Response to reviewer #2: "What Drives Daily Precipitation Over Central Amazon? Differences Observed Between Wet and Dry Seasons" by Thiago S. Biscaro, Luiz A. T. Machado, Scott E. Giangrande, and Michael P. Jensen

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.

We would like to thank the reviewer for the comments, questions, and suggestions. We have thoroughly revised the manuscript based on this feedback, and we have attempted to address the concerns and incorporate the changes suggested by all reviewers. In response, the literature review was expanded, with additional emphasis on studies of shallow-to-deep transition and the diurnal cycle of precipitation. This change included several suggested references by the reviewers. We worked on the conclusions and changed them accordingly when speculative sentences were written.

Our analysis is based on a starting hypothesis that nighttime cloudiness delays surface solar heating on the following day during the wet season; this contrasts with the dry season that suggests a smaller cloud coverage during those periods. Also, the locally observed quantities such as TKE, fluxes, temperature, etc., present different behaviors during the wet season for NR-NR/NR-RR days, features or characteristics that are not observed during the dry season. As the reviewer correctly states, that is not the only factor influencing the differences found in precipitating days between wet and dry seasons. We can cite large-scale moisture advection during the dry season as one of the factors that leads to precipitation within this season (e.g.: Ghate and Kollias, 2016) and the timing of the morning transition of the nocturnal boundary layer impact on the shallow-to-deep transition (Henkes et al. 2021). The large-scale convective features are similar for the wet season and independent of rainfall occurrence, again in contrast with the dry season, which presents a very distinguishable shift between NR-NR and NR-RR days. This shift is supported by the composite differences, as well as by the temporal evolution of the mean brightness temperature field. In conclusion, we suggest that differences in the nocturnal cloud coverage between the wet and dry seasons impacts the onset of convection within each season, with the mesoscale circulation being the main feature impacting local convection during the dry season, while the wet season has its local convection mainly impacted by local factors and night cloud occurrence.

Reference added:

Henkes, A., Fisch, G., Toledo Machado, L. A., and Chaboureau, J.-P.: Morning boundary layer conditions for shallow to deep convective cloud evolution during the dry season in the central Amazon, Atmospheric Chemistry and Physics Discussions, https://doi.org/10.5194/acp-2021-87, 2021.

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).

Thank you for the suggestion. As described in the following responses, a paragraph discussing the results by Guichard et al. (2004) and Grabowksi et al. (2006) has been included.

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.

Thank you for your comment. The “alternative” was dropped during our re-write, as this term was causing ambiguity/confusion. We refer to heat-induced turbulence as turbulence that is mainly generated by surface heating, which leads to convection and irregular low-level winds.

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.

Thank you for the suggestion. The following paragraph was added to the introduction:

“Regarding the diurnal cycle of precipitation, Guichard et al. (2004) and Grabowski et al. (2006) demonstrated that single-column models (SCM), using parameterizations to represent moist convection and clouds, reproduced the same early-precipitation behavior presented in full 3D large-scale models. Also, SCMs predict instantaneous growth of deep convective clouds within one timestep after their tops overcome the surface-based convective inhibition. Hence, a correct depiction of the convective diurnal cycle depends not only on the correct representation of deep convection, but also on the representation of a progression of regimes, from dry to moist non-precipitating to precipitating convection. Cloud resolving models (CRMs), on the other hand, can capture qualitative aspects of the convective diurnal cycle, although they are subject to model resolution and sub-grid scale processes representation.”

Also, we have added to our conclusions:
“Parameterization schemes must consider seasonal differences in their formulation, as noted by several studies (D’Andrea et al., 2014, Grabowski et al., 2006, Guichard et al., 2004, and references therein) …"

References added:
Grabowski, W.W., Bechtold, P., Cheng, A., Forbes, R., Halliwell, C., Khairoutdinov, M., Lang, S., Nasuno, T., Petch, J., Tao, W.-K., Wong, R., Wu, X. and Xu, K.-M. (2006), Daytime convective development over land: A model intercomparison based

on LBA observations. Q.J.R. Meteorol. Soc., 132: 317-344. https://doi.org/10.1256/qj.04.147

Guichard, F., Petch, J.C., Redelsperger, J.-L., Bechtold, P., Chaboureau, J.-P., Cheinet, S., Grabowski, W., Grenier, H., Jones, C.G., Köhler, M., Piriou, J.-M., Tailleux, R. and Tomasini, M. (2004), Modelling the diurnal cycle of deep precipitating convection over land with cloud-resolving models and single-column models. Q.J.R. Meteorol. Soc., 130: 3139-3172. https://doi.org/10.1256/qj.03.145

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.

Thank you for the question. The main goals of this study were to understand why some days presents only shallow convection and other days, in contrast, develop deep precipitating convection. To have a normalized situation, days with shallow and deep convection (no rain/rain) were selected where there is little influence of the day before, therefore only non-raining nights were selected. It is hypothesized that processes occurring during the night could influence the evolution of the boundary layer in the next day, therefore controlling the convective processes. The nocturnal period was also used as a control because daytime precipitating convection is frequently observed in Central Amazon, especially during the wet season (e.g., 91 days during the 2015 wet season). We have added the following sentence to the text: "We do not assume that convection is only dependent on nocturnal conditions, but our aim is to isolate the potential factors in the evolution of the convective environment that may lead to diurnal precipitation. This is a convenient simplification, as isolated convection also may occur during overnight periods (which would affect soil moisture and atmospheric stability during the morning, among other factors), and expanding this period would result in potential inclusion of convection occurring on the previous day."

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.

Thank you, this question is addressed in the response to question 6.

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.

Thank you for the question. The following text and reference were added to the revised manuscript:

“For CAPE and CIN calculations, the traditional approach of parcel theory was applied – water vapor phase changes only, and irreversible parcel ascent in a virtual potential temperature framework (Bryan and Fritsch, 2002). We define the originating level of the convective parcels as the level of maximum virtual temperature in the lowest 1000 m of the atmosphere representing the most buoyant parcel in the boundary layer, maximizing the CAPE and minimizing the CIN.”

Reference added:
Bryan, G. H., and Fritsch, J. M. (2002). A Benchmark Simulation for Moist Nonhydrostatic Numerical Models. Monthly Weather Review 130, 12, 2917-2928, https://doi.org/10.1175/1520-0493(2002)130<2917:ABSFMN>2.0.CO;2

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.

Thank you for the questions/comment. Energy consumption, as stated in line 152-153 and 155, is referring to CAPE. Given the relatively high humidity or cloudiness in the lower atmosphere in the Amazon, we expect the maximum in longwave cooling to be elevated, thereby cooling the mid-troposphere and increasing surface-based CAPE. For a drier atmosphere, the longwave cooling will be lower in the atmosphere, and could act to decrease the buoyancy of the surface-base convective parcel, reducing CAPE. Clouds formed during nighttime will decrease the instability during the day and reduce CAPE between 20 and 02 LT observations. As presented in Fig. 3, this CAPE reduction is more pronounced during the NR-NR mode of the wet season since there is greater cloud coverage (Fig 1).

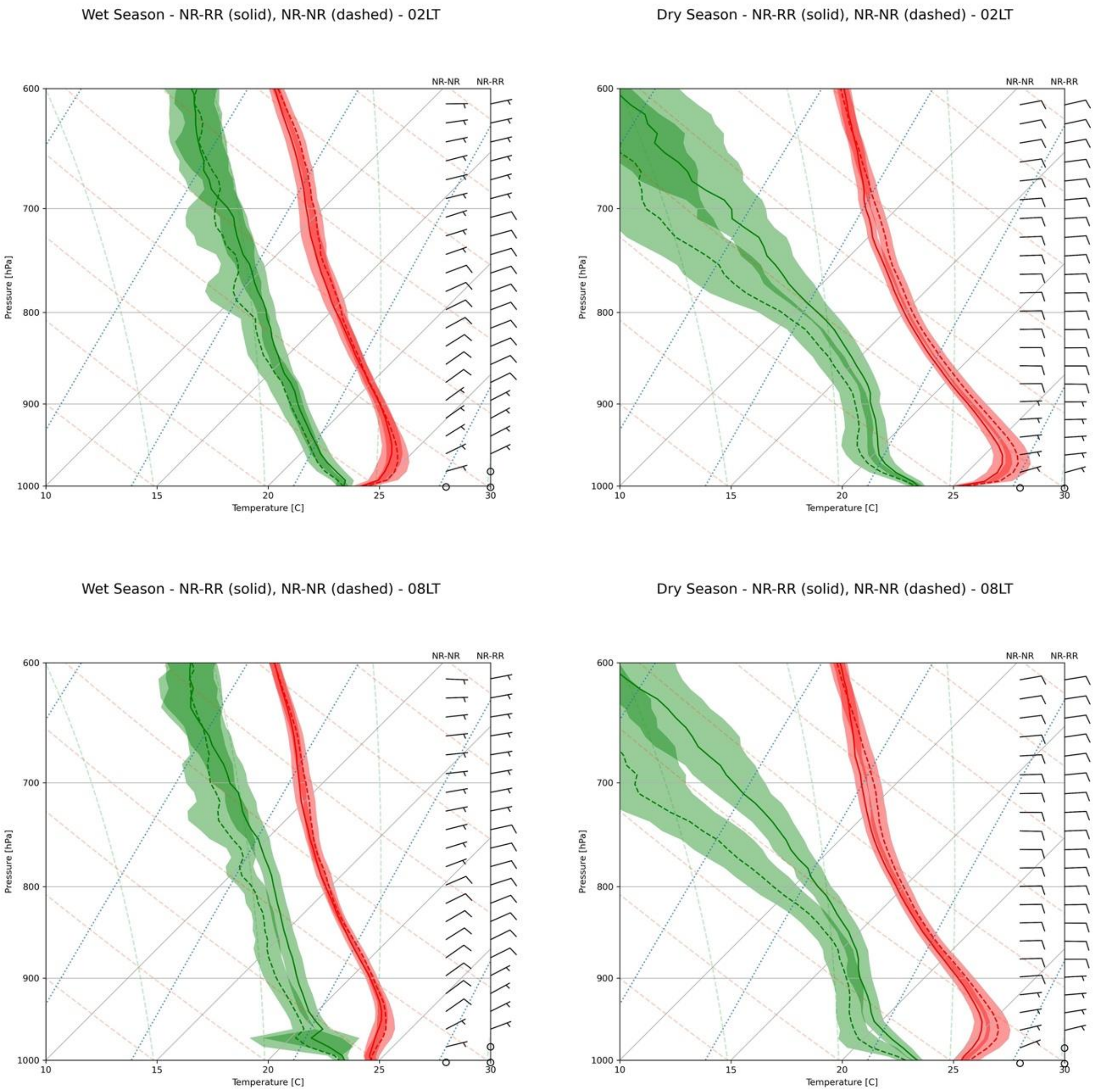
7. Fig. 3 and its discussion. Increase of CAPE in the early morning hours (02 to 08) is similar between NR-RR and NR-NR, but the reduction of CIN is larger for NR-RR. Is that important?

Thank you for the comment. Yes, reduction of CIN during NR-RR days imply less stability, increasing the likelihood of deep/precipitating convection. The following text was added to the manuscript:

"Between 02 and 08 LT, CIN reduction observed in both seasons for the NR-RR mode implies a higher probability of deep/precipitating convection during the afternoon".

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.

Thanks for the suggestion. We have added the standard deviation as shaded areas, as presented below:



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.

Thank you for pointing out this issue. The sentence was rephrased as:

“This analysis also indicates the role of the surface moisture in the PBL development, since higher soil moisture in the wet season may lower the Bowen ratio (Thomas et al., 2018), thus lowering the PBL compared to the dry season, as also discussed in the next sections."

Reference added:

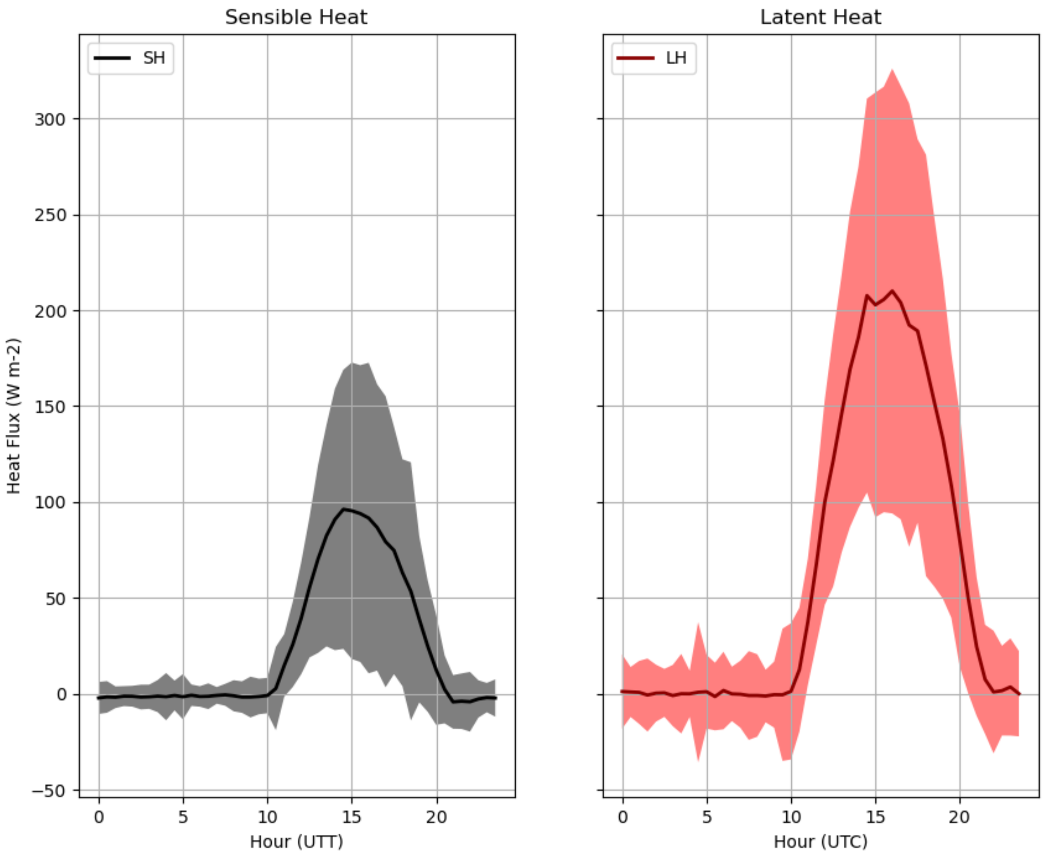
Thomas, L., Malap, N., Grabowski, W. W., Dani, K., and Prabha, T. V.: Convective environment in pre-monsoon and monsoon conditions over the Indian subcontinent: the impact of surface forcing, Atmospheric Chemistry and Physics, 18, 7473–7488, https://doi.org/10.5194/acp-18-7473-2018, 2018.

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).

Thank you for your question. The PBL height is derived from the gradient in aerosol backscatter profile (not from cloud detections, but the DOE ARM "Value-Added Product" CEILPBLHT, e.g., ARM 2013). It is important to note that there is a cloud/precipitation filter associated with this product. This is different than radiosonde-based products that may associate PBL with LCL (e.g., Thomas et al., 2018). This statement was added to the text as well. We note that are other ARM VAPs (PBLHT) that are similar to those products, when appropriate to apply.

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.

The LBA experiment was set up in the Southwest Amazonian region, near the arch of deforestation. In our case, the values were obtained in Central Amazon, in a cleared area surrounded by forest and two large rivers. Besides, values in Grabowski et al. (2006) were theoretical values, not observed ones. We used LH and SH values obtained with the ECOR (e.g., ARM 2014), with their means and standard deviations for all data flagged as good quality. As with the ARM CEIL datasets, these data are available through www.arm.gov and displayed below. The fluxes presented in Grabowski et al. (2006) are significantly higher (max H = 270, max LE = 554 W/m²) than those observed during GoAmazon2014/5. They are also higher (in some cases twice the value) than the composite values observed during LBA by Betts et al. (2002) fig 6.



Reference:

Betts, A. K., Fuentes, J. D., Garstang, M., and Ball, J. H., Surface diurnal cycle and boundary layer structure over Rondônia during the rainy season, J. Geophys. Res., 107(D20), 8065, doi:10.1029/2001JD000356, 2002.

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.

Thank you for your question. TKE is derived using the variances of the u, v, and w wind components provided by the sonic anemometer which is part of the ECOR system. We did not discard data due to synoptic conditions, hence all good-quality flagged data were included in the analysis. There is additional information on the ARM ECOR located at https://www.arm.gov/capabilities/instruments/ecor, and within the instrument handbook at: https://www.arm.gov/publications/tech_reports/handbooks/ecor_handbook.pdf. This statement was added to the text as well. The increase in boundary layer turbulent kinetic energy facilitates convection by helping to raise parcels to their level of free convection. There are no large-scale factors directly impacting the wet season results when differences in TKE was observed, therefore, the shaper increase in surface fluxes combined with more intense turbulent process favors deep convective processes.

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.

Thank you for your question/comment. The main information presented on these figures (or in any figure presented in the manuscript) is not the difference between wet/dry season, where we agree that a moister soil during the wet season will impact our observed data, rather the difference between NR-NR/NR-RR days within a season. What we are trying to show is that during the wet season there is a clear difference in the NR-NR/NR-RR days in the local observations, difference that is not noted during the dry season. Cold pools could contribute to lowering the temperature during raining days, but it should be noted that we are measuring temperature and precipitation at the same point, so it is most likely that the temperature drop observed is related to precipitation. Also, cold pools are observed in deep convective processes, and until 08 LT we are dealing with shallow convection. We would probably find differences related to cold pool effects in the afternoon, but not at the early morning.

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

The figure is a composite brightness temperature IR field, which we have offered as one quantification of the cloud-top temperature or physical cloud top height for optically thick clouds. Colder brightness temperature values indicate higher clouds (deep convective clouds and their associated anvils), warmer values indicate lower clouds (shallow or mid-level convection) or absence thereof. We note that optically thin cloud layers and atmospheric water vapor will also impact these brightness temperature values, but the difference is between the same season, so it is a very small effect. The main goal is to evaluate if the large-mesoscale field is associated with the different pictures we found for NR-NR and NR-RR events. We note that for the wet season, the large scale has no significant influence, there are little differences between the NR-NR and NR-RR composites during the nighttime, indicating that the local processes are the main driver of the transition (or lack thereof) from shallow to deep clouds. However, during the dry season, there is a clear difference in the large scale among both composites, indicating that rainy days during the dry season have different synoptic scale patterns.