Atmos. Chem. Phys. Discuss., 9, C7912–C7916, 2009
www.atmos-chem-phys-discuss.net/9/C7912/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Statistical properties of aerosol-cloud-precipitation interactions in South America" *by* T. A. Jones and S. A. Christopher

Anonymous Referee #2

Received and published: 4 December 2009

Overall, this is a decent paper. The authors are grappling a difficult topic, and make a credible effort at retrieving a small signal from a very noisy system. PCA is a novel approach for inferring the effect of aerosols on clouds. I believe that the analysis is reasonable, but streamlining the presentation would produce a stronger paper. As I read the paper, I had trouble following the main point, and sometimes I was not sure where the authors were taking me. For instance, the authors spend a fair bit of time discussing the lack of correlation between aerosol and cloud properties in section 4.1. I think that this discussion could be significantly reduced, since most readers don't expect an obvious correlation between any two aerosol and cloud parameters in this complicated system. Further details are provided below.

The authors seem to be looking signs of the 2nd indirect effect in the data set, but

they chose a biomass burning month. Their PC analysis doesn't indicate evidence of a relationship between rain rate and their PC components, but the authors suggest that this may be because of the semi-direct effect. Perhaps the authors would have better luck finding the 2nd indirect effect in a different month of the year.

Significant issues:

The authors demonstrate several weak correlations, but they do not provide correlation coefficients for the reader. Figures 3,5,and 8 should provide an R-squared value for each figure. Most of the figures (all of them) shouldn't even include a linear regression line, as the correlation appears to be so poor that any trend indicated by the regressions is unreliable. Also, the authors often speak in the text as though the sign of the correlations is significant when the correlation is weak; some of this is demonstrated on page 16 in lines 1-6. Another example of this issue can be found on page 24, line 5, where the authors state that AOT and Rc are positively correlated overall; earlier in the paper they state that the correlation was not statistically significant, though. This sort of narrative tends to defocus the reader from what the data is really telling us (i.e., that the variables are uncorrelated).

page 10, lines 11-29: This section is a bit misleading, as it indicates that the additional scattering associated with aerosol humidification should not be associated with AOT. Most non-desert aerosols are humidified, though, albeit some are more humidified than others. Humidified aerosols are still aerosols, and the additional scattering produced by humidified aerosols is still appropriately part of AOT. The authors state that the magnitude of the humidification enhancement is 13-11% when compared to AERONET, but sun photometry measurements also include the effects of aerosol swelling. Then again, perhaps the authors are speaking of *activated* aerosols, which one could argue should be separate from AOT. Differentiating between these two types of aerosols is not possible in partly cloudy regions via remote sensing (not even with sun photometry); I understand this to be the point of Koren (2007). At any rate, this section needs some clarification.

page 11, lines 3-6: the authors state: "However, Wen et al., (2006) observed that this phenomena is only occurs on a spatial scale of a few kilometers. Since MODIS derived AOT at 10km (and we use AOT data that has been remapped to a 20 km resolution), this effect will not be resolvable in the MODIS data used here and should not significantly impact the interpretation of the results."

But on page 9, line 26, they also state: "MODIS products are derived from cloud-free 500m resolution data (20x20 pixels) and aggregated to 10 km footprint used by the Collection 5 MODIS level 2 aerosol product (MOD04)."

Since the 500m AOT is susceptible to the "bluing" effect, why should the effect disappear in the 10km aggregated field? That is, if the 500m AOTs are spuriously high, the 10km avg of the 500m product will be spuriously high as well. The authors need to discuss how this issue may impact their results.

page 11, line 16: Potential temperature is a better indicator of stability than temperature – why not use potential temperature?

page 15, line 21: The authors state: "Without considering precipitation, aerosol effects should still manifest themselves as significant correlations between AOT and certain cloud properties such as Rc, COT, and CTP."

Why? As the authors demonstrate throughout this paper, there are many parameters affecting the microphysics of clouds. Why would we expect correlations with any single aerosol parameter? It would be better to quickly summarize this section with a table showing the poor correlations (and omit fig 3) and move quickly to the main point of the paper – the PCA analysis.

page 18, lines 4-7: correlation coefficients should be included in figure 5, and would be more informative than the least-squares lines drawn through the scatter plots. it is not obvious from them that any of the x-y relations in that figure should be statistically significant. Maybe even replace this figure with a table.

page 19, line 20: "weakly positive" needs to be quantified. If the correlation is indeed small, how confident are you in the sign of the correlation? Some numbers can help the reader put this into perspective.

pages 20-21: The authors spend a fair bit of time discussing some very high PC components that explain < 5% of the variance. I would limit the discussion to PC components less than PC3 or PC5, especially since none of the PC components are correlated with rain rate. This discussion of the higher PC components defocuses the reader.

page 21, line 23: Which other months were analyzed? Other Septembers, or different seasons? This needs to be stated in the paper. Also, September is biomass burning season in much of South America, but this is not discussed at all in the paper. You'd expect a stronger semi-direct effect in September than in other seasons. Some discussion on this in relation to your results would be useful.

page 26, line 28: the authors state: "We find that the radiative effects of absorbing aerosols outweigh the microphysical effects when reducing the probability for stratiform precipitation." It is not clear to me that they demonstrated this.... need a better focus.

Tables 1 and 3: The caption talks about 'variables in italics,' but there are none. There are variables in bold that are not explained.

Table 2: Remove all 0.00 from Table 2 (i.e., leave those spaces blank). This will enable the reader to quickly see where the action is at. Also, an additional row at the top of the table indicating % variance (from fig 2) would be helpful for keeping the PCs in perspective.

Minor issues: page 7, line 1: AOT is fig 1a, not fig 1b. page 7, line 2: why not mention that precipitation is shown in Fig 1b, here? page 10, line 10: I don't understand the relevance of this sentence page 16, line 23: should be table 1, not table 2.

page 17, line 15: The authors state: "Breaking down precipitation into stratiform and convective components, we find that monthly mean convective rainrate is much greater the stratiform rainrate (0.7 vs. 3.3mmh-1)."

Conventionally, the first number in parenthesis should correspond to the first variable mentioned in the most recent part of the sentence (in this case, the convective rain rate). However, the first number is smaller than the 2nd number, which is opposite of what is stated in the sentence.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 21463, 2009.