Response Reviewer 2: egusphere-2026-684

We thank the two referees, as well as Dr. Bruce Hicks and Dr. Peter A. Taylor, for their valuable comments and constructive suggestions. We greatly appreciate their feedback and have carefully addressed their comments in both our responses and the revised manuscript. Before addressing the individual comments, we would like to clarify the general aim and scope of our study.

The main aim of this study is to investigate whether the prototypical nocturnal boundary layer (NBL) above tropical rainforests, as commonly represented in weather and climate models, adequately captures the observed dynamics of this layer. By a prototypical or canonical NBL, we refer to the boundary layer developing above a bulk (zero-order) representation of the canopy and governed primarily by the interplay between turbulence generated by wind shear and stable thermal stratification associated with longwave radiative cooling.

Our aim is therefore to bridge the complexity of the observed nocturnal canopy–atmosphere system, influenced by thermal stratification, turbulence, and interactions across the canopy–atmosphere interface, with the necessarily simplified and relatively coarse representations commonly used in weather and climate models. We do not attempt to characterize all processes occurring within the canopy or all sources of variability in the observed NBL. Rather, we use observations to assess to what extent the canonical representation captures the main characteristics and governing processes of the NBL above the rainforest, and to identify where this simplified representation may become insufficient.

To address this question, we make use of a comprehensive observational dataset to characterize the main features and governing processes of this canonical NBL above the rainforest. Specifically, we analyse: (i) the evolution of the NBL height using different criteria that reflect the different mechanisms controlling its development (Fig. 2); (ii) the characteristic vertical profiles of turbulence and state variables (Figs. 3 and 5); (iii) the stability within and above the canopy (Fig. 4), which provides an important lower boundary condition and may introduce variability at the canopy–atmosphere interface; and (iv) the individual terms in the budgets of turbulent kinetic energy, heat, and carbon dioxide above the canopy (Figs. 6, 7, and 8). These budgets enable us to quantify, among other processes, the role of stable thermal stratification associated with longwave radiative cooling near the canopy top and the mechanical production of turbulence by wind shear.

Together, these four elements provide an observational characterization of the prototypical NBL above the rainforest and establish a framework that can be used in future work to systematically evaluate model representations of turbulent kinetic energy, potential temperature, and carbon dioxide under nocturnal boundary-layer conditions.

Regarding the more specific comment raised by the referee, we agree that atmospheric turbulence generated by wind shear plays a central role in controlling the mixing of CO_2 and heat between the canopy top and the NBL. However, a key question addressed in our study is to what extent longwave radiative cooling near the canopy top enhances local stability and thereby inhibits this turbulent mixing in the lower part of the NBL. It is precisely the interplay between these two competing mechanisms—shear-generated turbulence promoting mixing and radiatively induced stable stratification suppressing it—that we aim to characterize from the observations.

Based on all the comments, in the revised version of the manuscript, we have further strengthened this central objective throughout the paper. Below we have responded to the comments raised point by point.

If adjustments of the manuscript are indicated below the response, the newly added text is indicated in blue.

Response

1. 'Accurately representing the CO_2 exchanges ----', I think the authors need to elaborate on the challenges here.

> **Response:** Based on the comments received from all referees, we have rewritten our introduction. In doing so, we have clarified current challenges found in representing the CO_2 exchanges in weather or climate models including the active role of CO_2. Below we attach the improved section of the introduction.
>
> (L 25-34 in manuscript): During the night, net longwave radiative cooling of the canopy top leads to the formation of a stable layer, referred to as the nocturnal boundary layer (NBL) (Mahrt et al., 1979; Garstang and Fitzjarrald, 1999; Mahrt, 1999). This stratification has a direct impact on the vertical distribution of CO_2. Because of the stable thermal atmospheric stability above the canopy top, the respired CO_2 is trapped within the canopy and accumulates over time (Mahrt, 1999). The extent to which the in-canopy atmosphere and stable layer aloft interact is governed by the intricate interplay of shear-driven turbulence and buoyancy inhibition at the canopy-atmosphere interface, which can either decrease, maintain or enhance the thermal atmospheric stability (Stull, 1988; Mahrt, 1999).
>
> These canopy-atmosphere interactions are known to be non-stationary in nature, associated with intermittent bursts of turbulence (Van de Wiel et al., 2002a, b; Acevedo et al., 2008; Marth, 2011; Acevedo et al., 2016; Santos et al., 2016; Dias-Junior et al., 2017), and affected by both the characteristics and processes happening within the canopy and the roughness sublayer aloft (De Kauwe et al., 2017). The non-stationarity results from the opposing mechanisms of thermodynamic stratification by longwave radiative cooling and turbulent mixing caused by shear and lead to misrepresentations of the NBL in numerical weather and climate models. In addition, the non-stationary complicates observations with the eddy covariance (EC) technique, which are limited due to the absence of turbulence and its dependency on height, complicating the validation of nocturnal representation CO_2 exchange (Aubinet 2008; Nicolini et al., 2018) in numerical weather and climate models.

2. Paragraph in Line 50: This paragraph sounds to me like summarizing the results in the Introduction section. The authors should focus more on motivating the present study with respect to previous ones rather than stating the results in the Introduction.

> **Response:** In line with the comment above, we have decided to remove this part of the introduction. For the complete changes made to the introduction we refer to the revised manuscript.

3. Line 85: 'tall 90 tower', tall 90 tower or 325 m tower?

> **Response:** Thank you for pointing this out to us, the number 90 is misplaced. In the revised manuscript we have removed the number 90.

4. Line 85: 'Moisture and CO_2 fluxes ---', Were the moisture and CO_2 fluxes WPL-corrected?

> **Response:** In the final manuscript we have included a more elaborate data description paragraph. For the sake of completeness, we have included a brief description here. To respond

to the raised comment, for the calculation of the half hourly fluxes, the raw eddy covariance data was processed using the standard data processing procedure of Eddy Pro (LI-COR Biosciences. 2021; Fratini & Mauder 2014). The data has been corrected for outliers, detrended, double rotated, compensated for time-lags, and corrected for spectral loss, crosswind and WPL (Webb-Pearman-Leuning) density fluctuations.

5. Line 100: To not limit ourselves ---', I do not understand what the authors mean.

Response: We understand the confusion; to clarify we rewrite the line in the revised manuscript:

(L 101-102 in manuscript): Before estimating h_NBL we fitted a 2nd-order logarithmic function to the observed vertical profiles of θ, CO_2, U from the canopy top up to the highest measuring level. The 2nd-order logarithmic function was fitted using a least-squares polynomial. The fitted analytical logarithmic functions enable us to estimate h_NBL not only at the present measuring heights, but also in between the measuring levels.

6. Line 105: What sort of data limitations?

Response: The data limitation is mainly related to incomplete data series, after the 19th of August. The incomplete data series includes radiation (short- and longwave) and CO_2 flux measurements. We clarify in the revised manuscript as follows.

(L 104-105 in manuscript): We applied the criteria to the data spanning 8 to 19 August; we excluded the final two days of the field campaign because of multiple unavailable variables including longwave radiative and CO_2 flux observations.

7. 'U_g being the geostrophic wind', Is U_g really the geostrophic wind? Why do you need to specify that as a geostrophic wind, would it not suffice to just mention as the wind speed at the highest height?

Response: Thank you for pointing this out, a similar comment was written by the first referee. Therefore, we did some additional research to clarify this issue and adapt it to our data. It is explained below. We acknowledge that it is indeed not valid to use the windspeed measured at 298 m, to estimate U_g. To support this, we make use of the high-frequency soundings taken during the CloudRoots campaign.

During the measuring campaign radio soundings were released during two hours in two nights (00:00 and 03:00 LT on the 12th and 13th of August) (Fig. 1). In Fig. 1 we observe that U_g is reached around 2000 m height. Between 2000 m and 5000 m we find a four-hour average value of U_g of 10 m s^{-1}. At 298 m (top of the tower) we find a four-hour average windspeed of 4.3 m s^{-1}. From this we can conclude that we indeed cannot estimate U_g with the wind speed measured at 298 m.

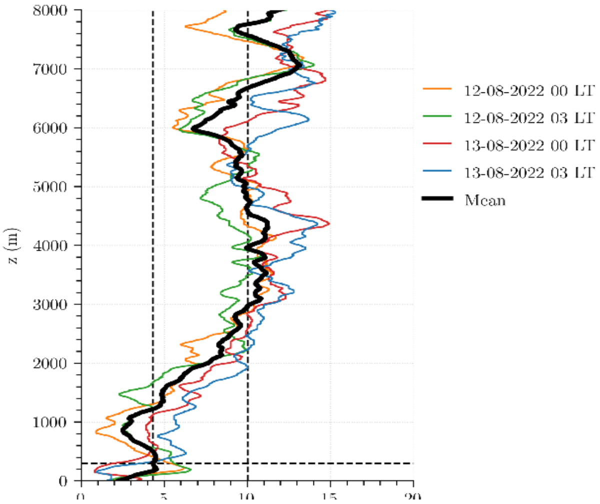

Figure 1. Radiosonde profiles of windspeed, during four periods in two nights. Here the thick black line indicates the mean of the two profiles, the horizontal dotted line the highest tower measurement at 298 m, and the vertical dotted lines the average windspeed at 298 m and the average Geostrophic windspeed between 2000 and 5000 m.

However, our aim is to estimate our h_NBL directly from our observed vertical wind profile. We have therefore calculated the h_NBL with an alternative method based on the vertical wind speed profile. Our criteria uses the vertical gradient ($\partial U/\partial z$) and estimates h_U with a similar method as for θ and CO_2. Although this definition departs from the criteria of U equal to Ug, it has the advantage to ensure consistency with the other two variables Fig. 2 (3^{rd} row).

In addition, this criterion can be further supported based on the NBL dynamics. We assume that the residual layer is well mixed because of the turbulent convection of the previous day, and therefore the wind speed is constant with height. Based on this principle we can find h_U once $\partial U/\partial z \sim 0$ ms-1. In Fig. 2 (3^{rd} row), we show the results for one night (11-12 August), the remaining nights show similar results. In this night we find values for h_U, between 100 m and 200 m, which is in accordance with our results and other studies on h_NBL utilizing ATTO data (Mendonça et al., 2025, 2026, Souza et al., 2026). In Fig. 3, we show the updated version of Fig. 2 from the manuscript.

In Fig 3. we can observe that h_U estimated with the new method closely approaches the other five methods mean. Nevertheless, we must stress one limitation, no value for h_U can be found with linear vertical wind profiles, (05:00 and 06:00 UTC in Fig. 2), as the gradient does not reach 0 m s-1, within the measuring range of the tower.

In the Manuscript, we have updated Section 2.3 and Figures 1 and 2 (Fig 3 in response) and adjusted the results (Section 3.1) to reflect the new findings.

(L 119 – 121 in manuscript): When no NLLJ is present, we used the U gradient with height (Fig. 1c). Similar to θ and CO_2, we normalized the U gradient and relaxed the threshold to 2.5 x 10^3 m^{-1}.

(L 210 in manuscript): Estimations of h_U (Fig. 2d) are found to be 156 ± 46.5 m.

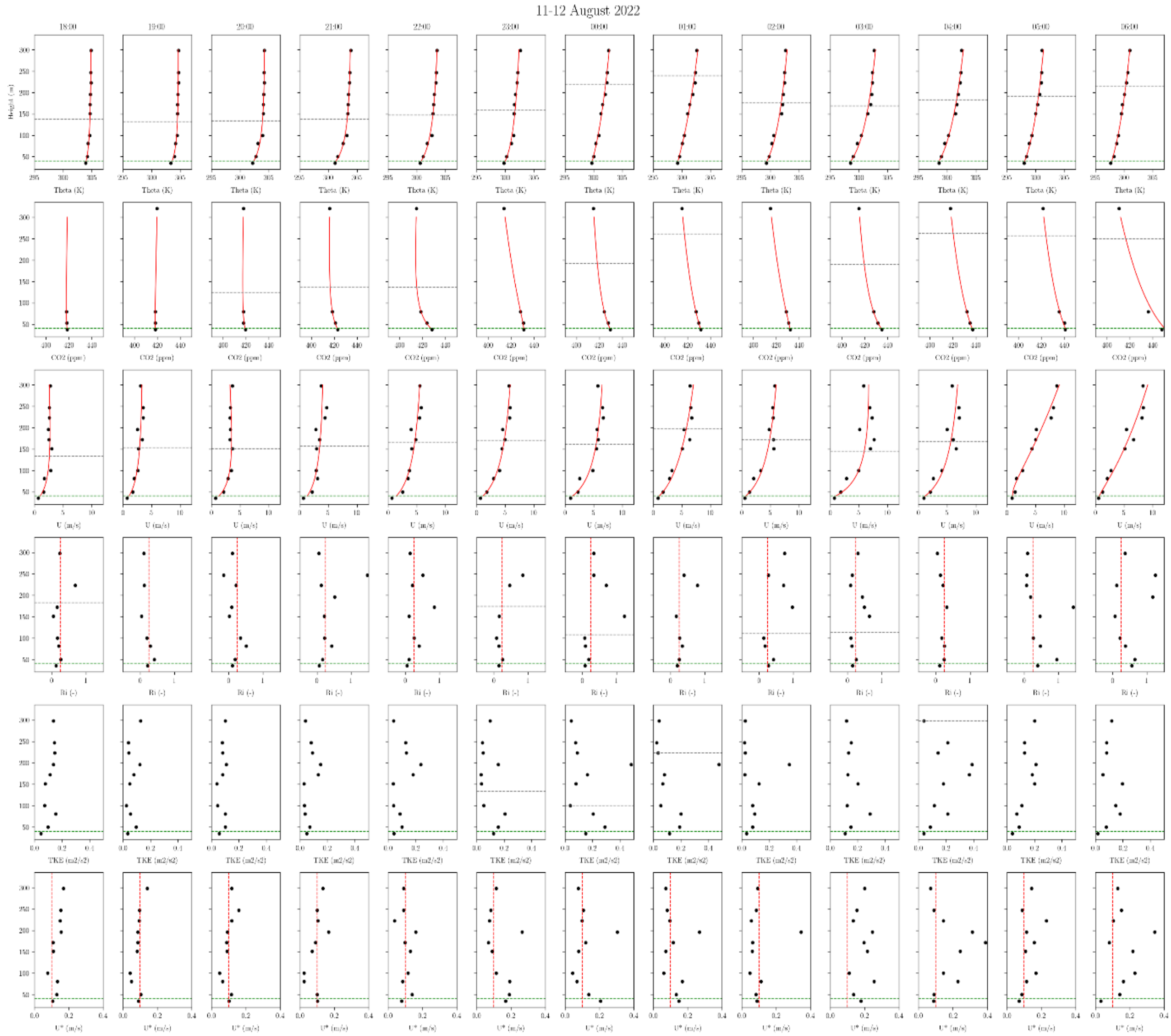

Figure 2. Vertical profiles from top to bottom: θ, CO_2, U, Ri, TKE and u_*. The black dots represent the tower observations, the red lines the fitted logarithmic profiles, the horizonal green line the canopy top, the horizontal grey line the estimated h_NBL, the vertical red line for Ri the critical Richardson number (0.25) and for U_* the threshold value of 0.1 m/s.

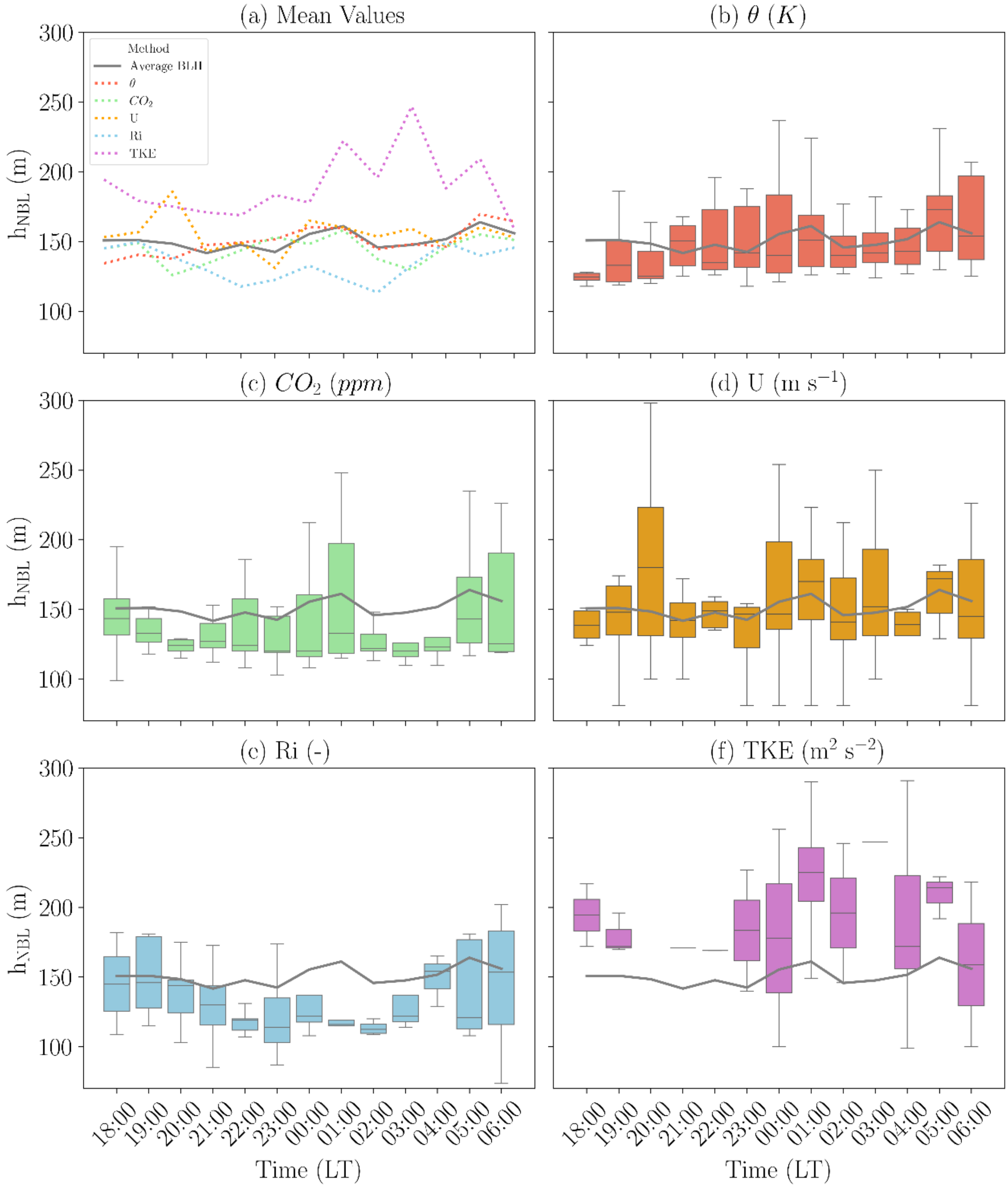

Figure 3. New version of Figure 2 is in the manuscript. Hourly nocturnal boundary layer height in LT (UTC-4), estimated for the period between 8 and 19 August 2022. Panel (a) gives the mean boundary layer height for each of the five criteria (θ, CO_2, U, Ri & TKE) and the average of those five criteria (grey line). The remaining five graphs give the variation between the nights for every hour, for (b) potential temperature (θ), (c) CO_2, (d) windspeed (U), (e) Richardson number (Ri) and (f) turbulent kinetic energy (TKE). Here, the grey line represents the five-criterion mean boundary layer height.

8. Line 145: '----- are done', This sentence reads odd.

Response: Thank you for pointing this out, it words "are done" will be removed in the final manuscript.

9. 'In addition, we aligned ---', Was this alignment done at each height? What was the turbulence intensity of the flow? If the turbulence intensities were large, this alignment can be highly inaccurate.

Response: The alignment (using the double rotation technique) has been done at each height. We acknowledge your concern and calculate the turbulence intensity of the eddy covariance data. We calculate the turbulence intensity as follows: $I = \sigma_u / \overline{U}$, where σ_u is the variance of the u-wind component and $\overline{U}$ the mean windspeed. In Fig. 4, the calculated I profiles for three nights of the CloudRoots period are shown. In most nights, the values are slightly elevated close to the canopy, this can be attributed to the rough canopy surface that introduces flow instabilities. Above the canopy layer the values approach zero, the typical profiles look similar to 8-9 August (Fig. 4 top) and 17-18 August (Fig. 4 bottom). However, there are exceptions (~10% of hours), for example during the night of 12-13 August (Fig. 4 middle) and at 05:00 LT in the night of 17-18 August. During moments with high turbulence intensity, the observations might be less accurate, however we do decide to stick to the rotation technique. To address this issue, we add a data uncertainty discussion to the general discussion. It is important to mention here that during the nights of both 12-13 and 17-18 August a nocturnal low level jet has been observed, this can possibly related to the observed increases in I.

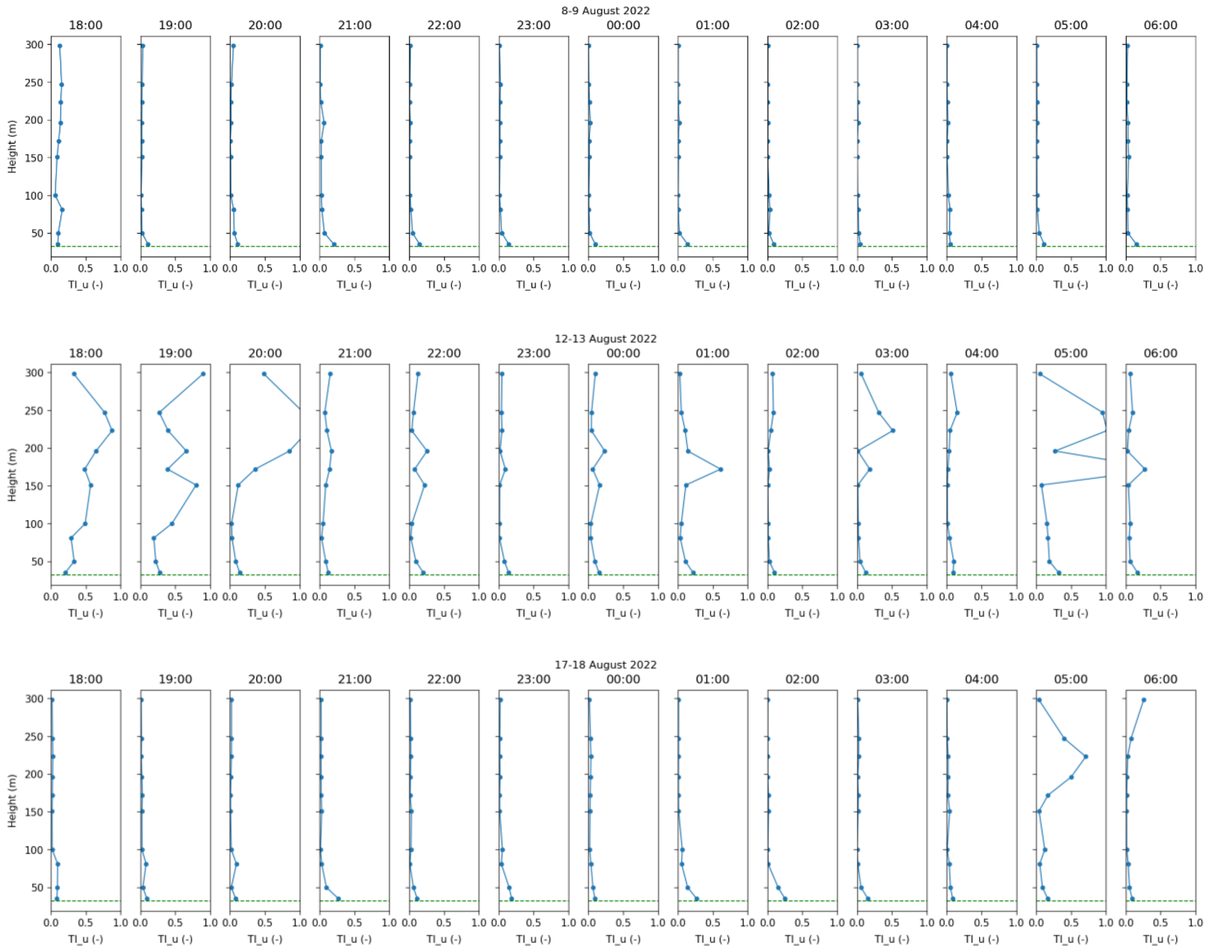

Figure 3. Vertical turbulence intensity profiles (I) for three nights of the CloudRoots Campaign. At the top 8-9 August representing the general pattern, in the middle 12-13 August representing a disturbed pattern, and at the bottom 17-18 August with a partly disturbed pattern. Important to note is that there was observed low level jet activity during the nights of 12-13 and 17-18 August.

10. Line 160: Why do you need to parametrize the dissipation rate? Can't you directly compute it from the u spectra?

Response: We justify using a parameterization of the dissipation rate as this is currently the way dissipation is calculated in weather and climate models. To solve the turbulent kinetic energy, potential temperature and CO_2 governing equations, we aim to replicate the prototypical stable profiles used in the parameterized climate models

Nevertheless, we acknowledge the large misrepresentation in the parameterized dissipation rate. Therefore, following the advice of the referee, we have calculated the dissipation rate using both the fast Fourier transform (FFT) and an Auto Regressive Moving Average model (ARMA) with the u, v, w components of the wind. The dissipation rates are calculated based on the method explained by Hartogensis & de Bruin (2005). In short, the method follows six steps: 1) the wind vector was rotated with the planar fit routine; 2) 30-min spectra of the u and v wind velocity were calculated; 3) For the ARMA method the optimal auto-regression is determines and a moving-average applied; 4) Calculate dissipation with Equation 9 in the paper; 5) performing a quality check on the spectrum and calculated dissipation values to determine whether an inertial range was present in the spectrum. 6) The average dissipation rate for the 30 min period is calculated if the slope deviated less than 20% of the theoretical 5/3 slope, and the root mean square error of the dissipation was less than 30% of its block-average value. When none of the blocks fulfilled these criteria, the dissipation value was rejected for that 30-min period.

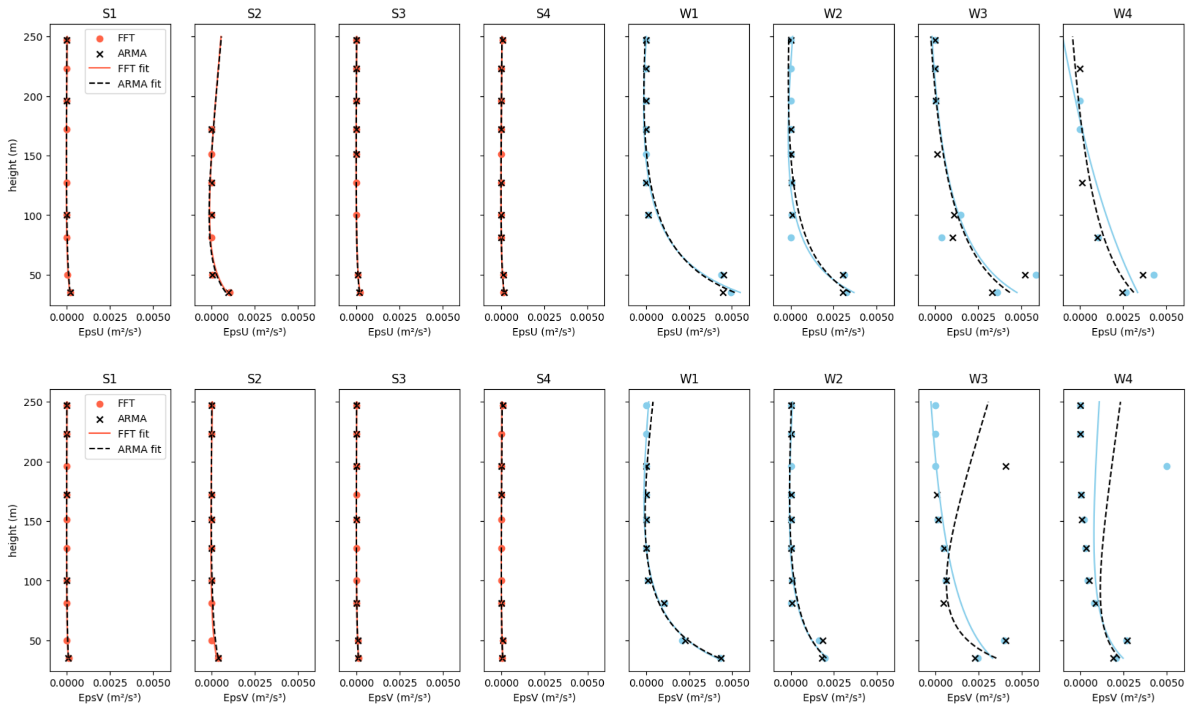

Figure 5. Dissipation rates estimated for the eight selected hours (Table 2 in manuscript). The top represents the dissipation calculated from the u-spectra, for both the FFT (dot) and ARMA (cross) method. The bottom represents the same as the top but calculated from the v-spectra. The lines are fitted through the profiles using a least-squares polynomial fit.

We find that the spectra based on the FFT and ARMA spectra for both the u and v wind components yield similar results (Fig. 5). The dissipation rates calculated from the w-wind component are not shown, because these results were less reliable due to the influence of the canopy. For the final estimation (used in revised manuscript), we have selected to use the FFT spectra from the u wind (widely used), to estimate our dissipation rate. We use the newly calculated dissipation rate to update Fig. 6 in the manuscript (Fig. 6). As expected, the absolute magnitude of our dissipation increases from about ~ -0.1 m^2 s^{-3} at the canopy top with the parameterization to ~ -4 m^2 s^{-3} (weakly stable) and ~ 1 m^2 s^{-3} (strongly stable) with the spectral method. With the new spectral calculations our dissipation is now more comparable, but with opposite sign, to the shear production rate. Consequently, the residual term largely reduces (in the weakly stable atmosphere from ~ -5 m^2 s^{-3} (with the parameterized form) to < 0.6 m^2 s^{-3} (Fig. 6)).

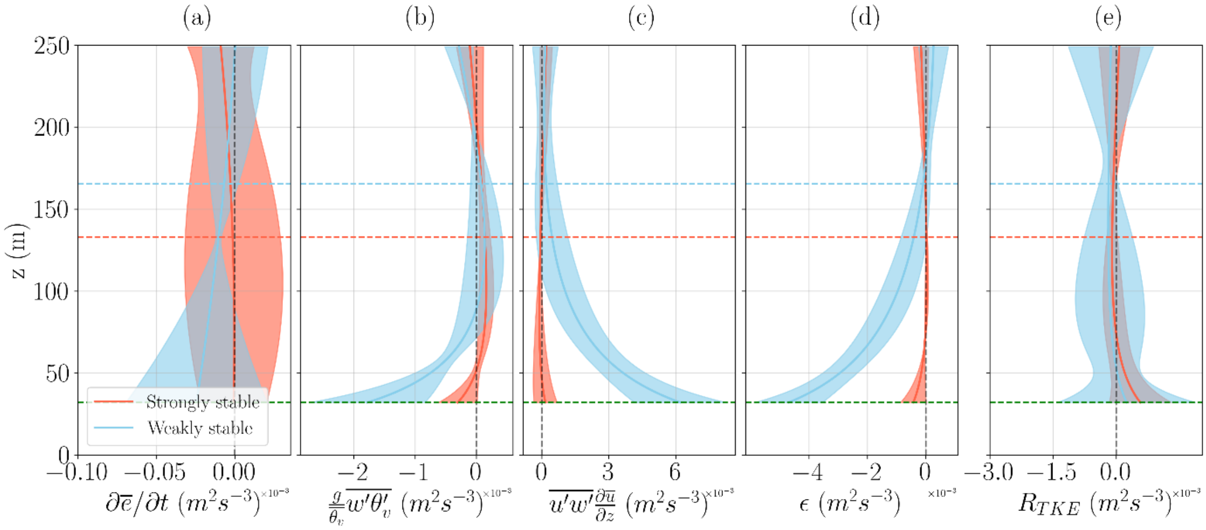

Figure 6. Fig 6. From the manuscript, but with dissipation rate calculated with the u-spectra.

11. We expected the latent heat ---', I do not understand this, since you have the moisture flux measurements why you need to invoke this assumption?

Response: To clarify, we could indeed calculate the latent heating term. However, based on an earlier study with the CloudRoots observations, we find that the latent heat flux above the canopy approaches zero during the night (González-Armas et al. 2025). In the paper there can be found that the evapotranspiration flux approaches 0 W m^{-2} s^{-1} at the beginning and end of the day (Fig. 8b in Gonzales-Armas et al., (2025)). These lower values can be related to maximum values of the water storage flux at 50 m in the early morning, suggesting a storage of moisture in the NBL canopy (Fig. C14b in González-Armas et al., (2025)). Therefore, we have assumed that the latent heat flux is neglectable on the budget (above the canopy) and have decided not to include the term.

12. Line 260: 'canopy crown', What does the +- sign refer to here?

Response: The +- sign refers to the height where the main uptake takes place, the +- indicates that this is roughly 24 meters. The exact location of the canopy crow is between 15 and 30 meters (Gomes Alves et at., 2023), where the canopy is most dense. In the revised manuscript the exact sentence has been removed due to rewriting of section 3.1. However, in the revised manuscript we clarify the location of the dense canopy crown more precisely.

(L 259-260 in revised manuscript): in the dense canopy crown, roughly between 15 and 30 meters (Gomes Alves et at., 2023).

13. Line 285: How does it decoupling parameter compare the against the previous studies, such as the one by Peltola et al. (2025)?

Response: In Peltola et al. (2025) they calculate the stability parameter as, $\Omega = \frac{\sigma_w}{w_{e,crit}}$, here $w_{e,crit}$ is the minimal vertical windspeed needed for a parcel to reach the forest floor, and σ_w the variance of the vertical wind component. In theory **Ω** approaches infinity when coupled and 0 when stable, this will be similar above the forest canopy. However, it is hard to directly compare the two stability criteria, as the one proposed in the paper is based on a two-layer system (differentiating between the stability within and above the canopy), while Ω indicates one value for both the within canopy layer and the layer above.

Additionally, it is highly unlikely that the three needed assumptions to estimate $w_{e,crit}$ hold in the dense and moist Amazon forest canopy. The following assumptions need to be fulfilled are the following: (1) humidity gradients are compared to potential temperature, and therefore the buoyancy flux can be calculated only as a function of the θ-gradients, (2) canopy density and drag are constant with height and (3) mean horizontal wind speed air parcel downward speed below canopy decay exponentially with height. Even if we assume (1) (Fig. A1 in manuscript) and (3) (Fig. 5c in manuscript) to be valid, the canopy density and with this also the drag cannot be assumed to be constant with height. In Gomes Alves et al. (2023) there can be observed that the canopy density peaks between 15 and 30 meters, and a secondary peek (~ 5 m) exists in the canopy understory.

14. Line 325: 'Dissipation below ----", This term is parameterized and therefore I have significant concerns. The authors should reevaluate this term from the velocity spectra. I am unsure why the authors state that this was beyond the scope of this study. If you have the data, this should not be too cumbersome exercise.

Response: See response to 10, here we have shown that the dissipation rate is estimated with the turbulent u-spectra

15. Line 370: Why are you attributing this residual to horizontal advection? You have not included the dissipation rate of temperature, which also would play a significant role in deciding the heat budget. This could also be done with the high-frequency temperature observations. Yes, within the canopy the temperature fluctuations could be sufficiently damped and might not show an inertial subrange but above the canopy it should be OK to see a well-behaved temperature spectra and heat flux cospectra.

16. Line 405: Again, you are ignoring the dissipation terms in the CO_2 *budget.*

Response (15+16): We base our formulations of the mean potential temperature governing equation following Stull (1988), in this book the mean equation of heat is formulated as equation 1 and simplified to equation 4 in the manuscript given the described simplifications in the manuscript.

$$\frac{\partial\bar{\theta}}{\partial t} + \bar{U}_J\frac{\partial\bar{\theta}}{\partial x_j} = -\frac{\partial\overline{u_j'\theta'}}{\partial x_j} + \frac{1}{\bar{\rho}c_p}\frac{\partial\bar{Q}}{\partial x_j} + \upsilon_0\frac{\partial^2\bar{\theta}}{\partial {x_j}^2} - \frac{\partial\overline{L_v E}}{\partial\bar{\rho}c_p} \quad (1)$$

In equation 1 υ_0 is the molecular thermal diffusivity and LvE the latent heating.

We agree with the referee that dissipation is important in the TKE budget (see response to 10). However, in the mean potential temperature budget, calculated with 30-min mean variables, we believe the dissipation is not significant in magnitude compared to the effect of turbulent transport and longwave radiative divergent cooling. The same reasoning holds for the mean CO_2 budget equation.

We believe our approximation to be sufficient, given our objective to evaluate whether a prototypical nocturnal boundary-layer parameterization, of the type commonly used in numerical weather and climate models, can reproduce the observed variability. Such model parameterizations generally represent the effects of turbulence on the mean heat and CO_2 fields through turbulent fluxes or eddy diffusivities, while scalar-dissipation terms are taken into the equation imbalance term. Therefore, we choose to calculate the same term with our observations.

Nevertheless, we agree with the referee that our residual cannot only be explained by the advection term. In the revised manuscript we will state this more clearly.

(L369 -376 in manuscript): Similar to the TKE equation, a large residual remained in the heat equation. At 50 m, the residual is 0.74 ± 0.62 K h^{-1} for the strongly stable case and 1.75 ± 0.40 K h^{-1} for the weakly stable case. Temperature advection may explain a significant part of the residual. A horizontal temperature gradient of 0.15 K km^{-1} downwind would already lead to a heat advection of about 1.0 K h^{-1} in the strongly stable regime and 1.6 K h^{-1} in the weakly stable regime at 50m height. Multiple processes can initiate horizontal temperature differences in the Amazon rainforest, for example, differences in land use or soil moisture content (Al-Kayssi et al., 1990; Gash et al., 1997). To the east of the ATTO, a swampy area is located, which likely increased nocturnal temperatures; this may explain the positive residual given the observed dominant South-eastern wind direction (Appendix B). Additionally, the residual term includes effects of temperature dissipation and measurement and calculation errors.

References

LI-COR Biosciences. (2021). Eddy Covariance Processing Software (Version 7.0.8) [Software]. Available at www.licor.com/EddyPro.

Fratini, G. & Mauder, M. (2014). Towards a consistent eddy-covariance processing: an intercomparison of EddyPro and TK3. *Atmospheric Measurement Techniques*, 7, pp. 2273-2281. DOI: https://doi.org/10.5194/amt-7-2273-2014.

Gomez Alves, E. G., Santana, R. A., Dias-Júnior, C. Q., Botía, S., Taylor, T., Yáñez-Serrano, A. M., Kesselmeier, J., Bourtsoukidis, E., Williams, J., De Assis, P. I. L. S., Martins, G., De Souza, R., Júnior, S. D., Guenther, A., Gu, D., Tsokankunku, A., Sörgel, M., Nelson, B., Pinto, D., . . . Toro, I. M. C. (2023). Intra- and interannual changes in isoprene emission from central Amazonia. *Atmospheric Chemistry and Physics*, *23*(14), 8149–8168. https://doi.org/10.5194/acp-23-8149-2023

Hartogensis, O. K., & Bruin, H. a. R. D. (2005). Monin–Obukhov similarity functions of the structure parameter of temperature and turbulent kinetic energy dissipation rate in the stable boundary layer. *Boundary-Layer Meteorology*, *116*(2), 253–276. https://doi.org/10.1007/s10546-004-2817-1

Mendonça, A. C., Dias-Júnior, C. Q., Acevedo, O. C., Marra, D. M., Cely-Toro, I. M., Fisch, G., Brondani, D. V., Manzi, A. O., Portela, B. T., Quesada, C. A., & Mortarini, L. (2025). Estimation of the nocturnal boundary layer height over the Central Amazon forest using turbulence measurements. *Agricultural and Forest Meteorology*, *367*, 110469. https://doi.org/10.1016/j.agrformet.2025.110469

Mendonça, A. C., Dias-Júnior, C. Q., De Oliveira, M. I., Maroneze, R., Martins, L. G. N., Marra, D. M., D'Oliveira, F. a. F., Costa, F. D., Fisch, G., Hall, D. H., De Oliveira, R. S., Portela, B. T., & Acevedo, O. C. (2026). Is Low-Level Jet height a good approximation for the top of the nocturnal boundary-layer? Agricultural and Forest Meteorology, 380, 11065. https://doi.org/10.1016/j.agrformet.2026.111065

Peltola, O., Aslan, T., Aurela, M., Lohila, A., Mammarella, I., Papale, D., Thomas, C. K., Vesala, T., & Laurila, T. (2024). Towards an enhanced metric for detecting vertical flow decoupling in eddy covariance flux observations. *Agricultural and Forest Meteorology*, *362*, 110326. https://doi.org/10.1016/j.agrformet.2024.110326

Souza, C., Mendonça, A. C. S., Van Asperen, H., D'Oliveira, F. A., Botía, S., Martins, L. G. N., Hall, D. H., Santana, R. A., Fisch, G., Oliveira, L. R., Mata, J. R., Figueiredo, R., Portela, B. T. T., Quesada, C. A., and Dias-Júnior, C. Q.: Estimation of Nocturnal Boundary Layer Height in the Central Amazon, Supported by Gas Concentration Profiles, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2025-6531, 2026.

Stull, R. B. (1988). *An introduction to boundary layer meteorology*. Kluwer Academic Publishers.