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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The invigoration of deep convective clouds over the Atlantic: aerosol effect, meteorology or retrieval artifact?" *by* I. Koren et al.

I. Koren et al.

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C: I am afraid that I cannot agree with the enthusiasm of the other reviewer. While I think Koren et al are looking at an interesting part of the world, and it is indeed an interesting subject, I find the paper has many major shortcomings which I believe require an entire rework. After doing so I think they will have a stronger paper worthy of citation.

A: We thank the reviewer for a careful review and his/her suggestions. As a result of these suggestions we have made several modifications and have added another section. In some ways we feel that we failed to communicate the main point of this paper and misled the reviewer. The intent of this study is to directly address the criticisms aimed at satellite-based studies of aerosol-cloud interaction. It is not meant to quantify the aerosol effect, which is addressed in a separate paper (Koren et al., 2010) that applies the methods proven here to larger areas in the Atlantic and Pacific tropical regions.

C: The paper starts strong with a reasonable (if optimistic) survey of satellite-aerosol cloud findings and methods. However, the paper quickly degenerates into a multi-regression fishing expedition with no meteorological context and even less statistical strength. They examine a mere 2 months of data (July and August 2007) for a ten degree box along the equator (0-10 N; 20-30 W). They do not explain why this area and time period was chosen. It is just south of the typical easterly wave/SAL track, as well as the ITCZ. Looking at TRMM satellite products online, looks like the area of precip is very thin and at the very north of this area. Given this is hurricane season, this precip is mostly modulated by easterly waves. How does all of this fit into the meteorological picture?

A: The reviewer is correct in pointing out the lack of meteorological context in the original paper. We have added Section 1.2 and Figure 1 to remedy that omission. This section discusses the study area from both a meteorological and aerosol perspective, explains why the area was chosen and shows graphically three different typical scenarios experienced during the study. The comment about the "multiregression fishing expedition" ignores the intent of the study, which is to show the orthogonality between a meteorological tracer associated with the convection and one associated with the aerosol. Moreover we did not use multiregression in this study and in page 3908 we explained: "After finding the meteorological variables that have the most statistically significant correlations with the measured cloud properties, the meteorological variance will be restricted by limiting the allowed range of the selected variables. Since convective systems are expected to have a well-defined set of meteorological properties (Jakob and Tselioudis, 10 2003), such a direct approach is preferred to a multi-regression analysis (Kaufman et al., 2005a) where many meteorological variables are mixed together to completely span the meteorological variance space. The multi-regression approach may yield better overall correlations with the free (convective cloud) variables but when mixing many different variables with different units one may lose the physical reasoning and the likelihood of statistically insignificant correlations appearing is higher. " The new section describing the meteorological context and the statement of intent leading into this section, should allay the reviewers' objections.

C: Based on their area of interest, they can observe periodic dust outbreaks as well as pollution/biofuel/smoke from central Africa. This yields the variability in fine mode fraction observed in Figure 2. But each of their AOD regimes (very clean <0.06, moderate (0.11), and heavy (0.23)) likely has a very specific transport pathway and corresponding meteorological pattern.

A: This is now investigated. The heavy aerosol category includes a few situations in the beginning of the study period with dust intrusions, but is mostly dominated by smoke aerosol from the south and east. A new Figure 1 illustrates the different aerosol scenarios. Using fine mode fraction at AODs in the very clean and moderate categories is insufficiently accurate to differentiate aerosol types. The MODIS algorithm reports fine fraction for all AOD ranges, but the retrieval loses sensitivity to particle size for AOD less than 0.15. The very clean category is obviously background marine aerosol (sea salt, DMS and fine organics). The other two categories involve a combination of marine with rarely dust and more often smoke. Section 1.2 discusses the different aerosol transport paths.

C: The authors very quickly say aerosol particles are not associated with other meteorological features associated with cloud development. Well certainly, if one has a background marine airmass, RH will be very high, whereas a SAL intrusion will be dry.

A: In our study period and region, the SAL intrusions are rare. The smoke may also be associated with dry air, but as far as we can tell this association has not been previously documented. In any event, the convection is associated with higher RH and the aerosol is associated with lower RH. This only strengthens our point. If it were

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all connected then we would expect more aerosol to be associated with drier air and weaker convection. But that's not what we see. We see more aerosol associated with invigorated convection. The aerosol association is working counter (or orthogonal) to the meteorology.

C: Also, area of active precipitation scavenge particle.

A: Following the reviewer's premise, the more invigorated the convection, the more precipitation, the greater the scavenging, the less aerosol. Again, less aerosol is associated with more convection, which is contrary to what we see. We see more aerosol associated with greater invigoration of convection, which supports the argument in the paper.

C: But they are then dismissive of any other meteorological transport features. Indeed, any discussion of meteorology is very minimal. For each of your regimes, is there a corresponding EOF? How about periods of high AOD and high and low fine mode fraction? Is it smoke coming from central Africa or Saharan dust? What do model soundings look like for these cases?

A: We agree that the original paper lacked discussion of meteorological transport features, and we have added a detailed meteorology section to the introduction and a figure (new figure 1) to remedy that omission. The intent of this paper does not warrant further investigation of transport features or EOF analyses. The AOD values are too low in our study box to make detailed fine mode fraction analysis. Because of the reviewer's suggestion below, we have calculated two-dimensional histograms of AOD and fine fraction. These appear in the paper's new Figure 3. In these histograms we see the broad distribution of fine fraction at low AOD, demonstrating the lack of sensitivity in the retrieval and the inability to distinguish differences between aerosol types except when aerosol loading increases. The MODIS aerosol fine fraction product is a tool that can aid in determining aerosol type but it requires additional baseline information (Kaufman et al., 2005b and Yu et al., 2009) and sufficient aerosol signal.

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C: The next issue I have is on the cloud definitions. How are the authors differentiating cirrus tops associated with high pressure with cirrus on anvil tops from individual storms? For low AOD (clean marine), the normalized histogram shows no high clouds, whereas the moderate to higher AOD do show clouds at 200 mb. How do we know the nature of these clouds? From the paper, I can't tell if they have convective origins. Also, using an "average cloud top pressure" is a very misleading statistic in a three modal system.

A: One of the reasons for selecting the research area to be over the center of the Atlantic ITCZ is to reduce the likelihood of cirrus clouds that are not linked to convective systems. This would have had the potential to be a problem if we were to work over the subtropics or mid-latitudes. In the tropics the pressure is mostly low and therefore specified by a convergence zone. To be sure we visually inspected the MODIS images and found that in almost all cases there is a clear association between convective cells and anvils.

C: Speaking of statistics, the authors here again fail to make a compelling case. It is a limited areas of only a few months and is really a multivariate "pour in data and stir" method. They do not even list how many cases they have in each of their AOD categories. A histogram like figure 3 for AOD would be good. A three dimensional plot of cloud to pressure and regional average AOD would be best and would likely go a long way to explaining what is going on. Without physics and meteorological analyses to back studies like this up, the statistical conclusions are at best ambiguous, at worst downright dangerous.

A: We explicitly avoided multiregression analysis in this study (please see our answer to comment #2). What we did is examine the correlation between MODIS measurements of cloud top height and cloud fraction with each of the model variables separately and found which model variables correlate best with the satellite-derived convective cloud metrics. This is quite different from a multivariate regression. The results were shown to be robust with highly significant statistics. In line with the reviewer's suggestion we

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now add 2-D histograms of the AOD and fine fraction before and after the filtering. The 2D histograms demonstrate the effect of filtering out AOD pixels with high cloud content.

C: The use of GOCART in this study is a bit odd. Given the narrowness of the impact between middle and high AOD, I am not sure the free running GOCART model can be of that much help. They should prove for this period of time that GOCART is generating adequate analyses that make sense.

A: We offer GOCART as a completely independent estimate of the aerosol loading to support our argument that the relationships seen in the satellite analysis are not solely caused by cloud contamination of the retrieved aerosol products. GOCART data is less accurate than the MODIS retrievals and subject to many uncertainties, but these uncertainties differ from those affecting the satellite retrieval. Given that two independent measures of aerosol loading are associated qualitatively in the same way with invigorated convection gives the papers' assertions greater validity. Quantitatively, the associations differ. The MODIS AOD product on a day-to-day basis is more accurate than GOCART. Therefore, the quantitative relationships between cloud parameters and aerosol should be ascribed to the MODIS product and not to GOCART. The text in section 3.2 and section 4 (discussion) is revised to clarify this point. We added: "Use of the GOCART-modeled AOD output has the advantage of removing any concerns of cloud contamination in the reported AOD. In this case, correlations of CFR and CTP with AOD show similar trends with larger magnitude, reinforcing the results using MODIS-derived AOD. When the AOD changes from 0.05 to 0.28 the cloud tops are higher by 330 $\pm$ 30 hPa and the cloud fraction is significantly larger (0.4 $\pm$ 0.1). GOCART model output is less accurate than the MODIS retrievals and subject to many uncertainties, but these uncertainties differ from those affecting the satellite retrieval. Given that two independent measures of aerosol loading are associated qualitatively in the same way with invigorated convection lends validity to the assertion that the correlations between MODIS-derived aerosol and cloud products in this region and season of interest are

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not due to artifacts of the MODIS aerosol retrievals. "

C: Bottom line, is that this is a good idea but quickly and carelessly executed. If real meteorology and statistical rigor were folded in, it would be suitable for publication. This will take some time to put in.

A: We again thank again the reviewer for his/her comments but have to disagree with this conclusion. We apparently failed to convey the main points of the paper and we have now made them clearer (especially the discussion on the meteorology). We emphasize again that in this study we directly tackled the strongest criticisms leveled at satellite-based studies of aerosol-cloud interaction and we have not attempted to quantify the aerosol effect. We trust that with the new additions the paper is now clearer.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 3893, 2010.