Response Bruce Hicks: 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 the 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.

Response

1. *An immediate reaction is that the authors have set themselves a big challenge with a limited dataset. In particular, while I applaud their attention to nocturnal intermittency, I suspect that examination of this topic is severely constrained by the sampling protocol. To be blunt, 30-minute averaging has been shown to hide most of the signal indicative of this phenomenon. In my own experience, while 30-min might satisfy old-fashioned micrometeorologists, a separate 5-min (or shorter) dataset is necessary to look into the basic processes that control the nighttime air-surface exchange of all quantities, including momentum.*

To my mind, the historic focus on log-linear profiles and their consequences is old stuff, maybe best forgotten. I am convinced that this is one of many areas where artificial intelligence will provide opportunities to delve into datasets of many kinds, including the impressive archive assembled by ATTO. My present belief is that the gradients detected at night are the result of how many intermittent occurrences occur during a 30-min period. Consequently, I look at Table 1 with a suspicion that strong stratification might be best quantified using the standard deviation of CO_2 *concentrations within 30-min periods rather than using the gradients. This would, of course, redirect much of the analysis that follows, and hence while this represents my own view of the dataset and the questions addressed in the present text, I cannot suggest anything more than the inclusion of a paragraph to address the intermittency matter a bit more thoroughly.*

My own examination of intermittency on and above forest canopies indicates that temperature is affected much as CO_2. *The signal is harder to extract, but it is still there.*

Response: We thank Dr. Hicks for this comment. We agree that nocturnal intermittency is a fundamental feature of stable boundary layers and that 30-min averaging cannot resolve the individual intermittent turbulent events that contribute to the exchange of momentum, heat, and CO_2. An analysis based on 5-min or shorter time intervals would indeed be more appropriate if the objective were to identify and characterize these individual events and their underlying mechanisms.

However, this is not the primary objective of the present study. As clarified in our general response above, our aim is to characterize the prototypical nocturnal boundary layer above the rainforest and the interplay between the main processes governing its mean structure: shear-generated turbulence and stable thermal stratification associated with longwave radiative cooling. For this purpose, we use 30-min averaged quantities consistently to characterize NBL height, vertical profiles, stability, and the different terms of the TKE, heat, and CO_2 budgets.

We would also like to clarify the role of the classification introduced in Table 1 (Table 2 revised manuscript). We apply relatively strict criteria to identify well-defined stable and weakly stable NBL conditions. We find that these criteria are generally satisfied for relatively short periods during the night (Table 2 (Table 3 revised manuscript) and therein). However, the limited duration of these periods does not necessarily imply that they are entirely characterized by intermittent turbulence. Intermittent events may occur within these periods and contribute to the variability of the 30-min averaged quantities but distinguishing such events from more persistent turbulent or stratified conditions requires an analysis at higher temporal resolution.

We nevertheless agree with the referee that intermittency may contribute substantially to the variability observed within the 30-min periods and, in particular, to the observed CO_2 and temperature gradients. The referee's suggestion of analyzing the within-period variability of CO_2 concentrations is therefore relevant. Such an analysis could provide additional information on whether strong mean gradients are associated with a small number of intense intermittent events or with more persistent stratification. However, addressing this question systematically would require an event-based analysis at substantially higher temporal resolution and represents a different research objective from that pursued here.

However, and following Dr. Hicks's suggestion, we have added a paragraph to the revised manuscript explicitly discussing this limitation of the 30-min averaging and the potential role of intermittency in both CO_2 and temperature variability. We also identify the analysis of the high-frequency ATTO observations as an important direction for future work.

2. The discussion around line 115 caused me to pause a bit. Normalizing the CO_2 *gradient must involve the* CO_2 *eddy flux determination. The latter is a 30-min average made up of a bunch of intermittent events (I suspect – prove me wrong!) as is the gradient. My mind is struggling with what happens when one considers the ratio of these two, which I think normalization does.*

Response: We thank Dr. Hicks for this comment. We understand the source of the confusion and would like to clarify that the normalization used here does not involve the CO_2 eddy flux. It is based exclusively on the vertical CO_2 molar-fraction profile.

As discussed in our response to Comment 1, intermittent turbulent events may contribute to shaping the 30-min averaged CO_2 profiles by redistributing CO_2 within the NBL. However, the purpose of the normalization is not to quantify these intermittent events or the associated turbulent exchange. Rather, it allows us to compare the shape of CO_2 profiles that have substantially different absolute concentration differences during the course of the night.

We would like to show this with the schematic example shown in Fig. 1. The profiles are not observations but represent different representative CO_2 vertical distributions during the night. Each profile is normalized using the difference in CO_2 molar fraction between the lower and upper levels of the profile. Consequently, the normalization removes differences in the absolute magnitude of the CO_2 accumulation while retaining the relative vertical structure of each profile. This makes, for example, the transition from a more nearly linear to a more curved vertical profile much easier to identify.

Mathematically, the normalization simply rescales the complete profile by a constant factor. The magnitude of the CO_2 gradient therefore changes, but its vertical structure does not: the height at which the vertical gradient approaches zero remains at the same location. This is the information that we use to estimate (h_{NBL}).

Importantly, the normalized CO_2 profiles are used only for determining (h_{NBL}). They are not used in the subsequent analysis of CO_2 fluxes or CO_2 budgets. For those analyses, we use the original, non-normalized observations. We have clarified this point in the revised manuscript and explicitly state that the normalization does not alter the height at which the CO_2 gradient approaches zero, and it helps to characterize the CO_2 profiles in terms if they are under weakly stratification (linear) or strongly stratified (logarithmic),

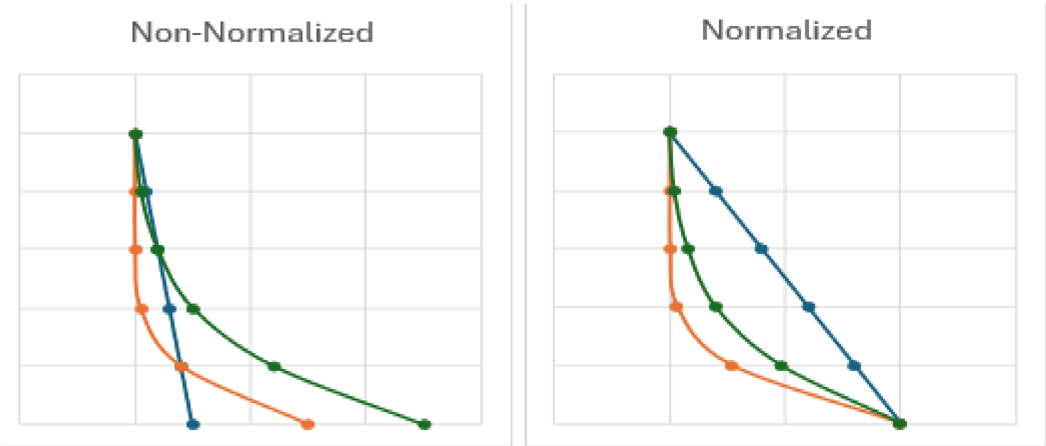

Figure 1. Ideal non-normalized and normalized profiles of carbon dioxide.

3. Equations 1 and 2 are the same but applied over different height regimes.

Response: Yes, this is true. We chose to specify both to clearly differentiate between the within and above canopy layer and the heights used to calculate both Richardson numbers.

4. Section 2.5.1 needs some clarification. I could not find a definition of v (I believe the symbol in the definition of the dissipation is Greek nu) but my own opinion is that the various relationships introduced in this paragraph are rather old and based on analyses about which I could argue because they manipulate hard-to-measure quantities in ways that lead to statistically uncertain results. In any case, why not just use the measured TKE without embellishment? Has Stull's "15" been verified?

Response: You are correct that is the kinematic viscosity of air. In the original analysis, we used a value of $1.5e^{-5}$ m^2 s^{-1}. However, following the comment of Referee #2, we have revised our approach and now estimate the TKE dissipation rate directly from the FFT-based turbulence spectrum, following Hartogensis and De Bruin (2003), rather than using the previous parameterization. We consider this spectral approach to provide a more robust estimate of the TKE dissipation rate for our observations. Further details are provided in our response to Referee #2 and the revised manuscript.

5. In Section 2.5.2 we find that results published by others, elsewhere, are used to modify actual observations prior to analysis. This was the case earlier in the manuscript and gives the impression (I hope incorrect) that results could depend on assumptions made regarding the selection of models to be used prior to data analysis. Nevertheless, the 1 – 12 W m-2 difference between model and observations is outwardly impressive, until it is realized that this is the absolute difference range. I would like to see the average difference and its standard error.

Response To clarify, the CloudRoots tower observations were not modified or used as input to the radiative transfer calculations. As explained in Section 2.5.2, we estimate the longwave radiative cooling rate (Eq. 4 in the manuscript) using the RTE+RRTMGP model. This modelling approach was necessary because radiation observations were available at only one height and therefore did not allow us to directly estimate the vertical divergence of the longwave radiative flux.

The radiative transfer calculations use ERA5 reanalysis data as input, which are downscaled using the $(LS)^2D$ model to provide the atmospheric profiles required by RTE+RRTMGP. No tower observations are used in this procedure. The CloudRoots tower observations are used only afterwards, as an independent dataset to evaluate the modelled radiation.

To address the referee's second question, the mean absolute difference between the modelled and observed net radiation is 6.92 W m^{-2}, with an associated standard error of 3.80 W m^{-2}. We have added these values to the revised manuscript.

6. Figure 3 is of great interest. In particular, I would interpret the profiles (CO_2 *as well as temperature) as indicating linearity with height within the canopy and a different linearity with height above it. This would align with experimental results elsewhere (many not yet published, so I cannot be too outspoken about this).*

Response: We agree with Dr. Hicks. The different vertical behavior within and above the canopy suggests that representing the forest as a single bulk layer, as commonly done in weather and climate models, may not adequately capture the distinct dynamics of these two layers and, importantly, their coupling at the canopy–atmosphere interface. A detailed investigation of the within-canopy dynamics is beyond the scope of the present study, which focuses primarily on the NBL above the canopy. Nevertheless, our results show the relevance of these within-canopy processes and their coupling to the overlying NBL as an important topic for future studies.

7. Section 3.2.1 poses some problems that would be solved if there was a listing of nomenclature. I finally found that Ri_c is taken to be 0.25. I consider this to be an average based on countless studies of gradients in the past, when instrumentation was not yet adequate to examine the matter in a manner that would account for chaotic variability and intermittency.

Response: We thank Dr. Hicks for this suggestion. We have now explicitly stated the adopted value of in the revised manuscript where the critical Richardson number is introduced. We agree that is a commonly adopted reference value and is not specific to the Amazon rainforest. However, this value is widely used in boundary-layer theory and model parameterizations as a threshold for the ability of shear-generated turbulence to persist under stable stratification. Its use is therefore consistent with the main objective of our study: to assess to what extent the canonical representation of the NBL used in atmospheric models is able to characterize the observed nocturnal conditions above the Amazon rainforest.

8. Section 3.3.1. Here, I cannot comment much, but once again I would like to see how you might expect nocturnal intermittency to affect the analysis and conclusions.

Response: We thank Dr. Hicks for this comment. In Section 3.3.1, our main objective is to characterize the observed vertical profiles of TKE and the dominant terms in the TKE budget. This information is particularly relevant for weather and climate models, in which TKE is commonly used to close the turbulent transport. We agree that intermittent turbulent events may contribute to the variability of the 30-min averaged TKE and its budget terms. However, resolving and characterizing these individual events is beyond the main objective of this section and would require a dedicated analysis at higher temporal resolution. We have added a short discussion acknowledging this limitation and the potential contribution of intermittency to the observed variability.

9. Line 358. I cannot relate "enhanced shear" to the "weakly stable" situation. Increased stratification means grater shear, surely. And I interpret the causative factor as being increased transport of warmer air into the canopy from aloft due to intermittency.

Response: We thank Dr. Hicks for this comment and agree that our use of the term "enhanced shear" was not sufficiently precise. A stronger vertical wind-speed gradient can indeed occur under strongly stable conditions. However, a larger mean wind shear does not necessarily imply a larger mechanical production of turbulent kinetic energy, since strong stratification suppresses vertical turbulent motions and can therefore limit the turbulent momentum transport required for shear production.

Our interpretation is based on the observed TKE budget rather than on the wind-speed gradient alone. Figure 6c shows larger mechanical production of TKE in the weakly stable regime. This enhanced TKE production supports more efficient turbulent mixing between the atmosphere above and the colder air near and within the canopy, consistent with the larger downward turbulent heat transport observed at the canopy top.

We therefore agree with the referee that the physical consequence of this turbulence is the transport of warmer air from aloft toward the colder canopy layer. Intermittent turbulent events may contribute importantly to this transport. However, with the 30-min averaged analysis used here, we cannot determine to what extent the observed heat transport results from individual intermittent bursts or from more persistent turbulence during these periods. Resolving this distinction would require the higher-frequency analysis discussed in our responses to the referee's previous comments.

To avoid ambiguity, we have replaced "enhanced shear" by "enhanced mechanical production of turbulent kinetic energy" and revised the manuscript accordingly.

Revised text (L356–359):

"The weakly stable profile showed a more negative turbulent heat-flux divergence at the canopy top (-3.62 ± 1.54 K h^{-1}) than the strongly stable profile (-1.23 ± 0.79 K h^{-1}). This difference is consistent with the enhanced mechanical production of turbulent kinetic energy observed in the weakly stable regime (Fig. 6c), which promotes turbulent mixing between the cold canopy layer and the warmer atmosphere aloft."

10. Section 3.3.3 I believe that at night a major contributor to changes in CO_2 *concentration is the variation of soil efflux. Changes in soil temperature are central issues, with changes in atmospheric pressure being suspected contributors. As soil cools during the night,* CO_2 *efflux might well decrease (in summer). In daytime, the efflux rate might decrease with increasing temperature. The temperature influence on sub-surface microbial activity is well documented in general, but doubtlessly there must be some sort of species dependence.*

Response: We agree with Dr. Hicks that temporal and spatial variability in soil CO_2 efflux can contribute to the observed nocturnal evolution of CO_2. As shown, for example, by Zanchi et al. (2012), soil CO_2 efflux in tropical forests is strongly influenced by soil temperature and moisture. Moreover, the heterogeneous Amazonian landscape, including spatial variations in soil and vegetation properties, can lead to substantial spatial variability in soil respiration.

This variability may therefore contribute to the CO_2 concentrations and gradients analyzed in Section 3.3.3 and should be considered when interpreting the CO_2 budget. However, our observations do not provide sufficient spatial coverage of soil CO_2 efflux to quantify this contribution or its variability across the footprint of the tower measurements. We have therefore added a short discussion of this limitation to the revised manuscript, while avoiding a quantitative attribution of the observed CO_2 variability to changes in soil efflux.

11. For future activities, I would recommend (1) a separate archive of 5-min average (or shorter) observations, (2) soil temperature data, (3) inclusion of discussion about soil efflux changes and (4) supporting weather data such as the strength and height of the low-level jet (when it exists).

Response: We thank Dr. Hicks for these constructive suggestions. We address the four points individually below.

1. **Shorter averaging periods.** We recognize the advantages of using shorter averaging periods for investigating nocturnal intermittency and individual turbulent events. However, for the objectives of the present study, we have retained the 30-min averaging period, which enables a consistent characterization of the mean NBL structure and the TKE, heat, and CO_2 budgets. As discussed in our responses to the referee's previous comments, a dedicated analysis using

5-min or higher-frequency observations would be valuable future work, particularly for quantifying the role of intermittency.

2. **Soil temperature.** In the revised manuscript, we have added a short discussion of the potential influence of soil temperature on soil respiration and, consequently, on the observed nocturnal CO_2 concentrations.

3. **Soil CO_2 efflux.** We have also expanded the discussion of the spatial and temporal variability of soil CO_2 efflux. This is particularly relevant for interpreting the variability in the CO_2 profiles shown in Fig. 3 and the residual term in the CO_2 budget shown in Fig. 8. However, the available observations do not provide sufficient spatial coverage to quantify this contribution across the tower footprint.

4. **Low-level jet and meteorological conditions.** In the revised manuscript, we have added further information on the meteorological conditions, including wind direction and the occurrence and characteristics of low-level jets (LLJs). An LLJ was observed for two nights (12–13 August and 17–18 August), with the jet nose located approximately between 80 and 100 m and winds predominantly from the North/North-east. These characteristics are consistent with the recent analysis of Mendonça et al. (2026), who also reported relatively low-level LLJs associated with similar wind directions.

   Finally, several of the processes highlighted by the referee motivate our ongoing work using a multi-layer canopy model coupled to the atmospheric boundary layer. This modelling framework explicitly resolves the vertical structure of the canopy and its interaction with the overlying NBL, providing an opportunity to investigate in more detail the canopy–atmosphere coupling and its response to intermittent turbulent events. We consider the observational characterization presented in the current study an important basis for evaluating such process-resolving modelling approaches.

References

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., 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

Pedruzo-Bagazgoitia, X., Patton, E. G., Moene, A. F., Ouwersloot, H. G., Gerken, T., Machado, L. a. T., Martin, S. T., Sörgel, M., Stoy, P. C., Yamasoe, M. A., & De Arellano, J. V. (2023). Investigating the diurnal radiative, turbulent, and biophysical processes in the Amazonian Canopy-Atmosphere interface by combining LES simulations and observations. *Journal of Advances in Modeling Earth Systems*, *15*(2). https://doi.org/10.1029/2022ms003210

Zanchi, F. B., Waterloo, M. J., Kruijt, B., Kesselmeier, J., Luizão, F. J., Manzi, A. O., & Dolman, A. J. (2012). Soil CO2 efflux in central Amazonia: environmental and methodological effects. *Acta Amazonica*, *42*(2), 173–184. https://doi.org/10.1590/s0044-59672012000200001