Review of "Planetary boundary layer evolution over the Amazon rain forest in episodes of deep moist convection at ATTO" by M. Oliveira et al.

This study uses data from a tall tower in the Amazon to assess the thermodynamic and kinematic properties of convective downdrafts/outflows/cold pools. The study focuses on four deep convective cases of differing spatial characteristics. Three of the four cases were nocturnal, while one occurred during the early afternoon hours. The authors find interesting differences between the thermodynamic and kinematic properties of the PBL after the different convective system passages. Notable differences include (1) well-defined gust fronts in the nocturnal cases vs. a weakly defined gust front in the daytime case; (2) different PBL layers recover quite differently after system passage for the isolated system cases; (3) nighttime cases have clearly defined increases in sensible heat near the time of gust front arrival and decreases afterwards, whereas the daytime case exhibits different behavior. Interesting differences are noted in the response of the surface layer of the PBL vs. the top of the canopy, including that heat fluxes are most pronounced above the canopy rather than within the canopy.

I think this study is well-written and presents many interesting findings. The authors provide insightful discussions throughout. The authors' findings are complementary to past studies, yet provide new insights into processes that are difficult to observe and are thus not readily studied (downdrafts, PBL dynamics and thermodynamics, detailed land-atmosphere interactions).

Overall, I recommend that this study be published in ACP with ***minor revisions***.

General comments:

1. You provide various explanations for defining and choosing your cases. You also attempt to explain why you chose such a short study period on page 4. However, your explanations seem rather unclear to me. More specifically, could you clarify what you mean by "We have chosen such a short time window primarily because of the nonstationary nature of the events under study, but also to avoid contamination from low-frequency, non- turbulent processes, and, therefore, guarantee that the discussion refers to turbulent quantities alone (lines 11-14, page 4)"? Stating that "Only storms that produced detectable impacts on the evolution of meteorological variables at the tower site were selected (p. 4, lined 28-29)" makes sense over such a short time period, but again, I don't feel that the short time period is ever adequately justified.
2. Since it's hard to generalize day vs. night, organized vs. disorganized convection differences in PBL behavior following system passage when you only have four cases, I think you should add a few concluding sentences cautioning the readers against generalizing these conclusions. Perhaps an appropriate place to do so is after the schematic is introduced in the conclusion?

Specific comments:

Lines 9-10: Please revise to read "The nocturnal events had well-defined gust fronts with moderate decreases in virtual potential temperature and increases in wind speed."

Line 12: "experienced an increase" – how about just "increased" ?

Page 5, line 21: Schiro and Neelin (2018, ACP) compare statistics on downdraft/cold pool properties from both sub-MCS size system and MCS systems at the GoAmazon2014/5 site. Wang et al. (2019) also uses GoAmazon2014/5 data to look at cold pool/downdraft characteristics. Both studies use the S-Band radar to classify the deep convection. It seems that references to these studies could be appropriate here.

Figure 1: It would be very helpful to add spatial information to the axes on the subpanels, especially since you discuss the degree of spatial organization. Also, please mention what the circles (dashed lines) mean in the caption (what distance is this from the radar?). Lastly, please label the panels a-d.

Oct 31 case – It seems to me (from Fig. 1) that this exhibits a decent amount of organizational structure (leading line, trailing stratiform), even though the individual leading-edge cells passing over the tower may have seemed disorganized or separated from one another at any given time or may have merged with other isolated cells (as you mention). The thermodynamic and dynamic responses (Figs. 2 and 3) also suggest that this is an MCS. If you agree with this assessment, you may wish to revise your classification in the table and in lines 24-25 in Section 3 (p 5): "In comparison to SR98, the storms on 31 October (event 1), 2 November (event 2), and 4 November (event 3) mostly 25 resembled the unorganized arrangement that they referred to as sub-MCS-scale nonlinear systems."

P. 6, lines 9-10: You could probably reword this sentence to make it reference Figs. 2a and 3a respective to the order in which they are mentioned. Same for lines 28-29. (and pg. 7 line 26).

Page 6, line 12: What is the time of the first drop, shown in the dashed vertical line on Fig. 2a?

Page 6, line 10: I wouldn't say that the temperature decrease was significantly damped in Fig. 3a, especially if you look out past the 2nd drop in temperature. In fact, it's interesting that the 14m temperatures seem to be lowest, whereas at 22m, they are highest (after 18:00 LST). You could maybe discuss that here and speculate why you think that might be.

P. 6, lines 29-30 – That increase in moisture is interesting. Maybe you could speculate here about why that might have occurred. Maybe it was moisture convergence occurring along the gust front edge? Saturated convective downdrafts from low levels entering a previously unsaturated PBL?

Nov 2 and Nov 8 event recovery vs. Oct 31 and Nov 9 recovery: The fact that the smaller, more isolated convective cells have a detectable PBL recovery time period than the larger MCSs, regardless of the time of day, is consistent with what we found in Schiro and Neelin (2018).

Pg. 7, line 13 – I wouldn't classify this as a drop; it's more like a "decrease," since it's rather gradual

Pg. 7 line 16: instead of "slow", how about "gradual"?

Insightful discussion in lines 16-22 of pg. 7. I agree with your assessment, since radar reflectivity at 14:57Z does seem to suggest that the cell did not pass directly over the tower.

Pg. 7, Line 24: I'd be careful about using phrases like "the most organized." It's hard to distinguish organization in the first place (though it's often loosely defined using spatial characteristics). I think classifying it as "organized" is speculative as it is, since you mention that the spatial scale is somewhere in between "isolated" and MCS. Instead, maybe you could classify it as the "system with the largest convective core"?

Fig. 3d – Why do you think the 40 m spikes are so much larger (and the data generally noisier) than at 14 and 55 m? Also, where is the rest of the data? Does missing data suggest data quality issues for this sample?

Heat flux measurements and discussion: I can't comment too much on the reliability of these data, but I don't doubt that there are noteworthy data concerns here (especially given the really large magnitudes observed in certain instances). At the very least, I think a discussion of the strengths and limitations of using these data during pre-storm and precipitating conditions is warranted in these sections.

Please explicitly define TKE and how it is computed.

<u>References:</u>

Wang, D., S. E. Giangrande, K. A. Schiro, M. P. Jensen, and R. A. Houze, 2019: The Characteristics of Tropical and Midlatitude Mesoscale Convective Systems as Revealed by Radar Wind Profilers. *Journal of Geophysical Research: Atmospheres,* 124(8), 4601-4619.

Schiro, K. A. and J. D. Neelin, 2018: Tropical Continental Downdraft Characteristics: Mesoscale Systems versus Unorganized Convection. Atmospheric Chemistry and Physics, 18, 1997-2010.

-Kathleen Schiro