

Title: Planetary boundary layer evolution over the Amazon rain forest in episodes of deep moist convection at ATTO.

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Authors: Maurício I. Oliveira, Otávio C. Acevedo, Matthias Sörgel, Ernani L. Nascimento, Antonio O. Manzi, Pablo E. S. Oliveira, Daiane V. Brondani, Anywhere Tsokankunku, and Meinrat O. Andreae

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Recommendation from the reviewer: Minor revisions

Replies to Reviewer #3 (Dr. Kathleen Schiro):

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.

The authors deeply appreciate the in-depth critics and suggestions provided by the reviewer. We believe the manuscript has been significantly improved as a result of this revision. Below the reviewer will find our point-by-point responses, written in bold-faced dark blue.

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.

We agree with the reviewer that both the choice of the period of study as well as the use of short averaging windows can be further explained and clarified. These points are addressed below.

Period of study: The dataset used in this paper refers to an Intensive Operating Period (IOP) at the ATTO site focused on the period from late October through mid-November 2015. At the time this IOP was conducted, most of the instruments had not been were deployed for continuous measurements; this is scheduled to happen in the upcoming months. Nevertheless, only during this IOP, there was multiple micrometeorological instruments (CSATs) operating simultaneously at several tower levels, making this period suitable for conducting the case studies we presented. We have added to the manuscript that the period of observations refer to an IOP.

Averaging time window: The short, 1-min time window we describe in lines 11-14 (pg. 4) refers to the averaging time interval from which turbulent fluctuations are calculated from. Such short averaging time window is needed to capture the dynamics of the gust front passage given the highly transient, abrupt nature of the phenomenon. Average flux calculations determined over a more typical 30-min window would yield much smaller flux magnitude in the cases studied, i.e., introducing the adverse effect of smoothing out the flux peaks and thus, missing all the dynamics of the event passage.

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?

Thank you for the comment. This concern, also raised by Reviewer #1, is a relevant suggestion which helps to present our conclusions more clearly and caution readers about the generality of our findings. Motivated by your suggestion, we have included the following statements in the conclusion:

“Despite the consistency found among the events analyzed, it is important to stress that the study is based on a reduced number of events (4) and that a more detailed analysis with a larger number of cases is necessary to validate the conclusions. They will be possible along ATTO project, when continuous turbulence observations will be available from the surface to 320 m.”

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.”

The sentence has been modified as suggested.

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

The modification has been done.

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.

Thank you for pointing that out. Your comment has motivated us to rephrase a couple of sentences in the manuscript. On page 4 we have added a citation to Schiro and Neelin (2018) when mentioning previous studies that have applied quantitative criteria to select the convective events. On page 5 we now cite both Schiro and Neelin (2018) and Wang et al. (2019) together with SR98.

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.

We agree that relevant spatial information was lacking in the subpanels and caption of Figure 1; in the new version such information is provided. Thank you.

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 resembled the unorganized arrangement that they referred to as sub-MCS-scale nonlinear systems.”

Thank you very much for raising this important point, but during this event we found no contiguous region of reflectivity above 30 dBZ displaying 100 km or more in length. To further verify if an MCS could be characterized in any given moment of the evolution of this event, we checked the GOES-13 thermal IR imagery during the life

cycle of the storm system, but the only MCS observed in that period was located in northern Pará state, hundreds of km to the northeast of the region of interest. To illustrate that, we are copying, in this reply, the GOES-13 enhanced thermal IR image valid around the time of the radar image shown in Fig. 1a. Given these points we have no solid argument to support a claim that the event was indeed an MCS.

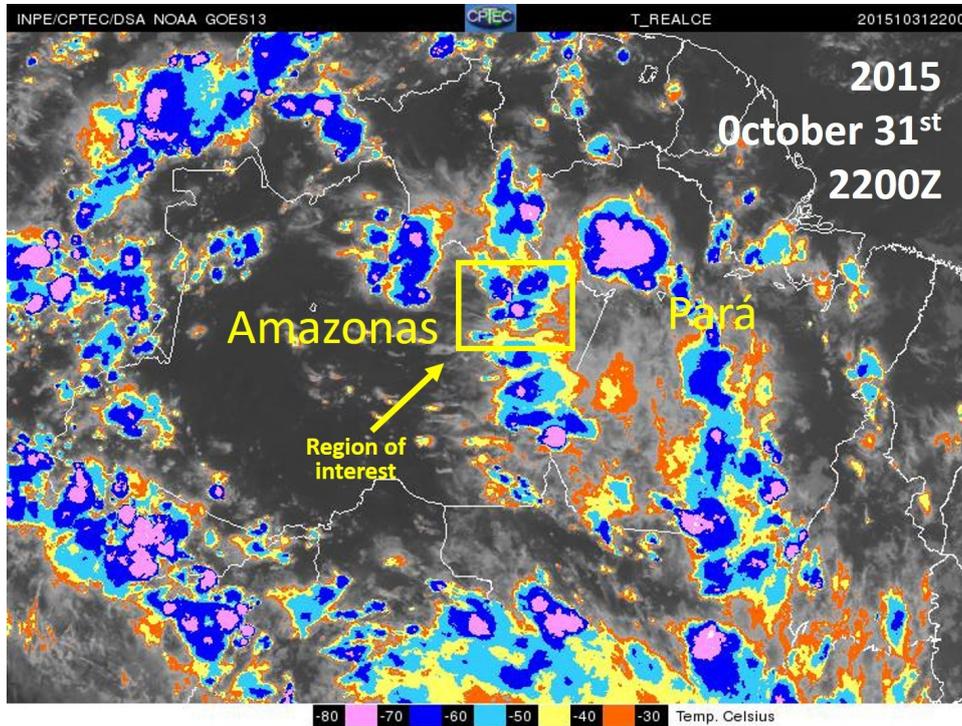


Figure R3.1: enhanced thermal infrared GOES 13 image at 22:00 UTC 31 Oct 2015 over the Amazon region. Brightness temperatures indicated by the color shading, in °C). The yellow rectangle indicates the convective system of interest.

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).

We agree with your suggestion. The sentences in lines 9-10, 28-29 and 26 (pg. 7) have been reworded to properly reference Figures 2a and 3a.

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

The time of the drop represented by the dashed vertical line on Fig. 2a is 17:15 h Local Standard Time (UTC = LST + 4 h). In view of this comment, we also included the times of the drops in the caption of Figure 2 for all events. These correspond to: 17:58 LST on 2 Nov 2015 (Fig. 2b), 10:00 LST on 4 Nov 2015, (Fig. 2c) and 03:00 LST 9 Nov 2015 (Fig. 2d).

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.

The reviewer is right when we look out after the 2nd drop in temperature. However, this is addressed later in the same paragraph. When we said that the temperature was damped inside the canopy, we were referring to the 1st drop, during the period right after the outflow starts (period II in Fig. 3a), as the drop rate of temperature at 14 and 22 m was smaller than above the canopy. In fact, temperature at 14 m was smaller than above the forest before the outflow starts, and became larger during period II.

The fact that temperature at 22 m is larger than at the lower levels inside the canopy is very interesting, but it is not surprising. Previous studies have shown that the temperature within the forest is consistently smaller close to the ground, especially during daytime (Viswanadham et al., 1990; Kruijt et al., 2000). This occurs because the radiative heating inside the forest starts from the canopy top towards the ground. During the night, however, we think that the energy loss at 22 m is not enough to reduce the temperature to levels below those observed close to the ground.

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?

Thank you very much for drawing our attention to these ideas. This is indeed an interesting aspect of this particular event. We agree with the reviewer's suggestions for the possible physical processes operating and, hence, we have added a new sentence taking into account these plausible hypotheses (following the "not shown" statement):

"This transient moisture increase may have been caused by moisture convergence along the gust front or the intrusion of low-level saturated convective downdrafts into 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).

Thank you again for point this out. The results regarding PBL recovery time as a function of convective mode/organization discussed in Schiro and Neelin (2018) are definitely in line with the results we found. Therefore, we have included a new paragraph at the end of subsection 3.4 and referenced Schiro and Neelin (2018) in order to shed light on the relationship between PBL recovery and convective system spatial scale.

“The longer recovery period observed in event 4, as well as that found in event 1, are in contrast with the short recovery observed in event 2, which points to the dependence on the spatial scale of the outflow-producing system. This observation is in line with the results of Schiro and Neelin (2018), who show that recovery time of the PBL tends to be shorter for isolated convective cells than for MCSs, regardless of the time of the day when the convective activity occurs.”

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

Thank you for pointing that out. We have changed “drop” by “decrease”.

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

The word has been changed.

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.

Thank you for your comment. In fact, it seems that the cell actually “glanced off” the station site at the time shown in the radar image. It may be speculated that the outflow in the wake of the cell reached the tower site later resulting in the observed gradual decrease in temperature and attendant increase in wind speed.

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”?

We agree with the reviewer’s point. Deep convection organization classification is indeed difficult, especially in situations lacking significant vertical wind shear, characteristic of barotropic atmospheric environments. As a result, we incorporated the reviewer’s suggestion and change the term “the most organized” to “system with the largest convective core”, as it is more appropriate.

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?

In Fig. 3d, the data at 40 m had, indeed, quality issues between 3:30 and 5:00 and it has been removed from Figs. 3, 4 and 7. The 80-m data is not available for this event and the 22 m has been added to the Figure.

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.

This is a valid concern. Following the suggestion of reviewer #1, we analyze TKE spectra and heat flux cospectra for the 4 different portions of events 1 and 2: before the gust front (I); the period of upward heat flux that marks the gust front arrival (II); the period of large downward heat flux that corresponds to enhanced storm-generated turbulence (III) and the wake period after the event (IV). We also analyze the raw turbulent velocity and temperature data from events 1 and 2 and the precipitation evolution along each event. All plots have been included as supplementary material and a brief discussion referring to them has been included to the manuscript.

Please explicitly define TKE and how it is computed.

TKE is computed as:

$$\text{TKE} = \frac{1}{2}(\overline{u'^2} + \overline{v'^2} + \overline{w'^2}),$$

where:

u' , v' , and w' are turbulent fluctuations relative to the 1-min Reynolds averaged x , y , and z wind components, respectively, calculated as:

$$u' = u - \overline{u}$$

$$v' = v - \overline{v}$$

$$w' = w - \overline{w},$$

where u , v , and w represent total (non-averaged) wind components. Overbars indicate Reynolds-averaged quantities.

We have included in line 21 (pg. 10) the definition of TKE presented above for clarification.

References:

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