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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Relationship between Amazon biomass burning aerosols and rainfall over La Plata Basin" *by* G. Camponogara et al.

Anonymous Referee #2

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It is well documented that biomass-burning activities in Amazon during dry season produce a considerable fraction of global biofuel-emitted aerosols. Whether this large quantity of smoke aerosols had already affected downstream cloud and precipitation is an interesting science issue. This paper reports an attempt to establish statistical correlations between Amazon smoke aerosols and rainfall in downwind La Plata Basin. For this purpose, the authors have used AOD data from AERONET stations in the region of interest from 1999-2012, along with TRMM rainfall data and reanalysis meteorological data. Statistical methods including the AOD-rainfall bin analysis and the empirical orthogonal function (EOF) analysis have been applied.

The topic of the paper is definitely suitable for the ACP, and its conclusion would interest the ACP readers. The authors have put an effort to address the typical issue that a

data based analysis of aerosol-rainfall correlation has to deal with, that is to identify the causal relation rather than simple correlation. However, I have several concerns about the lack of certain depths in the paper and would like to see the authors to address them before the paper could be accepted for publication.

It is commonly known that AOD as an optical measurement does not always reflect the variation of aerosol number concentration. The latter is a microphysical measure and the major factor that shapes the foundation of the aerosol-cloud interaction or aerosol indirect effect. This study uses AOD rather than aerosol microphysical or chemical measurements to represent aerosol abundance and thus comes short of providing a solid causal relation between aerosol number concentration and rainfall variation. Perhaps the authors could use existing field experiment data or network data to demonstrate the correlation between AOD and the number concentration or even size distribution including size disperse of smoke aerosols particularly hygroscopic aerosols during the period of interest. This could lay a more solid ground for the later correlation analyses.

The authors presented a case study first, mainly to demonstrate the ranges of various diagnostic variables of an observed MCS. It is not quite clear, however, how these knowledge and skills obtained from the case study had been used in the latter analyses, for example towards data filtering. Specifically, the discussion after the case study lacks adequate descriptions of data scope and analysis procedure. My understanding is that the analyses had been applied to a period from 1999 to 2010, however, this was not clearly indicated in the paper, so was the total number of cases, the nature of these cases, and the filtering procedure (descriptions of Section 2 hardly answer these questions). The authors have indicated that the studied area has very extensive MCS appearance. However, regarding data filtering, it is not quite clear whether they had specifically masked cloud/precipitation events other than MCS. Aerosol effects on different types of clouds, and on clouds over different geographic locations have been suggested to be very different. Without a careful selection of studied cases, the correlations established might lack of solid physical background and serve little purpose to advance our knowledge.

The discussion of bin analysis is not clearly presented. What is the definition of "rainfall fraction" in Figure 8? Also, it occurs that in most of the stations, rainfall rate and AOD correlation (Figure 7) exhibits a bimodal distribution, i.e., besides a negative correlation between AOD and rain rate in lower AOD range, rain rate also positively correlates to AOD in higher AOD range (then negative again with the increase of AOD). This interesting feature has not been discussed at all. In addition, I found the attempt to use omega to separate aerosol-rainfall correlation is not very convincing as presented in the paper unless certain statistics of omega are provided.

The EOF analysis is interesting because it derives statistically established scenarios. However, as often occurs that the modes derived from EOF analyses do not necessarily represent physical status but statistical correlation, or a good fit for wrong reason. Specifically, the authors discussed EOF analysis results of one station (Alta Floresta), and concluded that the two major modes respectively correspond to one dynamical factor dominated scenario and one aerosol influencing scenario. The explanation, however, is somewhat too brief (note that the sign of omega in the discussion needs to be clarified), especially without a definition and examination of the dynamical factor(s). Why the aerosol variation could correlate to those of rainfall and relative humidity when omega was around average value? What type of systems typically occurs under this scenario? A benefit of EOF analysis is the time series data, perhaps the authors could use this data source add to the analysis to retrieve the system types, corresponding time of the year, and most importantly, to relate to the statistical smoke aerosol input to the region of interest. Note that there is always a "chick-and-egg" issue to answer when discussing aerosol-rainfall correlation. Without identifying a clear causal relation, this would become an unfortunate dilemma.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 23995, 2013.