for a retrieval with overlying absorbing aerosols (Haywood, 2004; Coddington et al., 2010) so these cannot be used without correction for a claimed improved estimate of the DARE. Similarly, one would like to know how the ANG is determined, what the retrieval uncertainties of this parameter are, and how they would affect the DARE estimates.

Either the comparisons of different DARE estimates should be complete, or preferably and probably the intention of the authors, the new ideas should be explored and described more extensively. Furthermore, the paper could be structured better and the use of a spell checker might have avoided a sense of haste and carelessness.

I feel that the estimation of the DARE over the southeast Atlantic warrants publication in ACP, but I would suggest to address the issues raised above, and the textual issues I have added below:

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Textual issues:

The abstract starts with a definition of the aerosol effect, incorrectly referred to as aerosol forcing. The consensus is to refer to forcing when indicating the change of a flux due to the anthropogenic fraction of a constituent, (e.g. now minus pre-industrial). The rather sloppy definition in the first sentence refers to the instantaneous change in net flux due to aerosols at TOA, which is better referred to as aerosol direct radiative effect, or following IPCC AR5, the instantaneous radiative forcing. A clear definition of what is meant in the paper is not superfluous, in view of the many confusing mix-ups of the terms.

1 Intro p. 23297

line 22. A few references are expected here.

line 25. Forster et al (2007) refers to AR4, not AR5 p. 23298 line 14-15. I agree that aerosol properties over land from passive instruments over land are less useful than over ocean, but data exist from e.g. MODIS and AVHRR. Also, data from PARASOL, MISR and CALIOP can be used for this, as the authors will be more than aware of. Furthermore, AERONET data is available and used often over land. I would suggest a more subtle discussion of these facts, than the mere dismissal of data over land.

line 16 - p. 23299 line 5 is also discussed in section 3.2 and should be combined with this section, either in the introduction or in the discussion. Section 1.1 is a lonely section. The authors do not explain what they do with it. Even if the SSA is clearly discussed in the next section, the purpose of this section is poorly explained. E.g. SSAc is not mentioned anywhere else in the manuscript.

p. 23300 line 4 souht-> south

line 14-17. The authors claim that desert dust leads to a cooling effect, as opposed to abs. aerosol above clouds. I would guess the authors refer to desert dust aerosols in clear-sky, because desert dust will also have a warming effect over highly reflecting

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clouds, but this is not clear from this sentence. I would suggest to state more generally that abs. aerosols will cool when over dark surfaces and warm over bright backgrounds, and provide references.

line 18-23. The interesting part of this paper is the treatment of SSA variations by using MODIS retrieved ANG. However, the sensitivity of the DARE to this parameter is missing, as well as a description of the MODIS retrieval. The latter may be available in other literature, but the importance of this parameter warrants a rather thorough treatment of the retrieval uncertainties here. The idea of using ANG may be an improvement over previous estimates of the aerosol DRE, but it is somewat disappointing that the sensitivity for the change in SSA is not quantified. If the change in aerosol DRE for a change in SSA would be given, the paper would have been much more interesting.

p. 23301. line 18 According -> Following (or: Based on these experiments)

p. 23303. line 4- 8. These remarks are unclear to me.

line 26- p. 23304 line 3. This sentence is also very unclear. Please, state more clearly what you mean. Only discuss the absolute position or only the relative position of aerosols and clouds. To interchange them within one sentence makes it unnecessarily difficult to understad what is meant.

line 11. Start a new paragraph before 'Ocean'

p. 23304. Please, provide the figures in the same order as presented in the text, and number them accordingly.

p. 23305. line 5. I hope 'mixed case' is an error here and 'aerosol above cloud' is meant. Otherwise this discussion doesn't make any sense to me.

p. 23306. line 13. high -> highly

line 26-28. I don't see where this happens. It should be substantiated with evidence or explained better. Is this for all cases true? Or when the clouds and aerosols are close

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together and shift from mixed to unmixed or vise versa? Do aerosols 10 m over clouds have a very different effect than aerosols 100 m above a cloud? Then how much? And this is surely not true for aerosols under clouds..?

p. 23307 line 7. necessary -> necessarily

p. 23308 line 20-22. The results of Chand et al. are not weighed for cloud fraction, so the discrepancy should be fairly large. A proper description of the differences should be given here, and all comparisons may be grouped together, like has been done in Table 1. It would be helpful if the comparison with the various estimates of the DARE is restricted to one paragraph, e/.g. as a descriptioin of Table 1. An additional estimate of aerosol DRE over clouds can be found in De Graaf et al, 2012, where the effect is independent of any aerosol property estimate, and closer to the large numbers presented in this paper.

p. 23309 line 12 There is no Table 2!

p. 23310 line 4. -4.50 is not close to 5.5. or 5.0. Do the authors mean -5.5 and -5.0!? Section 3.3 Please, change all references to Chan et al to Chand et al.

line 21. 50 nm??

p. 23311 line 8. change 53 549 -> 53,549

p. 23312 line 3. sensible -> sensibly

references Haywood, J. M., S. R. Osborne, and S. J. Abel (2004), The effect of overlying absorbing aerosol layers on remote sensing retrievals of cloud effective radius and cloud optical depth, Q. J. R. Meteorol. Soc., 130, 779–800.

Coddington, O. M., P. Pilewskie, J. Redemann, S. Platnick, P. B. Russell, K. S. Schmidt, W. J. Gore, J. Livingston, G. Wind, and T. Vukicevic (2010), Examining the impact of overlying aerosols on the retrieval of cloud optical properties from passive remote sensing, J. Geophys. Res., 115, D10211, doi:10.1029/2009JD012829

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