

egusphere-2025-6531-Review

Estimation of Nocturnal Boundary Layer Height in the Central Amazon, Supported by Gas Concentration Profiles

This manuscript by Carla Souza et al. investigates the height of the nocturnal boundary layer (h_n) above the central Amazon using micrometeorological observations from the Amazon Tall Tower Observatory. By analyzing vertical profiles of turbulent sensible heat flux up to 300 m, the authors estimate h_n more directly than many previous studies that rely on indirect proxies. The analysis focuses on seasonal differences between wet and dry periods during one La Niña year and one El Niño year. The authors report substantial variability in nocturnal boundary layer height, with the largest values occurring during the wet season of the La Niña year (approximately 270 ± 40 m) and the smallest during the dry season associated with El Niño (approximately 100 ± 27 m). They further show that variations in h_n influence the vertical distribution of CO and CH_4 , with the boundary layer height acting as a barrier that can limit the exchange of air masses containing elevated trace gas concentrations. Overall, the study provides observational evidence of strong seasonal and interannual variability in the nocturnal boundary layer over the Amazon and highlights implications for the representation of boundary layer processes and trace-gas transport in atmospheric models.

The manuscript addresses an interesting and relevant topic; however, substantial revisions are required before it can be considered for publication. In its current form, the study would benefit from more in-depth investigation of the underlying physics and in-canopy processes. The analysis appears somewhat limited, and important mechanisms are not sufficiently explored or supported. Additionally, the manuscript is relatively short for a full paper, and several limitations of the study are not adequately discussed. Expanding on these aspects would improve the rigor and clarity of the work. While I find some of the ideas promising and appreciate the effort, there are a number of significant points that must be clarified. I would like to see detailed responses to the comments provided below, as they are essential for properly evaluating the study.

Given the scope and depth of the current manuscript, I suggest that the work may be more suitable for consideration as a letter rather than a full research article. Below are my specific comments:

1 Specific comments:

- Lines 24-25: Attribute the reported differences of z_i between pastures and forests patches to the physical implications. It is also useful to define how z_i is calculated in the literature
- Line 35: z_i and not Z_i
- Line 61: Again, mention how Mendonça et al. defined the calculation of z_i
- Line 82: Define the air temperature variable symbol (T).
- Line 94: Is 30-min your Reynolds average? Any double rotation of the wind is attempted? Linear detrending ?
- Line 100: 5-min turbulence statistics .. clarify how does it differ from the above 30-min Reynolds average? Delineate clearly between QC and Reynolds averaging. Justify these choices ..
- Line 116: Justify these thresholds and the sensitivity to these thresholds.
- Line 142: There is another definition of h_n as the height at which TKE drops below a certain percentage of the measured TKE near the surface. Why this criterion is not adopted ? Also, how would the results change based on the latter? In this context and since TKE friction velocity a measure of the shear strength, did you look at Richardson number that shows the competition between buoyancy and shear?
- Line 159: Report the value $\zeta = z/L$
- Line 163: After looking at figure 3, the results look so sensitive to the choice of 5% ! Without loss of generality, if you opted to choose 6% for example, the reported values would drastically change. Any thoughts?
- Line 175: Compute the shear and relate to $\zeta = z/L$
- Lines 178-184: The authors attribute the differences in nocturnal radiative loss and boundary-layer structure primarily to variations in cloud cover (e.g., “greater cloud cover results in greater turbulent activity”). While this mechanism is physically plausible from a boundary-layer meteorology perspective, the manuscript does not present a direct metric of cloud cover to support this interpretation. The analysis appears to rely on radiation measurements (e.g., net radiation, R_n), which can indeed reflect differences in radiative cooling but do not uniquely indicate cloudiness. I therefore suggest that the authors either (i) provide additional evidence linking the radiation differences to cloud cover (e.g., through analysis of longwave radiation or another proxy for cloudiness), or (ii) rephrase the discussion more cautiously, framing cloud cover as one possible explanation for the observed radiative differences rather than a demonstrated cause.

- Line 185: Mention briefly these methods
- Line 186: Based on which method ?
- Figure 8 does not differentiate between the 2 selected dates (add respectively in the caption) ..