

Interactive comment on “Aerosol indirect effect on warm clouds over South-East Atlantic, from co-located MODIS and CALIPSO observations” by L. Costantino and F.-M. Bréon

Anonymous Referee #1

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Review of paper:

Aerosol indirect effects on warm clouds over south-east Atlantic from co-located MODIS and CALIPSO observations. by L. Costantino and F.-M. Bréon

Strength

- nice introduction and background material (exploring expected relationships) - unique concept, simple to follow plots, clear results, good analysis linking pieces and previous work

Weaknesses

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- limited clarity on regional coverage and consideration of seasonality - somewhat inappropriate seasonal definition for southern African biomass season - larger region required for better statistics dilutes effects of biomass over stratus (sub-region) - unclear of AI determination (with MODIS) over overcast stratocumulus decks (neighboring data?) - limited efforts in error checking

General comments

Let make it clear from the start that this is a great paper pulling observational evidence from satellite data to provide insides and constrains for aerosol cloud interactions. The paper investigates satellite observations to address potential aerosol cloud interaction impacts over the south East Atlantic, where, at least during the Aug to Oct time-period elevated aerosol layers from wildfires in southern Africa are advected over lower altitude stratocumulus fields. The way the aerosol interacts with low level clouds depends on the relative altitude between aerosol and clouds, that is if aerosol is well above the cloud in a separate layer or interacting with the clouds by vertical co-location. These two cases, based on vertical profiling by the space-borne CALIOP lidar, are separately investigated on a statistical basis by comparing MODIS derived cloud properties at CALIOP co-location event with a spatially co-located MODIS derived aerosol index (AOD*Angstrom), which is used as a proxy for CCN. The results are nicely summarized in many plots, clearly demonstrating differences in relationships for cases interaction (by co-location) and for non-interaction between aerosol and clouds. I just wonder about stratifications into seasonality and sub-regions, considering changes in aerosol loading and type as well as changes in cloud cover and type (e.g. how different are results when moving from overcast to closed cells to open cells and eventually to trade wind cumulus? Minor problems (see below) should be fixed or clarified ... otherwise a strong recommendation for publication.

Minor comments

Page 3 “experimental evidence” ... maybe better “observation-based evidence” and

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“drives” is awkward . . . maybe “can illustrate”

Page 4 I am not convinced that smoke particles have solubility that is (relatively . . .to what)? high.

Page 8 the derivation of the relationships in equation 9 is not quite clear (sub-steps would help)

Page 8 “use used”

Page 9 what is done to detect AOD above the stratocumulus, if there are not cloud-free scenes (as we would expect for stratocumulus decks), which are required for MODIS AOD retrievals?

Page 9 significant uncertainty to the microphysical cloud properties (reff, COT, LWC..) can be expected not only by inhomogeneity but also by the presence of absorbing wild-fires aerosol above those clouds. I hope the discussions address this issue. Cloud top altitude overestimates (in case inversions) should be discussed also in the context of CALIOP data.

Page 10 “than in . . .?” (upper troposphere?) Page 10 I wonder on cloud-top agreement by ground-based lidar (must be thin clouds)

Page 11 clear-sky cases are not considered (as there are no cloud data). Alternately are completely cloudy cases also thrown out, because there are not aerosol data, or are then aerosol data ‘borrowed’ from neighboring grid-points? Filtering data may introduce biases and limit the value of results.

Page 12 there are a lot of cases that are excluded for focus. Maybe a simple graph can illustrate the selection. And with so many exclusions I wonder if the remaining cases are sufficient for good statistics.

Page 13 I am not quite sure why so much detail is needed about the larger region even north of the equator when only the area off Namibia and Angloa is of interest (this

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section cloud be shortened) . . . or do we address in the analysis the Gulf of Guinea as there are not significant cloud amounts below most aerosol.

Page 14 interesting figures.

Page 15 I am not so happy with the seasonal choices. The biomass burning in southern Africa (e.g. Zambia) that is largely responsible for the elevated larger AOD values off Namibia is between August and October. Thus, the selected seasons are unfortunate.

Page 15 ‘figure 2’ should be ‘figure 3’

Page 16 I wonder about the likelihood of MODIS aerosol retrievals in the presence of stratocumulus decks. How much is this a contributing factor for the low AI values off Namibia for Oct-Dec? Similar are the Jul to Sep values largely defined by Jul values, as much fewer samples are expected for Aug and Sep?

Page 17 Is Figure 5 for data of the entire year or just one (biomass) season?

Page 17 ‘-14E’ is strange . . . suggest to use ‘14W’ (similarly later)

Page 18 I assume that the LWC investigation for the same (2S-15S, 14W-18E) region .. the text only mentioned latitude boundaries.

Page 18 can we speculated on the LWC reductions (e.g. loss of water by drizzle or dry-air entrainment)?

Page 19 The 4N-30S (and I assume 14W-18E) is much larger. I am in doubt that this larger region is really relevant due to the low frequency of cases of aerosol within low level clouds outside the core region. In addition, the statement confuses. There is little interest, if there is a higher value at AI=0 (this would be expected since now more tropical regions are included) but if there is a change in slope.

Page 20 delete ‘A’ before ‘statistical’

Page 21 ‘computed’ → ‘compute’, what is the ‘whole SE Atlantic’ . . . be more specific,

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figure → Figure 8

Page 22 captions in Figure 8 and 9 confuse with northern latitudes ! must be wrong.

Page 24 By using the larger area in order to increase stat significant, the focus on 'aerosol above stratus' cases is weakened.

Page 25 'for WHAT is very large'

Page 27 the separation into COT regimes is smart to address potentially delayed precipitation. On the other hand the COT class frequency for mixed and un-mixed clouds would be nice addition. I also would like to know, who this graph would look like for the smaller focus region (2S-15S) in the Aug-Oct frame. (In that context I am still confused as to the time-period of the data considered in the most plots. I know that they are multi-year . . . but are there considerations of seasons (or even months)?

Page 28 Figure 12 is very important. Maybe it is possible to offer this plots by season and then sub-sample regions (and seasons?) with relatively high frequency of low altitude stratus.

Page 32 'in function' → 'as function'

Page 33 'present' → 'presence'

Page 34 'precesses' → 'processes'

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/12/C5656/2012/acpd-12-C5656-2012-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 14197, 2012.

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