Comments on “Turbulent and non-turbulent exchange of scalars between the forest and the atmosphere at night in Amazonia”, by Oliveira et al.

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General remarks

This paper analyzes wind and scalar turbulence measurements during several nights at the ATTO project site. A great deal of the analysis is about comparisons between two specific nights. One of these nights is classified as being “fully turbulent”, and the other as displaying “intermittent turbulence”. Most of the analyses are made using multiresolution decomposition.

The results are interesting and should be useful to understand nighttime scalar exchanges between the forest and the atmosphere. However, the text needs a significant reorganization, as the comparisons between the two nights and the several heights proceed in a rather disorderly way. In this regard, I recommend that all discussions start with the turbulent night and proceed whenever possible level by level; that the same be done for the intermittent night; and that, finally, comparisons between the two nights are made. Most of the time, this should be done in different paragraphs. This will enhance readability significantly.

Moreover (“major issues”),

1. The text is ambiguous about the role of the low frequencies’ contribution to the above-canopy fluxes.

2. Gradients of temperature and velocity are being used in the Richardson numbers, but no mention to the systematic errors in the measurements between the levels is made. This should be addressed.

3. Turbulent bursts and activity are not quantitatively defined.

4. The discussion starting on p. 11, l. 5, on the turbulent regimes seems to be a repackaging of results already presented in the manuscript. It does not seem to bring any new information.

5. Clear indication must be given when only 2 nights are being compared and when all data are being used.

6. The effect of averaging per frequency without taking stability into account should be investigated.

Recommendations

In view of the above, I recommend a major review of the current manuscript.

Major issues

1. On page 2, l. 21–23, the authors say:

   > Equivalent analyses focusing on scalar flux cospectra have not been presented as often. Sakai et al. (2001) and Finnigan et al. (2003) used cospectral similarity to conclude that low-frequency contribution could account for missing energy and CO2 fluxes in their respective budgets, but neither study addressed how the cospectra varied across the canopy.

   later, on p. 2, l. 30–33, they say:

   > This result indicates that the exchange of scalars between the canopy and the atmosphere at night may occur at longer time scales than those traditionally used in the eddy covariance approach.

   and again, on p. 10, l. 10–15:

   > Our results support these findings, adding the information that the **non-turbulent contribution may dominate the exchange of CO2 and humidity from the interior of the canopy in very stable nights as well**. It is likely that the same process affects other scalars, such as O3 , whose concentrations are perturbed by intermittent events as shown in Fig. 4b.

   However, in the conclusions, they find that low-frequency components are important within the canopy, but that, above the canopy, it is the "turbulent scales" that contribute most of the flux. There seems to be a contradiction between the Introduction (and other parts of the manuscript) and the Conclusions. The introduction should not lead the reader to believe in a situation that will not be supported by the analysis.

2. "Bulk" Richardson numbers are used, but these are sensitive to velocity and, most of all, temperature systematic errors between the sensors. Because several analyses are dependent on these Richardson numbers, their reliability must be assessed quantitatively. Have the sensors been intercompared?

   In the worst case (no intercomparison, no calibration), a thorough sensitivity analysis must be made of the effects of the temperature (and wind) systematic errors on those Richardson numbers and in the analyses involving them. The reported accuracies for the sensors (assuming optimistically that they did not drift) can be used as a basis for this. The uncertainty introduced by those errors results should then be displayed graphically in all analyses regarding the Richardson number.

3. Turbulent bursts: the criterion for identifying the turbulent bursts and defining the shaded regions in Fig. 2 should be made clear (quantitavely).

4. Text starting on p. 11, l. 5, says

> Sun et al. (2012) found two regimes of nocturnal turbulence, distinguished by the turbulent kinetic energy (TKE) dependence on the mean wind speed. The fully turbulent regime, typically associated with weakly stable conditions, happens for mean wind speeds larger than a height dependent threshold and is characterized by TKE that steadily increases with wind speed. The other regime, associated with very stable conditions, has reduced turbulence intensities, which are very weakly dependent on the mean wind speed. Dias-Júnior et al. (2017) observed the two regimes above the forest at a site in the southwestern Amazon, finding that each is associated with an independent lognormal frequency distribution of quantities such as the turbulence dissipation rate. For the turbulent night of 15 November 2015 (Fig. 8, crosses), the levels of 41 and 55 m remained in the large wind speed regime for the whole period, while the two different regimes could be observed only at the 80-m level. On the intermittent night, on the other hand (Fig. 8, triangles), both regimes could be observed at all levels. Moreover, the connection intervals, given by shaded areas in Fig. 2, are generally in the large wind speed regime both at 41 m and 55 m (Fig. 8, filled triangles), while the decoupled periods are in most cases in the weak wind regime (Fig. 8, open triangles). **This is an important result, because it indicates that the intermittent bursts of turbulence observed above the canopy are intense enough to cause a regime transition. It means that, during these events, there is likely full vertical coupling over the vertical extent of the SBL (which is, at this time, shallower than 80 m). Therefore, scalars that are emitted from the canopy may be able to escape to higher levels in the boundary layer, as suggested by the episodic mixing of CO2 and O3 above 70 m shown in Fig. 4.**

   (my emphasis). But high turbulent fluxes above the canopy during the the bursts of turbulent activity are already clearly displayed in Fig 5. The "full coupling" is none other than the relatively high (absolute) values of the fluxes themselves. Given that the fluxes are there, the scalars have already "escaped" the canopy. Therefore, the reasoning in the bold-face text above seems to be rather circular, and nothing new seems to arise from this discussion. Moreover, if the criterion for identifying the bursts was TKE (as I suspect), then it is inevitable that this will be reflected in higher TKE values in Fig. 8. It appears to me that the definition of the bursts and the regime classification in Fig. 8 are one and the same, and that there is nothing to be added here. I strongly suggest deleting this whole passage.

5. Sections 4 and 5 seem to use all the data from the 15 usable nights. Because the previous section focused strongly on the comparison of the nights of Nov 14 and 15, I had a hard time (at first reading) realizing this. I suggest that both the title and the introduction of each of these sections reinforces the information that, now, data from all 15 nights are being analyzed.

6. (p. 12, l. 10): "Figure 9 shows the spectra and cospectra of the turbulent fluctuations

and fluxes averaged over the entire period".

Particularly in stable conditions, there is a strong shift of the spectra towards the higher frequencies with increasing stability (Kaimal, 1973). There is no equation describing how the spectra were "averaged", but there should be. The simplest approach (which I suspect is being used here) is to average per frequency. But then, because frequency depends on stability, different stabilities and their spectral densities are being averaged together. The consequences are far from clear to me, and this procedure should not be done without careful justification.

Remember, if $y = f(x)$ and f is nonlinear, then $\overline{y} \neq f(\overline{x})$ in general. It is not clear how the fluxes reported in Sect. 5 were calculated. Are they bin averages? Do they come from the integration of the *mean* spectra? If $F_{wa}^{(i)}$ is the flux from the $i^{\underline{\text{th}}}$ cospectrum, and if $F_{wa,\text{mean}}$ is the flux from the *mean* cospectrum (as depicted in Fig. 9), how do $(1/n)\sum_{i=1}^{n} F_{wa}^{(i)}$ and $F_{wa,\text{mean}}$ compare? In this sense, how valuable and correct are the conclusions derived from Fig. 9?

Specific comments

p. 4, l. 15–16: "Since the different levels of flow structures are analyzed simultaneously, only the data when all levels were available was used."

This should be: "…Since the different levels of flow structures are analyzed simultaneously, only the data when all levels were available **were** used".

p. 4, l. 19–20 "All the time series have been subject to quality control, which caused the removal of those series, which showed multiple spikes or spectra that did not converge to zero at the highest frequencies."

The meaning of this sentence is unclear! What does it mean for a spectrum to "converge to zero" at the highest frequencies? Turbulence spectra decay as $k^{-5/3}$ in the inertial subrange …

Do you mean spectra displaying noise in the higher frequencies? Not falling off as $k^{-5/3}$, levelling off?

Please explain.

p. 4, l. 33 – p. 5, l. 4 There appears to be a conflict of notation between C for the cospectrum and C for the concentration of CO_2.

Eq. (1) and (2) How did you calculate θ_{22}, θ_{41} and θ_{80}? From what instrument? Temperature profiles are sensitive to bias in the sensors: were the temperature sensors at these heights intercompared before deployment?

p. 5, l. 1–2 "and the standard deviation of the vertical wind component is $\sigma_w = \sum_\tau S_w$".

Wrong: the relationship is

$$\sigma_w^2 = \sum_\tau S_w.$$

Authors: check your calculations carefully to see if this is just a typo, or if you actually calculated (and are reporting) wrong values.

p. 5, l. 2–3 "Other variables, such as the Richardson number (Ri) and average horizontal wind speed (V) were calculated using the same data series used in the multiresolution decomposition."

Too vague: were the mean velocities from the sonics? Very important (see Main remarks above): from which sensors do the mean temperatures come?

p. 5, l. 22 Again, how were the θ's measured?

Section 3 Rename the section to indicate that it is about the comparison of two nights, one fully turbulent and the other intermittently turbulent. Suggestion: **Comparison of turbulence characteristics in a fully turbulent with and intermittently turbulent night.**

p. 5, l. 25–26 "The nocturnal flow at the site is characterized by the superposition of turbulent and non-turbulent fluctuations. In a fully turbulent night, such as 15 November 2015 (Fig. 1), there is a clear dominant wind direction at all levels."

Figure 1 does not show wind directions at the different levels. It is impossible to infer wind direction at each level from the figure.

p. 6, l. 3–8 and Table 1 "The most relevant difference between the two nights regards the magnitude of the turbulent mixing (Table 1). All relevant turbulence statistics are significantly larger on 15 November than on 14 November. The relative difference of the turbulence statistics between nights increases steadily in the vertical. As an example, TKE at 41 m is 3.4 times larger in the turbulent night than in the intermittent case, while at 80 m, TKE is 8.2 times larger in the turbulent night. Similar increases occur for the corresponding ratios of σ_w and u_* between the two nights."

The authors should reserve the symbol u_* for a single value in each period, which should be the most representative for the friction between the flow above the canopy and the forest. Obviously this would be the value reported at 41 m. The others are "local" values of the kinematic momentum flux, and it would be more appropriate to write them as $\sqrt{-\overline{w'u'}}$. Same comment applies for θ_*, etc..

Fig. 1-d, Fig. 2-d The title CO_2 is missing from the left vertical axis.

p. 7, l. 10 "All quantities showed much larger variation across the levels in the intermittent night (Fig. 2). Furthermore, sporadic events of coupling occurred during bursts of intermittent turbulence (Fig. 2, shaded areas)."

The authors never explain the exact quantitive criterion for the identification of the shaded areas. It *appears* to be TKE, but they should give the quantitative criterion in the text.

References

J C. Kaimal. Turbulence spectra, length scales and structure parameters in the stable surface layer. *Boundary-Layer Meteorol.*, 4:289–309, 1973.