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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Planetary boundary layer evolution over the Amazon rain forest in episodes of deep moist convection at ATTO" *by* Maurício I. Oliveira et al.

Anonymous Referee #1

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

The authors present a short case study regarding the passage of convective systems at the ATTO site and corresponding measurements of turbulent quantities at the ATTO site in the Brazilian Amazon. While the topic is of general interest to the community, I have strong methodological concerns regarding the turbulence measurements during convective episodes, which are not relieved by reading the methods section. It is imperative that they are addressed before publication, which may not be possible.

General / Major Comments:

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Discussion paper

1) My main comment, which needs to be addressed before publication is due to methdology. Section 4, which is the main results section investigates turbulent fluxes and TKE during the passage of storm systems. However, I am not convinced that the data during these episodes is reliable and supports the conclusions. During rain events or with water on the transducers CSAT3 do not work very well. While light rain may be acceptible, during heavy rain (>3 or so mm/h) sonic anemometers generally produce no accurate readings. There may also be an issue with virbrations of sensor mounts and tower that affects measurements during storms. For example I find the reported values of TKE (increase by factor of $\sim$50 during passage of cells) and H (up to -800 W/m2) questionable/ unrealistic. Can values like this be supported from the literature. The methodology does not mention any kind of data quality assurance. For example, the authors should look at turbulence spectra to check whether these look OK and eliminate data observed during rain events or during periods when sonic transducers are likely wet.

2) The paper presents 4 events (mostly with time series of theta, U and other variables during the course of the event), but it is not clear to what extent atmospheric behavior during these events is generalization. Are these events the norm, or are they unusual. I feel that this severely limits the knowledge that can be gained from this work.

Specific: P2L12: "Much of the knowledge on the effects of DMC on PBL evolution has been gained from research based on the GARP" > I suggest to modify this statement, as it sounds as if this experiment delivered a majority of knowledge on the topic.

Section 2.1: Given that the study concerns DMC, the authors should expand here on their treatment of periods with rain. Rainfall and water on CSAT3 transducers impacts turbulence measurements. How was this dealt with? Are there any longer datasets available? For example, the work described in Fuentes et al has 9 levels of turbulence between 0.5 and 55 m and data is collected for $\sim$ 1 year.

P4L3: "The study period extended from 29 October 2015 to 20 November 2015" > I

have a question regarding the study period. I know that this site is used extensively for research (mainly Atmospheric Chemistry). I am a bit surprised that there is only <1 month of data available for turbulence measurements. Could the authors elaborate on the deployment of the CSAT3s.

P5L9: "Following the aforementioned procedure, four DMC events were selected for investigation" > It would be good if the authors could provide some measure of how many systems there were in total. I understand that this work more or less presents case studies, but I feel some quantification of events should be done.

Table 1: Are there other measures that could be included, such as cloud brightness temperature/ cloud top height or precipitation to get a sense of the strength. The Table caption should indicate where Vh and theta_v where measured, as well as location of RAOBS

LP6L18: "In this situation, the establishment of a shallow, cool near-surface stable layer occurs earlier than it would be the case for a typical undisturbed diurnal cycle." > This may or may not be true, but 18 LST is roughly the time of sunset, so I am not sure to what extent this really constitutes and "early nightfall" because from this work, we don't know what the normal transition looks like.

P6L30: "As the gust front impacted the tower after sunset, an early nightfall effect was also observed, similar to event 1." > I don't understand this. I thought an early nightfall means that there is no recovery since there is no additional energy input in the system that can lead to recovery, but this Figure 3b does show that theta recovers.

P7L8: "very stable stratification" > can this be quantified. if not, I suggest to remove the "very"

P8L2: "An "attempt" of a recovery phase was observed as a slight increase in theta_v around 04:00" > I don't find this very convincing. What is different at 4:00 to lets say 5:00.

Figure 3d: Why does theta_v at 55m and 40m behave so differently, between 3:30 and 5:00. Can you make sure that this is not an issue with the data.

Section 4: I feel that there are very likely methodological issues with this section. We know that CSAT3 analyzers don't work well during (strong) rain. Also, storms might introduce vibrations to tower and sensor mounts that affect 'observed' H. In summary much care needs to be taken to make sure that the findings in this section are robust. I feel that the increase in H is consistent with the cooling of the air and a surface response. At the same time, I find sustained fluxes of -800 W/m2 for several minutes surprising (Figure 4b). Especially since before and after the passage of the front, fluxes are +/- zero. I would feel much more confident, if the authors could back up their findings with a comparison to H fluxes observed during other studies. Also if fluxes are integrated to 30 minutes (which is the conventional standard). Do they make sense? This problem affects Figures 4,6,7 as all these rely on data from the CSAT3s. One indication of issues with the data is for example, that Vh changes from ∼3-4 to 10m/s (factor of 3) during the passage from the first storm, but observed TKE goes from 0.1 (?) to 6 m2/s2, which is a factor of 60. I am don't think that this is real.

Technical: P2L10: "into the surface" > "into the ABL" or "towards the surface" P3L8: "engender the venting" > affect the venting P4L34: "BLIS" > consider writing out for readability. I had already forgotten what BLIS stood for and had to look it up. P6L17: "an effective" > this does not work very well in English (since it sounds as if the nightfall is effective" Maybe: "a situation akin to an early nightfall" ?

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