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Atmospheric Chemistry and Physics Discussions

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 #2

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This paper finds associations between MODIS aerosol and cloud parameters after employing CALIOP information to sort the MODIS data into two populations: situations in which aerosol mixes with warm clouds and situations in which aerosol remains above and separated from the cloud deck. A broad set of relationships are examined including cloud droplet radius (CDR), liquid water path (LWP), cloud optical thickness (COT), cloud fraction (CLR) and cloud top pressure (CTP) as a function of aerosol index (AI) where AI is defined as aerosol optical depth multiplied by aerosol Angstrom exponent. Also, CDR and LWP are examined as functions of COT. The paper focuses on the well-studied region of the Atlantic that is off the coast of central and southern Africa, where biomass burning smoke is known to often overlay a low altitude deck of warm clouds.

The paper approaches the task systematically with a great deal of thoughtfulness. The division of the data into situations with mixed and unmixed populations is new and provides very informative results that lead the authors to interesting new interpretations of aerosol-cloud interaction. This could be an excellent paper and a major contribution to the community.

There are a few problems, the most important being the need to better address the possibility of a co-variance of meteorology with aerosol properties.

Also, while the paper is overall very well-written in terms of structure and basic grammar there is an underlying low-level weakness in matching plurals, articles, parts of speech etc. It is too ubiquitous for me to call out each occurrence in a separate document, but if the authors and editor request, I would be happy to take the text in a Word document and edit the document for English. Right now, I am going to ignore the English and focus on the science.

1. Just because mixed and unmixed cases occur in the same geographical location on different days does not mean that the meteorology is the same at that location on the different days. The fact that on some days the aerosol mixes down into the cloud and on other days it stays lofted above is evidence that the meteorology is fundamentally different in the two populations. References to the incorrect interpretation of similar meteorology start from line 8 of the Abstract, and continue on line 14 of page 14201 and line 19 of page 14218.

There are two places in which meteorology can confound results. Meteorology, not aerosol height, may explain the different responses of the two populations to increasing aerosol loading. Or, meteorology not aerosol can explain the changes to cloud properties seen with increasing AI. Personally, I believe in both cases the interpretation given by the authors is correct and their results are dependent on aerosol properties not meteorology. However, my personal belief is insufficient for a published paper. It is the burden of the authors' to give us some evidence that the associations we are

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seeing are not confounded by co-varying meteorology or at least rise above the natural variability expected in the absence of aerosol. It is very hard, because if the smoke layer aloft is changing temperature and stability, we have a 'chicken and egg problem.'. Which came first the different stability or the smoke?

I'm not looking for an absolute proof, because that is impossible. I am looking for a retraction of the statements suggesting that the problem is even a wee bit solved by finding the two situations in the same location, a better statement of the overwhelming difficulty of separating results from meteorology and at least some evidence in support of the authors' interpretation of the situation.

2. The authors state on p 14206, "On the other hand, 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 (Haywood et al., 2004)." I was not familiar with this reference and have always thought that absorbing aerosol above a cloud will indeed create a negative bias in retrievals of COT. Wilcox et al., (2009) supports my understanding. The case is certainly not closed. If the MODIS product is affected by absorbing aerosols above clouds, then several results found in this paper need re-examination with the possibility that a MODIS artifact could be contributing to the variation of cloud product with AI.

Wilcox, E. M., Harshvardhan, and S. Platnick (2009), Estimate of the impact of absorbing aerosol over cloud on the MODIS retrievals of cloud optical thickness and effective radius using two independent retrievals of liquid water path, J. Geophys. Res., 114, D05210, doi:10.1029/2008JD010589.

3. The discussion of precipitation and the 2 regimes of thinner and thicker clouds relies on several plots that are not shown. Why not? There is no plot of LWP vs. COT, although that relationship is referred to several times through Section 4.5. It seems to me to be an especially important figure and it should be included. If there is a need to reduce the number of figures (I don't see why) Fig. 2 and Fig. 12 are less important.

Furthermore, in Section 4.5 the authors transition from using the terms mixed and unmixed to the terms polluted and clean. This is counterproductive. Mixed and unmixed span the AI range from 0.03 to 0.5. There are polluted and clean cases in both mixed and unmixed situations. To make sense of the data, the analysis should include dividing the data set into 4 categories: mixed and COT < 10; mixed and COT > 10; unmixed and COT < 10; unmixed and COT > 10. Then to bin by AI and see how CDR and LWP react to aerosol loading in each of the four situations. If there is not enough data to divide it into 4 and bin, then look at high and low AI in each of the 4 categories. This really needs to be done before conclusions like "polluted clouds rain less than clean clouds" for thicker clouds (p. 14223 line 19).

4. The data show almost no dependence of COT with AI, despite the fact that CDR is strongly dependent, following Twomey's theory. The authors' explanation of varying LWP is adequate explanation and probably correct. However, I propose another way of looking at it. The authors suggest that the aerosol above the clouds in the unmixed case is absorbing enough to raise the temperature in that layer and change the atmospheric stability. Why couldn't the aerosol embedded in the clouds themselves be dark enough to decrease the visible reflectance, which in turn would decrease the retrieval of COT? This was Kaufman and Nakajima (1993)' s explanation when they found signal in the CDR but not the COT for clouds in the Amazon.

Kaufman Y.J., and T. Nakajima (1993). Effect of Amazon smoke on cloud microphysics and albedo - Analysis from satellite imagery J. Appl. Meteor. (Squires special issue)„ 32, 729-744

5. The discussion of 'cloud lifetime effect' (p 14222) should be handled with great caution. In these single snap shot images there is no real information on cloud lifetime. I would rather leave the explanation unstated than to venture into a speculation with little evidence. At least there should be acknowledgement that the cloud lifetime effect is highly speculative, especially since we see the opposite to Albrect's hypothesis with regards to LWP.

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6. I cannot manage to derive Eq. 9 from Eqs. 6, 7 and 8. No matter what I try, I end up with

CDR proportional to COTˆ0.2 and Nˆ-0.4

The starting equations may have a mistake, or the authors may have introduced an algebraic mistake in the derivation or I may have made a mistake in mine. In the end it doesn't matter because the relationship that is used in the paper is the one between CDR and COT and the error is in the exponent of N. But the authors should check for errors

Minor error: Section 2.1. page 14206, line 6. MODIS uses only 6 channels over ocean to derive aerosol. The 0.47 μm channel is not used.

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Interactive Comment