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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "What Drives Daily Precipitation Over Central Amazon? Differences Observed Between Wet and Dry Seasons" *by* Thiago S. Biscaro et al.

Anonymous Referee #2

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General evaluation: This manuscript considers the problem of precipitating convection over the Amazon and the difference between dry and wet seasons. I found the scope of the paper suitable for the ACP, but I feel it needs to be significantly revised to meet the high standards of Copernicus publications. Below I present my major comments and follow with a long list of specific problems and questions that need to be addressed. The manuscript can be accepted for publication only after all these points are properly addressed.

Major comments:

1. I found the discussion in the paper speculative, lacking solid scientific basis and

references to the past literature. For instance, what is the main conclusion of this study? One possibility is a suggestion that the nighttime cloudiness delays surface solar heating the next day during the wet season and this leads to later moist convection development or no development at all. This is because night and early-morning clouds need to be "burned out" before the significant solar surface heating commences. This is no longer true for the dry season, perhaps because of the lower cloud cover in general (as suggested by Fig. 1). Or is there more to the story? The difference between wet and dry season is likely not as dramatic as the difference between pre-monsoon and monsoon condition over the Indian subcontinent as discussed in Thomas et al. (ACP 2018, p. 7473), but I expect some similarities. For instance, the extreme CAPE values do happen during the dry season, and differences in the surface temperature, Bowen ratio, and boundary layer height between wet and dry season are also consistent with such arguments. The impact of larger-scale factors (mesoscale and synoptic-scale) argued to be more important for the dry season is really not supported by the analysis shown in the paper. Perhaps this may be illustrated by more random timing of the deep convection that is initiated around the observation site and subsequently moves over the site at random hours. But this would require selecting a different analysis strategy, that is, not focusing on NR-NR and NR-RR alone.

2. The introduction presents an incomplete review of previous relevant studies. Daytime convective development over land was an emphasis of some important past studies, such as Guichard et al. (QJRMS 2004, p. 3139) or Grabowski et al. (QJRMS 2006, p. 317). The latter used data from the LBA project to design the modeling case. Those papers need to be discussed in the introduction and some of the studies referred to in those papers (like the Betts and Jacob JGR 2002 who were first to point out problems with ECMWF model over the Amazon) need to be brought up to set the stage for this study. Also, since Khairoudinov and Randall (2006) cited in l. 33 used the setup described in Grabowski et al. (QJRMS 2006), a reference to the original paper would be desirable (and appreciated by all coauthors).

3. I have numerous comments on specific figures and their discussion. They often lack precision and leave the reader unclear about the key points. Please see the list in the specific comments below.

Specific comments (some major):

1. The abstract: The first sentence is unclear. “Alternative approach” to what? Or maybe alternative explanation (per the last sentence in the abstract). The last sentence: “heat-induced turbulence”. What is that? Surface sensible heat fluxes? See comments in 12 below.

2. L. 25: please replace reference to Gentine et al (2013) with a discussion and references suggested above. Those are more relevant and provide a better context for this study.

3. L. 86: what is the reason for focusing on the contrast between no rain overnight leading to rain or no rain? Does the nocturnal rain affect daytime rain more randomly? This is a very basic question and I am left wondering.

4. L. 152-153. What is meant by “consumption of energy” in this sentence? I do not understand what energy this statement is concerned with. Is my interpretation in 1 in the major comments wrong? I expect that night-time clouds may be remnants of the previous day convection, so this would require looking at the previous day convection together with the night-time convection. Are advective effects not important in that regard? Overall, “consumption of energy” is an inappropriate term and it explains little.

5. L. 179. This is pseudo-adiabatic CAPE, correct? Please explain. Also, what surface conditions are taken for the CAPE analysis (lowest 500-m average?). This is detail, but it should be mentioned.

6. L. 185. Again, what is “energy consumption”? Nighttime increase of CAPE comes from longwave cooling of the atmosphere, and presence of clouds (especially low-level clouds) has a significant impact. Is that the key process? Also, drier atmosphere in the

dry season may result in a larger nighttime longwave cooling as well. Please explain.

7. Fig. 3 and its discussion. Increase of CAPE in the early morning hours (02 to 08) is similar between NR-RR and NR-NR, but the reduction of CIN is larger for NR-RR. Is that important?

8. For the soundings (Fig. 4 and 5) I suggest showing standard deviations among the dataset members. Those can be shown at a few levels as horizontal bars whose lengths show standard deviations.

9. L. 237-239: Higher soil moisture does change the Bowen ratio and leads to the higher latent heat contribution to the total surface heat flux. It makes the boundary layer to deepen slower (as show in the Thomas et al. paper mentioned above and likely in other studies). The logic in this sentence is reversed: more clouds does not lower convective PBL height, different Bowen ratio does.

10. L. 247. Please explain how the PBL height is measured with the ceilometer. I think you assume that the cloud base is close to the PBL height. This is true for a convective BL when the cloud base (if clouds are present) is close to the BL top. But this is not always the case, and unlikely valid in stable nighttime conditions. A comment on that would be appropriate. I feel the discussion in this paragraph is related to that in Thomas et al. (ACP 2018).

11. The maximum surface flux values seem low considering the LBA case setup in Grabowski et al. (QJ 2006) mentioned above (see appendix there). Please explain or correct the error.

12. Please explain how the TKE is estimated by ECOR. For instance, different surface wind conditions (due to different synoptic conditions) would affect sheer-produced TKE near the surface. Is that included in the analysis? Or maybe the analysis focuses on the thermally-driven turbulence that comes from different surface Bowen ratio. Please explain.

13. Fig. 10 and 11. To me, the two figures simply show the impact of different surface conditions between dry and wet season, and their impact on daytime boundary-layer and moist convection development. For the wet season, the lower TKE for NR-NR may be because of no cold pools associated with precipitating convection. Cold pools and presence of precipitation lower air temperature near the surface as shown in Fig. 11. But I think cold pools are not really part of the answer to the question in the title of the paper.

14. Fig. 12. How the presence or absence of clouds affects the comparison shown in the figure?

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