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Interactive comment on "Understanding nighttime methane signals at the Amazon Tall Tower Observatory (ATTO)" *by* Santiago Botía B. et al.

Anonymous Referee #2

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Review of the manuscript by Botía et al

Title: Understanding nighttime methane signals at the Amazon Tall Tower Observatory (ATTO).

The study presents several years of CH4 mixing ratio profile measurements in the ATTO station, located in the Amazonian upland forest. The focus is on nighttime dynamics related to events when CH4 mixing ratio at 79 m inlet is higher than the one measured inside the canopy at 4 m. The dataset, spanning from June 2013 to November 2018, is unique, however the framework analysis and the structure of results/discussion could be improved. I can recommend the final publication in ACP after the following comments are properly addressed:

1) I suggest to re-structured the Results chapter. At moment, the different sub-sections include both results and discussion, and in some cases even methodology. Moreover, they are too lengthy and sometimes the reader loss the story line. So, I suggest to separate text related to results and discussion in two different sections. The chapter 3.3 and related sub-sections are mainly Discussions and not really presenting results. Finally, I see the needs to reduce the length of the text, as also suggested by the other Reviewer.

2) Most of the events associated with positive CH4 gradient are associated with strong thermal inversions, low vertical mixing and decoupled regimes. I believe the decoupling happen somewhere between the canopy layer and the 79 m height. Could the authors see any systematic wind direction difference between the two layers? This would be a clear evidence supporting the idea of decoupling layer (see Alekseychik et al (2013) for an example of such analysis related to a boreal forest in Finland).

3) The authors suggest the nighttime CH4 enhancement above the canopy (at 79m) are advected from the Uatuma river by horizontal non-turbulent motion. Assuming this is true, do the authors see also an increase of CO2 mixing ratio at 79m? In fact it is evident that rivers (even in Amazonia region) are also a strong sources of CO2 (authors can easily find few recent papers in literature related to this topic). However, for CO2 the gradient would be still negative due to strong local sources related to nighttime soil respiration.

Minor comments:

- P2L21 and L23. CH4 mixing ratio?

- P2L34. "….accumulation of CH4 above the canopy…"

- P4L25-26. I would rephrase as "….wind speed profiles for specific nights, using additional data….".

- P5L26 – P6L2. This text can go under Discussion.

- P10L4-8. This text can go under Methodology, as it is related to how the data were analysed.

- Chapter 3.2.2 and figure 9. I would move this text and figure 9 to Appendix, or even removed from the manuscript. I am not sure about the meaning of friction velocity as surface layer scaling parameter in case of decoupling and/or very shallow NBL. I would rather suggest to look at some stability parameters, like bulk Richardson number (even calculated for different layers) or z/L, which would include both the mechanical forcing related to wind shear as well as the effect of buoyancy, which could act as suppression (stable conditions) or production (unstable conditions) mechanism for turbulent mixing.

- P12L9. Do you mean above-8-ppb CH4 gradients?

- P12L9. How H is calculated? Have you corrected the kinematic heat flux <w'T'> for the effect of H2O fluctuations? And if yes, how did you measure the H2O fluctuations? Was a Licor installed at 81m?

- P12L12. The heat fluxes close to zero may even indicate near-neutral atmospheric conditions, and it depends also on the relative magnitude of the wind shear forcing. See my comments above.

- P13L8-14. Can this behavior of the friction velocity profile (indicating actually a momentum flux divergence) be checked and analysed for the present dataset?

- P14L16. Please explain the difference between regimes 1 and 2.

- P14L25-26. I was thinking if this drop in the sigma_w the authors see for relatively moderate wind speed, could be related to the fact that 1 min average values are used for such analysis, which may filter out contribution of low frequency (e.g. submeso motions) to the std values.

- P15L4-11. This part looks more like methods, and not really results. Please define the Obukhov length L somewhere in the text.

- P15L11-15. How the authors can explain nighttime unstable conditions above the canopy? I have seen some other studies reporting unstable conditions within closed canopy (even in Amazonia), but not above it, as one would expected large cooling on the top of the canopy.

- P19L34. "….in this plot differs from that…."

- Fig 4. Why nighttime is defined including (probably for some seasons) sunset periods, but excluding sunrise hours? I understand that these atmospheric boundary layer transition periods are complicated, but they are also interesting. And are these criteria for separating nighttime and daytime holding for all seasons?

References:

Alekseychik, P., Mammarella, I., Launiainen, S., Rannik, Ü., Vesala, T., 2013: Evolution of the nocturnal decoupled layer in the pine forest canopy, Agricultural and Forest Meteorology, 174-175, 15-27.

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