Responses to reviews of "*On the measurement of stability parameter over complex mountainous terrain*" by Cantero et al. wes-2021-44

Elena Cantero

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We have received two comments and reviews which are addressed in chronological order. All changes in the manuscript are clearly marked in the attached version.

RC1: Anonymous Referee #1, 18 Aug 2021
RC2: Referee #2 (Llorenç Lledó), 1 Sep 2021
AC: Author, Elena Cantero
(1) Comments from Referees
(2) Author's response
(3) Author's changes in manuscript

Referee 01

General comments

(1) RC1: The paper addresses an interesting topic for the wind energy community. In general, it is well written and structured. The objective of the study is clear.
(2) AC: Dear anonymous referee #1 many thanks for the generally positive feedback about our manuscript. Your comments are really appreciated.

Specific comments

(1) RC1: Interesting to note that unstable situations are found in the 330º-350º direction sector, which is a predominant one (page 8). If possible, please add a possible explanation for that.
(2) AC: As can be seen in figure 1, and it is explain in line119, the North face of Alaiz Mountain has a steep slope (the RIX value in the north sector in MP5 position is 22.4%) that empties into a large valley at around 700 m lower altitude. According to the reference book An Introduction to Boundary Layer Meteorology (Stull 1998) this topography causes ascending hillside/valley winds that generate convective turbulence and therefore situations of instability that could explain some of the instability found in the 330º-350º direction sector.
(3) An explanation has been added on line 278.

(1) RC1: At page 10, please clarify that the bulk Richardson number is also calculated on a 10-min interval, i.e. the same period used for the calculation of the Obukhov length.
(2) AC: Yes, the bulk Richardson number is also calculated on 10-min interval.
(3) It has been clarified on line 314.

(1) RC1: As stated in the paper, sonic anemometers are not commonly used in the wind industry, so the Bulk Richardson number method would be of great interest for the industry. As the author mentions, one of the problems here for using that method is that the mast does not have a surface temperature sensor. It would be very useful if the authors can give their opinion on whether having the surface temperature sensor would improve the accuracy of the Bulk Richardson number method and thus the consistency between the 2 analyzed methods. If possible, please also give some guidance for future studies to estimate atmospheric stability using the Bulk Richardson number.
(2) AC: According to the good results with Richardson Bulk in offshore sites where the bulk Richardson method seems to be a robust approach to characterize stability offshore ((Sanz Rodrigo et al., 2015)) and where it is calculated using sea temperature we think that having the surface temperature sensor would improve the accuracy of the Bulk Richardson number method in onshore sites. In future experiments in Alaiz, we would like, on the one hand, to use measurements of the surface temperature and, on the other, keeping the heights close to the ground, of two meters or less, use differential temperature sensors instead absolute ones to see if the results improve. With this information it will be possible to give clear guidelines on how to proceed.
(3) Recommendations have been added on line 404.

<u>Corrections</u>

(1) RC1: There is typo in table 1 and table 2 first line.
(2) AC: We will correct “parametereter” in table 1 and table 2 in a revised version of the manuscript.
(3) There are corrected on lines 94 and 116.

(1) RC1: The vertical axis title of figure 6 is incorrect. Please check vertical axis titles in other figures.
(2) AC: We will correct that in figure 6 there is no vertical title in a revised version of the manuscript. The title is “Normalized frequency”.
(3) It has changed on line 270.

(1) RC1:The acknowledgements section is incomplete.
(2) AC: We will complete the acknowledgements section in a revised version of the manuscript. The text will be “*The authors are grateful to CENER for sharing the MP5 database with us during the course of this research*.”
(3) It has been completed on line 427.

Referee 02: Llorenç Lledó

General comments

(1) RC2: The research is very relevant to both the wind energy industry and the ABL scientific community around it. Further understanding of best practices for measuring atmospheric stability in complex terrain can yield important energy cost reductions. The research is well described and reproducible.

(2) AC: Dear Llorenç, thank you very much for reviewing our manuscript and the many valuable comments and suggestions to improve the quality and clarity of the paper. Your feedback is really appreciated. We have carefully considered all your comments and, where necessary, will made changes to the manuscript accordingly.

(1) RC2: However, I lack clear recommendations at the conclusions, e.g.:
   a. Which of the three studied levels provides the best Obukhov length for estimating turbulence intensity and wind shear at hub height?
   b. Bulk Richardson number cannot be safely computed if the lower level is not at the surface (or very close, e.g. 2m temp as in this case).
   c. Which of the two methods (Obukhov length or bulk Richardson number) is recommended? What is the benefit of investing in sonic anemometers?

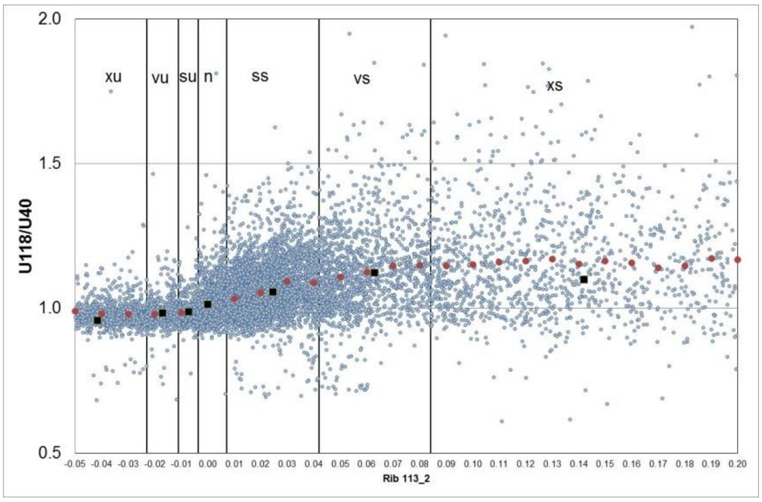
(2) AC: Yes some recommendations as you said are:
   a. The most suitable level to estimate TI and wind shear at hub height is the one closest to the hub height.
   b. Related with Bulk Richardson as it is explained in lines 382 to 384: *“The Rib number relies on smaller temperature differences for estimation of the mean gradient and its accuracy is therefore dependent on the sensor precision, calibration and measurement heights”* so, to obtain consistent results with bulk Richardson method it is required:
      i. Use high precision temperature sensors
      ii. Calibrate all the temperature sensors at the same time
      iii. Calibrate the temperature sensors in the operation range to guarantee better calibration in the temperatures of interest
      iv. Have a reference temperature sensor below 2 meters, as close to the ground as possible.
   c. We recommend the sonic method because with it we can obtain a local measurement of atmospheric stability that can be associated to a certain height above the ground. So the sonic method produces the most reliable estimate of stability since it is based on the local measurement of turbulent fluxes.

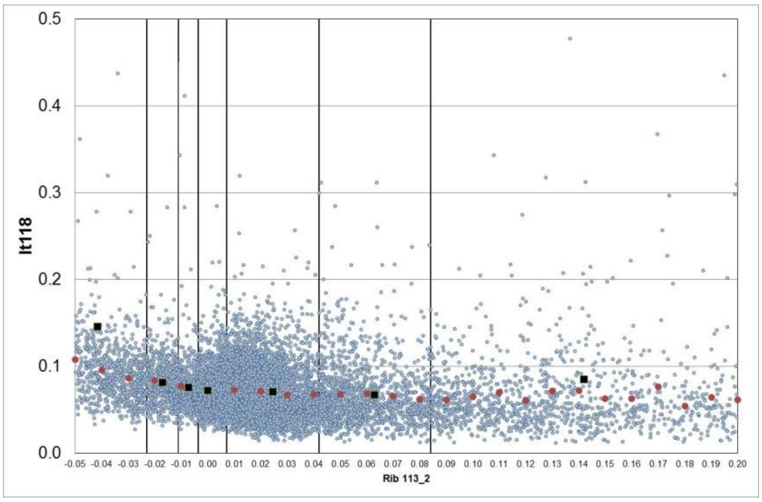
(3) Recommendations have been added on line 404 and 413.

(1) RC2: Additionally, I miss an analysis of how turbulence intensity and wind shear relate to bulk Richardson number (as in Figs. 9 & 10). It might be that although the stability description does not match the sonic one, it provides good predictive power of TI and shear at hub height, which is the ultimate goal of the stability characterization?

(2) AC: We had analyzed the behavior of the vertical profile and the intensity of turbulence with the bulk Richardson number (see below figure 1 and 2) and, yes it provides some predictive power of TI and shear at hub height, but in order to characterize the stability dependency of the wind profile it is also important to have the percentages well defined in each stability class. This is important, for instance, when assigning stability-based frequencies to a discrete number of microscale flow simulations to compute a weighted-averaged wind conditions for a wind direction sector.

(3) We have doubts about include these figures or not.





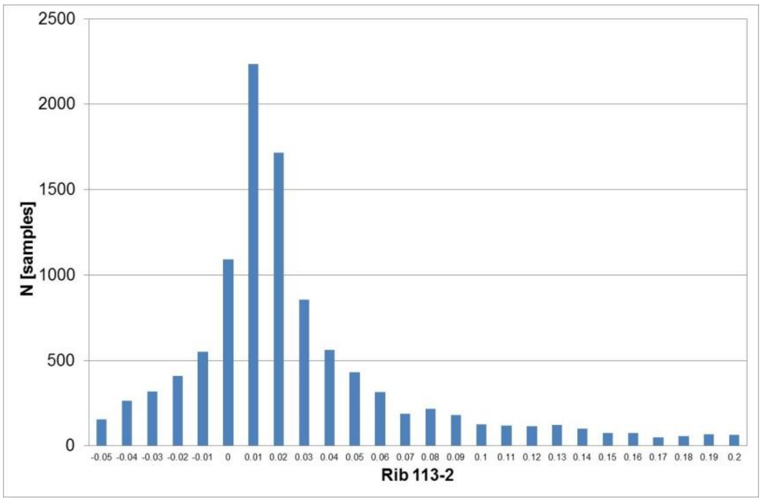
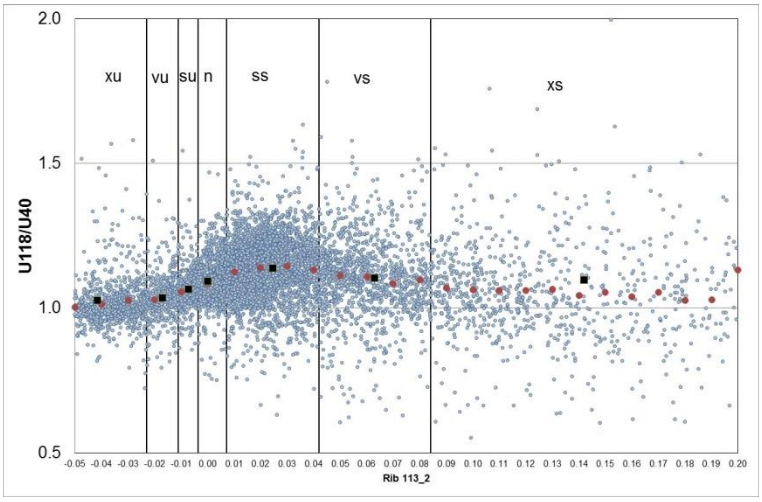
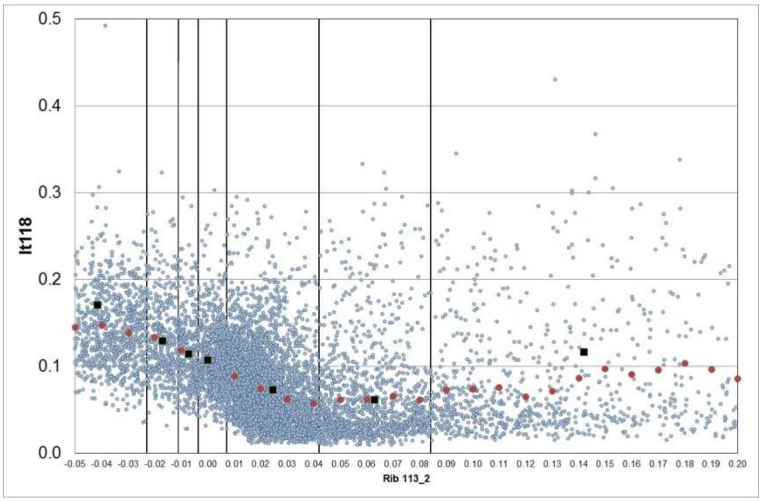


Figure 1 Wind shear and turbulence intensity vs Rib-based stability in *MP5*, [337.5º-22.5º] sector. Red dots are the *Rib* mean values for 0.01 resolution scale, black squares are the *Rib* mean values in each of the stability classes according to (Mohan, 1998).





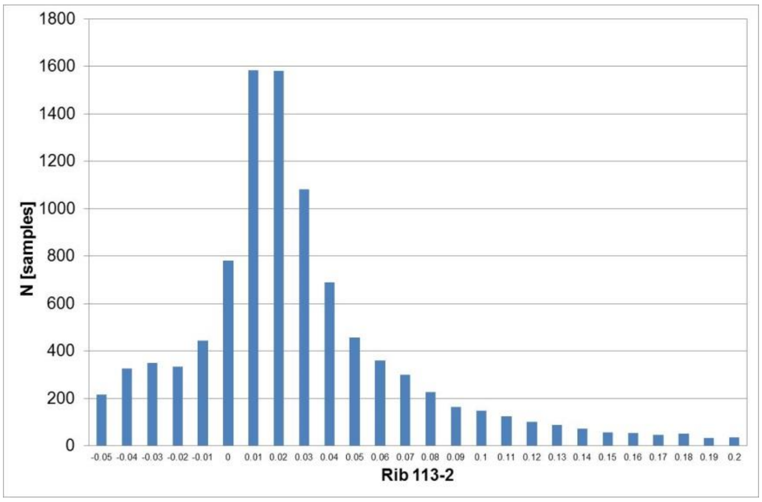


Figure 2 Wind shear and turbulence intensity vs Rib-based stability in *MP5*, [157.5º-202.5º] sector. Red dots are the *Rib* mean values for 0.01 resolution scale, black squares are the *Rib* mean values in each of the stability classes according to (Mohan, 1998).

(1) RC2: In my opinion, the number of figures could be reduced without loss of informativeness. Also, some figures are difficult to read due to size (see proposed improvements below).
(2) AC: About the figures, we are checking how optimize them for a revised article.
(3) We have improved the figures according the referee 2 specific comments, see below.

(1) RC2: The English language used and grammar is sometimes not very clear, and I recommend a revision of the language.
(2) AC: We will do it.
(3) We have done a English revision with some changes on lines 12 to 20 (abstract); 84 to 86; and according to referee 2 specific comments (see below).

Specific comments

(1) RC2: L50: Please give more details on the cost and complexity of using sonic anemometers. It would also be good to quantify the benefits of this extra effort.
(2) AC: In economic terms, the cost of sonic anemometers, without calibration, is one order of magnitude higher than the cup anemometers. The sonics models we usually use cost around 5000€, while the cup anemometers cost approximately 500€. Sonic sensor calibrations are also more expensive. About complexity, sonic sensors do not have moving parts but they carry more electronics and are therefore more sensitive to disturbances in the network, lightning, etc. Moreover the sonic is very sensitive to impacts (when handling, by hail ...) since they misadjust the distance between the transducers. However the big advantage, for stability purpose, is that using a sonic anemometer we are able to measure the turbulent fluxes and derive z/L directly. Also as is explained in (Cuerva et al., 2006) the sensors have other advantages.
(3) It has been completed on line 51.

(1) RC2: L58-59: what are the other challenges? Please describe them even if not addressed in this work.
(2) AC: The main challenges in complex terrain, which are affected by atmospheric interactions with the orography at different spatial scales, are those described in the article, the fact that the MOST is developed for horizontally homogeneous and flat terrain and in complex terrain vertical wind speed can be due to stability or sloping terrain, therefore, vertical fluxes will be "contaminated" by terrain effects. Another one, also mentioned in the article, is that small potential temperature gradients are difficult to measure accurately.
(3) No changes have been made to the manuscript.

(1) RC2: L63: Can this study be extended to the other towers in Alaiz? Or even to other instrumented sites in complex topography? How do these results for one site are expected to generalize to other sites in complex terrain?
(2) AC: Yes, we would like to have more data in complex terrain to consolidate the conclusions but, at this time, the other masts in Alaiz are not as well instrumented as the MP5. We would like, on the one hand, to use measurements of the surface temperature and, on the other, keep the heights close to the ground (below two meters) and use differential temperature sensors instead absolute ones to see how the results improve.
(3) No changes have been made to the manuscript.

(1) RC2: Table 1: please, provide the classes for the unstable range as well, with their names and abbreviations. The near-neutral class ranges from -0.02 to 0.02 I guess? This is unclear in the current description.
(2) AC: Yes the table with unstable range is:

Stability Class	Stability parameter ζ = z/L
Extremely unstable (xu)	ζ <-1
very unstable (vu)	-1 < ζ < -0.6
Unstable (u)	-0.6 < ζ <- 0.2
weakly unstable (wu)	-0.2 < ζ < -0.02
neutral (n)	-0.02 < ζ < 0.02
weakly stable (ws)	0.02 < ζ < 0.2
stable (s)	0.2 < ζ < 0.6
very stable (vs)	0.6 < ζ < 1
extremely stable (xs)	ζ > 1

(3) It has been changed in line 94, table 1.

(1) RC2: L100-101: please clarify in the methods section if heat fluxes are computed with the true vertical coordinate system, as indicated here.
(2) AC: The heat fluxes are calculated with respect to the streamline terrain-following coordinate system. This mitigates the effect of terrain inclination in the vertical component of the wind speed and it allows us to obtain heat flux values that are more comparable to those in flat terrain conditions.
(3) It is clarified on line 190.

(1) RC2: L120: Does the MP5 experience extra turbulence with south winds due to the vicinity of the Acciona wind farm?
(2) As is explained in (Sanz Rodrigo et al., 2013) the wakes from the Acciona wind farm, situated approx. 40 rotor diameters upstream, can be considered well mixed with the boundary layer flow in most conditions so we do not expect additional turbulence in MP5 due wakes from this wind farm.
(3) It is clarified on line 122.

(1) RC2: L184: what is the slope in Alaiz? What is the impact of not making this rotation? Is the method useful in places where slopes are higher and therefore there could be flow separation?
(2) AC: In the north sector in MP5 position the slope is approximately 20% and in the south sector approximately 8%. The impact of not making this rotation is that vertical wind components different from zero will appear and influence vertical fluxes as explained before ((Aubinet et al., 2012)). The streamline terrain-following coordinate system is closer to horizontally-homogeneous conditions that the Eddy covariance method requires ((Burba, 2013)). This is also true in places where flow separation could happen. Stiperski and Rotach, (2015) recommend "*a wind-speed dependent planar-fit to account for the occurrence of flow separation and the fact that flow of different speed follows a different plane".*
(3) No changes have been made to the manuscript.

(1) RC2: L205-206: this is an important premise of the study; state it early in the introduction. Explain why the flux method is expected to be more accurate than bulk Richardson, even if the MOST theory is not valid in complex terrain, as stated in (L214-217).
(2) AC: Perhaps L214-217 was misleading. Even though MOST is strictly valid in horizontally homogeneous conditions, it is a convention in the boundary-layer meteorology community to use the Obukhov length (or z/L) as a measure of local stability in complex terrain too. Taking into account this, as is explained in (Sanz Rodrigo et al., 2015), bulk Richardson is a simplification of flux Richardson number using eddy-viscosity assumptions. The Obukhov length, however, calculated with sonic measurements does not require any hypothesis, produces a local value of stability and is the most widely used stability parameter in ABL theories.
(3) No changes have been made to the manuscript.

(1) RC2: L247: If five classes are used here, I suggest naming them explicitly in table 1 instead of defining nine groups and then grouping. Otherwise, it is difficult to see which thresholds are applying.

(2) AC: We have added a new table with the groups:

Stability Class	Stability parameter $\zeta = z/L$
very unstable (vu)	$- \zeta < -0.6$
unstable (wu)	$-0.6 < \zeta < -0.02$
neutral (n)	$-0.02 < \zeta < 0.02$
stable (ws)	$0.02 < \zeta < 06$
very stable (vs)	$0.6 < \zeta$

(3) The new table is on line 259.

(1) RC2: L268: do you have any hypothesis why this is so? Could this be related to the higher wind speeds in the NW sector?

(2) AC: As can be seen in figure 1, and it is explain in line119, the North face of Alaiz Mountain has a steep slope (the RIX value in the north sector in MP5 position is 22.4%) that empties into a large valley at around 700 m lower altitude. According to (Stull, 1989) this topography causes ascending hillside/valley winds that generate convective turbulence and therefore situations of instability that could explain some of the instability found in the 330º-350º direction sector.

(3) An explanation has been added on line 278.

(1) RC2: L272-273: how are turbulence intensity and vertical shear computed? Add this to the methods section.

(2) AC: Turbulence intensity is computed as the ratio of the standard deviation to the mean wind speed and vertical shear as wind speed ratio between 118 and 40 m.

(3) An explanation is added on line 287.

(1) RC2:L280-284: please comment on the differences seen for NW and SE sectors. The TI seems to be very skewed (few large values shifting the mean) for Fig 10, and not so much in Fig 9?

(2) AC: Sorry, but we don't understand what the question refers to. TI distributions (vs stability) don’t need to be symmetric. Even though the diurnal cycle distributes stable and unstable conditions relatively uniformly around neutral conditions, each wind direction sector is affected by different wind climate and topographic conditions and this will result in differences in the distribution of wind conditions vs stability.

(3) No changes have been made to the manuscript.

(1) RC2:L307-308: can you also provide the mean difference value for the 2m sensor?
(2) AC: The mean temperature difference in the period analyzed between the level of 2m and that of 113 has been -0.02ºC.
(3) No changes have been made to the manuscript.

(1) RC2: L311: are differential temperatures sensors routinely used, or have been verified? Provide literature, please. Potentially include this in final recommendations.
(2) We have not used them but it is a recommended method to measure the difference in temperature (ΔT) between two heights above ground (Baker and Bowen, 1989; Brower, 2012).
(3) These references have been included on line 328.

(1) RC2: L320: provide table 2 already grouped in five classes, please.
(2) AC: We have added a new table with the groups:

Stability Class	Stability parameter Ri_b
Very unstable	Rib < -0.023
Unstable	-0.023 ≤ Rib < -0.0036
Neutral	-0.0036 ≤ Rib < 0.0072
Stable	0.0072 ≤ Rib > 0.084
Very stable	Rib ≥ 0.084

(3) The new table is on line 342.

(1) RC2:L324-325: it is not clear to me why those two arbitrary classifications (table 1 and table 2) should match.
(2) AC: As it is explained in (Sanz Rodrigo et al., 2015) it is relevant to define stability classes that can allow us to categorize wind conditions so we understand that a similar stability classification should be obtained with both methodologies but it doesn´t occur and it is something that we wanted to highlight.
(3) No changes have been made to the manuscript.

(1) RC2: L354: which height measurement is recommended for the sonic method?
(2) AC: It depends on the scope of the study.
(3) No changes have been made to the manuscript.

(1) RC2: L393: please compare the results obtained here with those in Sanz et al 2015 for offshore locations.
(2) For offshore sites analyzed in (Sanz Rodrigo et al., 2015) and taking the sonic method as a benchmark the bulk Richardson number reproduce reasonably well the stability distribution in the stable range and overpredicts the number of extremely unstable conditions, in the case of Alaiz, on unstable situations are similar to those obtained with sonic method.
(3) No changes have been made to the manuscript.

(1) RC2: Figure 4: use of a density line instead of a histogram would be clearer.
(2) AC: We are checking how optimize them for a revised article
(3) The new figure is on line 251.

(1) RC2: Figs 5, 6, 7, and 8: the three panels have the same y-axes and legend, unify it to gain space for the plots, no need to repeat the info.
(2) AC: We are checking how optimize them for a revised article
(3) The new figures are:
    a. On line 265 figure 5
    b. On line 270 figure 6
    c. On line 275 figure 7
    d. On line 283 figure 8

(1) RC2: Figs 6, 7, and 8 do not reveal anything out of common sense and are not used to compare the two methods. I think it is better to place them in a supplement.
(2) AC: We have doubts about move them to in a supplement
(3) No changes have been made to the manuscript.

(1) RC2: Figs 9 and 10: 7 classes are depicted but table 1 has nine classes. I suggest simplifying and using the five classes as in figs. 5 to 8.
(2) AC: About the figures, we are checking how optimize them for a revised article.
(3) The new figures are:
    a. On line 290 figure 9
    b. On line 293 figure 10

(1) RC2:Figs 9 and 10: please use the same scale for the lower row histograms.
(2) AC: About the figures, we are checking how optimize them for a revised article.
(3) The new figures are:
    c. On line 290 figure 9
    d. On line 293 figure 10

(1) RC2: Fig 12: the y-axes and legend of the two panels can be unified.
(2) AC: About the figures, we are checking how optimize them for a revised article.
(3) The new figure is on line 346.

Technical corrections

(1) RC2: L19: would -> should
(2) AC: We will correct it
(3) This sentence has changed.

(1) RC2: L23-24: sentence difficult to read
(2) AC: We will revise it
(3) It is modified on lines 23 to 26.

(1) RC2: L28 and others: some references appear in Spanish (using “y” instead of “and”), e.g. L28
(2) AC: We will revise it
(3) We have changed the language in Mendeley citation so all the references are now in English.

(1) RC2: L28: a reference uses name and surname
(2) AC: We will revise it
(3) It is correct on line 29.

(1) RC2: L30: define AEP acronym
(2) AC: We will revise it
(3) It is defined on line 31.

(1) RC2:L35: parameter -> parameters
(2) AC: We will revise it
(3) It is correct on line 36.

(1) RC2: Eq. 1 and line 80: please include ‘ in the fluxes for clarity.
(2) AC: We will revise it
(3) It is included in Eq.1 and on line 82.

(1) L84 and others: I find the “un/stable” wording difficult to read
(2) AC: We will revise it
(3) It is changed on the entire document.

(1) RC2: L106: why use parenthesis around “potential”?
(2) AC: We will revise it
(3) It is eliminated on line 106.

(1) RC2: L106: I suggest using “potential temperature vertical gradient computed by finite differences at two levels”
(2) AC: We will revise it,
(3) No change has been made to the manuscript.

(1) RC2: L107: as propose -> as proposed
(2) AC: We will revise it
(3) It is correct on line 109.

(1) RC2: L107: no need to cite twice the reference
(2) AC: We will revise it
(3) It is correct on line 108. Other references have been cited twice and them have been changed.

(1) RC2: L112-113: sentence difficult to read
(2) AC: We will revise it
(3) It is correct on lines 112 to 114.

(1) RC2: L147: measurements -> Flux measurements
(2) AC: We will revise it
(3) It is correct on line 152.

(1) RC2:L181: has -> have
(2) AC: We will revise it
(3) It is correct on line 186.

(1) RC2: L203: allowing -> allow
(2) AC: We will revise it
(3) It is correct on line 209.

(1) RC2: L220: cite equation 2
(2) AC: We will revise it
(3) It is correct on line 227.

(1) RC2: L224: specific heat -> specific heat capacity of the air. Please specify the value used.
(2) AC: We will revise it
(3) It is correct on line 231.

(1) RC2: L295-296: incomplete or unclear sentence
(2) AC: We will revise it
(3) It is correct on line 309.

(1) L304: does not -> do not
(2) AC: We will revise it
(3) It is correct on line 320.

(1) RC2: L322: propose -> proposed
(2) AC: We will revise it
(3) It is correct on line 340.

(1) RC2: L342: de -> the
(2) AC: We will revise it
(3) It is correct on line 362.

(1) RC2:L353: taking -> taken
(2) AC: We will revise it
(3) It is correct on line 373.

(1) RC2:L375: classified -> classify
(2) AC: We will revise it
(3) It is correct on line 395.

(1) RC2:L395: could -> can
(2) AC: We will revise it
(3) It is correct on line 420.

(1) RC2: L402: incomplete sentence
(2) AC: We will revise it
(3) It is correct on line 427.

References:

Aubinet, M., Vesala, T. and Papale, D.: Eddy Covariance : A practical guide to measurement and data analysis., 2012.

Baker, B. and Bowen, J.: Quality Assurance Handbook for Air Pollution Measurement Systems: Volume IV : Meteorological Measurements., 1989.

Brower, M. C.: WIND RESOURCE ASSESSMENT: A Practical Guide to Developing a Wind Project., 2012.

Burba, G.: Eddy Covariance Method for Scientific, Industrial, Agricultural and Regulatory Applications., 2013.

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Mohan, M.: Analysis of various schemes for the estimation of atmospheric stability classification, Atmos. Environ., 32(21), 3775–3781, doi:10.1016/S1352-2310(98)00109-5, 1998.

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Stiperski, I. and Rotach, M. W.: On the Measurement of Turbulence Over Complex Mountainous Terrain, Boundary-Layer Meteorol., 159(1), 97–121, doi:10.1007/s10546-015-0103-z, 2015.

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