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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Overview: Precipitation Characteristics and Sensitivities to the Environmental Conditions during GoAmazon2014/5 and ACRIDICON-CHUVA" *by* Luiz A. T. Machado et al.

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Response to Anonymous Referee #2.

We would like to thank you for your valuable comments (italic). We will improve the manuscript based on your suggestions. Please find a point-by-point response (bold) and proposed changes to the manuscript below.

This paper uses satellite and in situ data from two recent field campaigns to provide an overview of precipitation characteristics in the central Amazon, and their sensitivity to

environmental conditions including time of year (wet vs dry season), aerosol concentrations, land-surface type and topography. The paper describes the complex interactions between different processes in the region, particularly through their impact on cloud microphysics, in a way which is only made possible by these new measurements. While the broad scope of the paper means that each aspect cannot be explored in a lot of detail, it still provides interesting results while also show casing the potential of these new datasets for further work. The paper is well organized and mostly well written (some grammar issues aside), and I recommend it for publication after addressing the following fairly minor comments.

Thank you for your comments and suggestions. All points were addressed as described below.

General comments Language: While the paper is perfectly readable and understandable, there are minor grammar errors throughout – these do add up to quite a large number, which is why I haven't listed them below. I would encourage a thorough proofread by a native speaker.

The manuscript has been reviewed by American Journal Experts, an English-language editing service. We hope this has improved the grammar throughout the paper.

Introduction: This is quite long (about a quarter of the whole paper), although it is very comprehensive. I don't think it's a major issue, but worth pointing out.

We agree that the Introduction is long for a conventional manuscript; however, because we are treating the introduction as an overview, we wished to describe what is known about cloud processes in the Amazon as determined from the field campaign so far. This is why we split the Introduction in two sub-sections. We hope this structure is acceptable as is.

Methods: Given one of the aims of the paper is to show case a new dataset, it is really lacking in contextual information, including where exactly the whole experiment is

taking place. If the instruments are all exactly collocated simply the latitude/longitude might be ok, but I would strongly encourage you to include a map somewhere, showing the location of the instruments (particularly if placed at different locations), as well as the flight paths. This would also allow you to add some much need context. There are two papers that discuss the details of the GoAmazon campaign: Introduction: Observations and Modeling of the Green Ocean Amazon (GoAmazon2014/5) by Martin et al. (2016) (doi:10.5194/acp-16-4785-2016) and The Green Ocean Amazon Experiment (GoAmazon2014/5) Observes Pollution Affecting Gases, Aerosols, Clouds, and Rainfall over the Rain Forest by Martin et al. (2015) (doi:10.1175/BAMS-D-15-00221.1). We understand that the manuscript should be read independently from others papers; however, both papers were included in a special issue, and the introductory paper provides all of the descriptions about the project. Martin′s Figures 1 and 2 provide the information you request. In the beginning of the methodology, we reference these specific figures in order to provide the reader with a general description of the sites and flights.

I would suggest including land surface type, topography and maybe potentially mean winds/some other climatological data.

We added a figure (new Figure 8) indicating the radar-covered area, the vegetation and topography relative to T3 and the SIPAM radar.

State more precisely in the abstract where the experiment is taking place (i.e. not just 'Central Amazon Basin', but ' in the vicinity of Manaus' or something like that).

Changed as recommended.

Land surface results (3.2.2/3.2.3): while these results are interesting as a very general overview, I think it is difficult to draw particularly strong conclusions from them. Firstly, I'm not sure in Figure 8 there really is 95% confidence that the results are different; the test assumes all data points are independent, which will clearly not be the case. The most obvious example is the 'urban area', which accounts for only 0.5% of points

– presumably these points are all clustered together, and likely to be highly autocorrelated. Even if the differences were significant, potential cofounding factors are not considered at all by the authors. For example, topography and land surface type could be correlated in some way, in which case it wouldn't be clear which factor was really driving the differences.

We have revised the figure to illustrate the vegetation and topography distributions. The data are not correlated, and forest covers much of the area. We computed the T-student test for the vegetation and topography to identify the difference between seasons, but the best method would be to test the difference among the classes, as stated by reviewer 1. We agree with reviewer 1 that it is more appropriate to test significant differences among the classes than between the seasons. We have tested the differences among classes, and all differences in the dry season were significant. Furthermore, we used the T-student parametric test. We were curious why all classes passed the test and identified important discussion points related to parametric significance tests when using very large sample sizes. These comparisons were done for several days at the pixel level, so the sample set is very large. All tests generally pass under this condition, and there is no statistical way to use the test with such a large sample set. By using box plots, all basic statistics are shown. Therefore, we decided to eliminate the T-student test for vegetation and topography. This test was only valid when we tested T3 site specific data, which resulted in a much smaller sample set. We eliminated the arrows in Figure 9 (new, formerly 8) and 13 (new, formerly 12).

Finally, the explanation of physical mechanisms is sometimes inconsistent. In particular, p14, L24-26 states that the urban heat island over Manaus will drive convergence and enhanced rainfall, while reduced latent heating will decrease rainfall over non-forest. These statements are interchangeable – cities have reduced latent heating, and the non-forest will be warmer, so why do they have opposite feedbacks?

The answer to your question involves the scale. There is a forest around Manaus, so the city will be warmer and receive moisture convergences from the forest. However,

there is little moisture convergence from large deforested regions during the dry season because these regions have no moisture source to support this process (if the deforested area is large). We attempted to explain this scale problem and discuss the cautions that need to be taken when considering these physical explanations because scale impacts our discussion. The text was revised in the vegetation section to address this concern.

Minor comments P9, L18-19: "Figure 1 clearly reveals. . ." Looking at figure 1 it looks to me like the only bin where the wet season is higher is the lowest one (and marginally, the second), which represent RR < 5. This is a logarithmic scale; however, we have deleted the word "clearly."

P11, L20: "This result suggests. . ..the wet season" I don't quite understand this sentence. This sentence has been removed.

P13, L14-15: "During the dry season. . .mostly by drier days". Might be useful to add a short comment as to why? Presumably this is because biomass burning is more likely to occur on dry days? More broadly, some comments on what the different sources are for the aerosol you measure would be useful.

We introduced a sentence with this discussion and added a reference that describes the aerosol types.

P13, L19: What was the significance level? I think it's fine to discuss the results even if the significance is below 95% if they are still physically consistent, but there is still a difference between, for example, 80% significance and no correlation whatsoever.

We computed the significance (85%) and added it to the text.

P15, L21-28: if clouds were at different heights over forest and non-forest, could your fixed-height measurements simply be a reflection of what part of the cloud you were measuring, instead of the clouds having different microphysical properties over different surfaces? You are suggesting that clouds over forest have a different cloud base

than those over non-forest. This is true; however, we are not certain if this occurs within such a short path as those in this study (around 50 km). That said, the first leg occurred in the morning when the cloud base difference is very small. There was a cloud base flight (just below) prior to the level 1500. We have added a discussion about this possible effect on the measurements in the text.

Figure 1: it would be nice to have error bars (these could replace the squares and circles). I would only refer to the 'T3 site' in the caption if its location is defined in the text (not just with a reference).

Figure 1 is a frequency distribution; therefore, it is not possible to add error bars. However, we added a new Figure 2f, which shows the rain rate box plots.

Figure 6: It would be helpful to state in the caption roughly what ZDR, KDP and horizontal-vertical correlation refer to physically (e.g. ice orientation for ZDR).

Changed as recommended.

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