

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

Anonymous Referee #3

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The implementation of PCA to decipher the interdependencies of variables associated with indirect radiative forcing by aerosols appears to hold promise, especially when compared to other techniques that are useful in providing correlations but can say little about causality. It was in this spirit that I read the paper. However, there were obvious shortcomings and questions that arose that need to be clarified in order to make the reported results more meaningful.

Before listing the weaknesses, it should be noted that the authors did an admirable job of explaining why they were using PCA to find the relationships among variables, for example cloud optical thickness and rain rate. They showed that there were few statistically significant correlations between a raw variable and rain rate. However, without providing correlation coefficients it was not clear how well the linear regression worked,

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especially since most of the scatter plots did not exhibit clear linear relationships.

On some of their conclusions drawn from the physical interpretations of PCs, they did not make solid connections. Were there supposed to be two values per variable in tables 1 (units are missing!) and 3? The background discussion of PCA was not sufficient for a reader unfamiliar with PCA to understand its utility. At least some of the basic equations could be presented.

It is understood that they wanted to set the small eigenvector weightings to 0, but it would have been helpful to understand why they chose 0.2 as the limit, especially when there were weightings that weren't far above 0.2. They also should have explained why they thought 1% was an acceptable noise baseline for the variance contribution.

What they attempted to do with the relative magnitudes of the PC weightings was fine, but it is not clear that they can interpret the signs of the weightings so literally. For example, is it really true that negative values on the eigenvectors for winds implied negative directions of the wind velocities? It is fine to say that variables with opposite signs have opposite effects on the PC. Were the signs of the PCs changed, such that the signs would make sense with their interpretation?

Because their explanation of PCA was lacking, it was not clear why all of the weightings of the PCs (or scores, shown in Figures 6 & 7) were bounded between -2 and 2. The standard methodology does not constrain weightings, so presumably they were scaled somehow? And because they didn't discuss how they derived the weightings, it was unclear if the eigenvalues were bound between -1 and 1 or not. If they were not, then they are measures of how far away from the mean each raw variable is on each PC.

In general, I would prefer to see a nearly complete revision and reorganization of the paper, with more discussion of some of the details of PCA, and then on the limitations in their interpretation in order for a reader to understand the conclusions. I think some additional discussion is warranted on the data used. For example, some discussion on uncertainty and its impact on PCA would help. Also, cloud and aerosol properties

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are not retrieved simultaneously from satellite. What impact does this have or how is it mitigated? I began a list of minor comments which I will do not list here; suffice it to say that there are numerous grammatical errors and stylistic points which make the paper difficult to follow. I would recommend the authors have it carefully proofed before resubmitting.

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

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