

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

T. A. Jones and S. A. Christopher

tjones@nsstc.uah.edu

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Response 2:

General Comments:

We greatly appreciate the reviewer's comments and constructive criticisms and have modified the manuscript accordingly. Major changes include additional discussion of PCA methodology and how uncertainties within the raw data are taken into account. Correlation statistics have been added in the results and in table form to better assess the significance of the results. Finally, the discussion of higher order PC variables has been substantially reduced as their physical significance was questionable, as the reviewer noted.

Significant Issues:

Correlations: Additional correlation statistics have been added as suggested in a new Table 2, replacing the old Figure 3. We agree that overall these correlations are poor and plotting best-fit regression lines could be misleading. As such, these lines have been removed from all figures. Scatterplot figures that remain now include the correlation coefficients between the parameters being plotted. Additional discussion of the correlation values themselves has been added to the discussion. Section 4.1 has been shorted somewhat as suggested since nearly all the single parameter correlations are low and have marginal physical meanings. The discussion concerning the spatial distribution of these correlations and how they relate to stratiform vs. convective precipitation was retained in a somewhat revised form.

P10, lines 11-29: The discussion of aerosol properties nearby clouds has been improved and clarified. Specifically, we now note that the increase in AOT nearby cloud noted by Koren et al. [2007] is a result of both humidification of aerosols and non-detected cloud droplets (newly activated aerosols) in the vicinity of clouds. We also note that this difference is calculated by comparing AOT from confidently clear regions relatively far away from the cloud edge to those nearer the cloud edge, which are still assumed to be clear-sky by the retrieval algorithms.

P11, lines 3-6: The MODIS AOT algorithm computes AOT for a 10x10 km region using cloud-free 500 m pixels within that region. As part of this process, the 25% highest and lowest reflectivity pixels are removed prior to retrieving AOT. The removal of the high reflectivity pixels will also remove those associated with spurious scattering caused by nearby clouds. Koren et al., [2007] and others observed that this effect was small compared to the humidification / activation effects described previously. As a result, spurious scattering should not adversely effect the overall interpretation of the results.

P11, line 16: Yes, potential temperature alone is a better measure of stability, but adding this parameter to the already present temperature and humidity information did

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not a significant amount of “new” information to the overall data set. Thus, we decided to exclude potential temperature and other derived parameters from the analysis.

P15, line 21: “Why AOT should be related to cloud parameters. . . why show a correlation” This statement was meant to imply that a relationship between AOT and cloud properties should exist under an idealized scenario whereby other factors such as changing atmospheric conditions are held constant. This has been clarified in the text along with an added note that this research does not represent such a scenario and that these relationships may be outweighed by other factors.

P18, lines 4-7: Correlation coefficients are now included in this figure (Fig. 4) and the best-fit lines have been removed.

P19, line 20: “Weakly positive” in this case refers to a value of 0.13, which is now stated explicitly. Correlation coefficient values have been added in several locations throughout the text when discussing the physical interpretation of the relationships between various parameters.

P20-21: Discussion of higher order PC ($PC > 6$) has been shorted significantly and is now summarized in a single paragraph in section 4.2. Corresponding changes to the conclusions were made as well. As result of this, the spatial plot of PC7-12 in Figure 7 was also removed.

P21, line 23: By “other months” we were referring to August and October. However, this statement was removed when reducing the discussion of higher order PCs.

P26, line 28: The statement beginning with “We find that. . .” has been revised to note that the results shown here support this hypothesis, but cannot outright prove that semi-direct effects are associated with PC2. However, our results combined with observations from previous studies strongly indicate that we are indeed seeing a physically significant signal. This is now stated more clearly in the conclusions portion of this manuscript.

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Tables 1 and 3 (now 4). The italics highlighting was meant for those variables shown in bold print. This has been corrected. Units have also been added to Table 1.

Table 2 (now 3). The percentage of the variance explained by each PC variable has been added, while “0.00” values have been removed.

Minor Issues: Corrected as suggested.

[Interactive comment on Atmos. Chem. Phys. Discuss., 9, 21463, 2009.](#)

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