

Interactive comment on “Nocturnal boundary layer turbulence regimes analysis during the BLLAST campaign” by Yus Díez et al.

The paper deals with observations recorded during the Boundary-Layer Late Afternoon and Sunset Turbulence (BLLAST) field campaign which took place in 2011 a few kilometres north to the Pyrenean foothill, around the *Centre de Reserches Atmosphériques* in the Lannemezan Plateau. This database has been extensively analysed during the last years with many interested papers published in high impact journals. The present paper shows an original work looking for the application of the HOST (HOckey Stick Transition) theory (Sun et al., 2012) to the BLLAST data, where the presence of heterogeneous terrain and orographic features could modulate the theory. The subject is well introduced and the paper is generally well written and structured, but I think that the next general and specific comments should be taken into account before the manuscript could be accepted. My recommendation is ‘Major Revisions’.

General comments:

- A point that is not developed either discussed in the paper is the importance of the height of the Low Level Jet (LLJ) for the different events analysed in the work. Depending if you are analysing levels above or below the LLJ the behaviour of the turbulence transport can be different showing if turbulence is connected or not with surface, or if MOST can be used (see for example Grachev et al., 2016). This issue could be connected to the different regimes that are found using the HOST theory and it can be interesting to explore it.
- When it is said that more than 60% of the flows at nIOPs come from SE quadrant and correspond to shallow drainage flows (SDF), did you test that they are really SDF? How shallow? What is the height of the LLJ found? I think that you should analyse this issue in a deeper way.
- Both in the abstract and along the paper you associate the flow coming from NW's to mesoscale or synoptic scales. Y agree with synoptic, but not with mesoscale, or at least not will all the mesoscale; for example, thermally-driven flows producing mountain breezes have their origin in the SE's and they are mesoscale flows. So, this should be revised along the paper.
- You use the data from night-time (sunset to sunrise). As stably-stratified conditions are reached before the sunset, have you done any sensitivity test to what differences can be obtained in the results if you consider for example instead the sunset, the time when sensible heat flux changes sign and becomes negative?
- In section 2 (at the end of page 4) you mention that 5 min. is used to evaluate the turbulent quantities, and you cite some references. I think that it could be

interesting to discuss a bit more the importance of using 5 min. instead of other temporal average (larger or shorter) in your study.

- I find difficult to follow the information given in Figs. 3-4, those where you show the wind roses. This is not the traditional way in which wind roses are represented (see for example Hullin et al., 2019; Fig. 2 for a better representation). By the way, I think this paper can be interesting for your present work and could be referenced. With regards to the information shown in this figures (3-4), I would like the authors to discuss more the differences found in wind direction distribution between Valimev and Skinflow towers, both for nIOPs and night-time whole dataset. For example, SE is clearly predominant for the Skinflow tower heights vs. Valimev for both datasets.

- I think it could be interesting to discuss how do you estimate the intermittency of the turbulence. I think it is not enough explained along the manuscript. Moreover, in the literature there are different definitions of turbulence intermittency, so it is important to know what you are using in the present study.

- References:

- Grachev, A. A., Leo, L. S., Di Sabatino, S., Fernando, H. J. S., Pardyjak, E. R., and Fairall, C. W.: Structure of Turbulence in Katabatic Flows Below and Above the Wind-Speed Maximum, *Bound.-Lay. Meteorol.*, 159, 469–494, 2016.
- Hulin, M., F. Gheusi, M. Lothon, V. Pont, F. Lohou, M. Ramonet, M. Delmotte, S. Derrien, G. Athier, Y. Meyerfeld, Y. Bezombes, P. Augustin, and F. Ravetta: Observations of Thermally Driven Circulations in the Pyrenees: Comparison of Detection Methods and Impact on Atmospheric Composition Measured at a Mountaintop. *J. Appl. Meteor. Climatol.*, 58, 717–740, <https://doi.org/10.1175/JAMC-D-17-0268.1> , 2019.

Specific comments:

- 1) Revise the order of the references when you are citing more than one. Generally chronological order should be used and this is not always done in the manuscript (see for example in page 4, lines 13-14; pag. 7, line 9; pag. 14, line 5;).
- 2) Pag. 2, lines 19-21: some reference could be given in relation with the TTE concept (Zilitinkevich et al., 2007, for example that you already have at the reference list).
- 3) Pag. 3, line 29: replace pikes by peaks.
- 4) Pag. 3, line 30: I think it is less than 45 km.
- 5) Pag. 4, lines 10-11: I do not understand this sentence. Could you please revise it? I do not find any relationship with the phrase that comes next.

- 6) From my point of view, the information given in pag. 5 (lines 8-16) is difficult to understand as it is, and I think that it is not necessary and could be discarded. Maybe you can reference the papers by Said et al., but not giving the detailed information that comes next. However, I missed some post-processing information of the sonic data. For example, the kind of rotation applied (double rotation, planar-fit?).
- 7) Pag. 6, lines 16-17. The reference Román-Cascón et al. (2018) is Román-Cascón et al. (2019) and the complete reference at the Reference list is also wrong (for example the title or Journal); below you have the correct one.
- 8) Pag. 6, lines 28-29: when you say at the lower levels, indicate exactly the levels considered. And in line 30, the same for the higher levels.
- 9) Pag. 7, lines 1-9. In this context, it can be interesting reference the results found by Jiménez et al. (2019).
- 10) Pag. 7, lines 5-7: you relate the occurrences of NW at higher levels with a SBL height below the Valimev tower and strong synoptic forcing. Have you check this point? Have you estimate the SBL height? From my point of view, when strong synoptic forcing is present then the nocturnal ABL height should be weakly stably-stratified and the ABL height should be quite larger than 60m.
- 11) Pag. 7, lines 7-9: I cannot see in Fig. 2a the very small valley you mention at the south of the Skinfo tower. Could you give more information on this gully (slope and its orientation)? It can be quite interesting to know it.
- 12) Pag. 7, lines 10-13: When you reference the SDF described by Román-Cascón (2015), it is said that it ranges from noon 1st July to morning 2nd July, including nIOP8. This is wrong. The period analysed in Román-Cascón et al. (2015) ranges approx. from 18:00 to 22:00 UTC on the 2nd July (IOP10), and the SDF lasts from 19:00 to 20:30 UTC approx.
- 13) Pag. 7, line 13: Change 1st July by 2nd July.
- 14) Pag. 9, lines 1-2: It is said that the MPF is from SW. However, in pag. 6, line 31 it is mentioned that MPF comes from the SE quadrant. Could you explain this contradiction?
- 15) Pag. 10, lines 30-32 and pag. 11, lines 1-2: A comment in the line of that done in comment 10; you justify that regime 2 does not behave as HOST for the 60m for the largest winds because this height could be above the top of the SBL. However, I would not expect this just for the highest winds, where the NBL height can be higher due to mechanical turbulence generated for stronger winds.
- 16) Pag. 12, lines 13-15: “the turbulence intensity can be enhanced due to the presence of coherent structures.” My question is: for what range of wind speed do you think it is more relevant the presence of coherent structures (CS) and why? In relation with this question, in this same page, lines 18-21, it would seem that you have more presence of coherent structures (internal gravity waves for example??) for larger winds, so for near-neutral conditions. Do you really think that CS are related to NBL more than SBL? Could you please clarify this in the discussion?
- 17) Pag. 12, line 26: ‘big difference’. Could you explain these differences? In line 28, when you use ‘In addition’, it seems that you are going to discuss about Skinfo tower, but you are referring to Valimev tower. Clarify it, please.
- 18) Pag. 16, lines 6-7: ‘There are few outliers in z3m and z5m since the surface smooths the quick shifts of wind speed and direction’; could you please explain better this sentence?

- 19) Pag. 17, lines 3-8: In this paragraph you are discussing the presence of outliers in the SE's directions, and in part it is related to the presence of storms and low pressure systems affecting that region. I think that at least low pressure systems are related to NW's not to SE's directions.
- 20) Pag. 19, line5: when you mention 'atmospheric disturbances', at what scales are you referring to? Are internal gravity waves or other submeso motions important in this context?
- 21) Pag. 19, lines7-9: why don't you consider SW's instead of nIOPs to illustrate the intermittency categories? In fact, you can have suitable conditions (SBL) even when an IOP is not defined in BLLAST.
- 22) Pag. 20, lines 1-11. The paper from Roman-Cascón et al. (2019) does not use BLLAST data (this is done in Román-Cascón et al. 2015), although they characterize the thermally-driven flows at the BLLAST site. So please cite properly both papers.
- 23) Pag. 20, lines10-11: 'The categories can also be found during other nIOPS.'. Please indicate explicitly those nIOPs.
- 24) Pag. 21, lines19-23: Can you indicate any references at the end of this paragraph?
- 25) Pag. 21, Fig. 12b: I cannot find the purple line, corresponding to the 30m height.
- 26) Pag. 21, Figure 12 caption: I understand that category A is related to MP flow and category B to SDF, so it would be clearer if you state: Both stages....enhancement of turbulence ...and transitions between reg 1 to reg 2 respectively.
- 27) Pag. 24, line 12-13: 'mesoscale and synoptic scale meteorological situations'. According to this statement, mountain breezes, SE's are not a mesoscale flow?
- 28) Pag. 24, lines 17: 'whole nocturnal dataset' or it should say 'whole SE's nocturnal dataset'?
- 29) Pag. 25, line 6: after C you could add 'related to turbulence intermittency'.
- 30) Pag. 25, lines 9-10: Could you explain how local shear can be generated by internal gravity waves?
- 31) Pag. 27, lines 33-35: This paper is already published and the right journal is 'Atmospheric Research' and not 'Atmospheric Environment'. Please change it.

- References:

- Jiménez, M. A., Cuxart, J., and Martínez-Villagrana, D.: Influence of a valley exit jet on the nocturnal atmospheric boundary layer at the foothills of the Pyrenees, *Q. J. Roy. Meteor. Soc.*, 145, 356–375, 2019.
- Román-Cascón, C., Yagüe, C., Arrillaga, J., Lothon, M., Pardyjak, E., Lohou, F., Inclán, R., Sastre, M., Maqueda, G., Derrien, S., Meyerfeld, Y., Hang, C., Campargue-Rodríguez, P., and Turki, I.: Comparing mountain breezes and their impacts on CO₂ mixing ratios at three contrasting areas, *Atmos. Res.*, 221, 111–126, 2019.