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Comment on wes-2021-44

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Referee comment on "On the measurement of stability parameter over complex mountainous terrain" by Elena Cantero et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-44-RC2, 2021

General comments

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. However, I lack clear recommendations at the conclusions, e.g.:

- Which of the three studied levels provides the best Obukhov length for estimating turbulence intensity and wind shear at hub height?
- 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).
- Which of the two methods (Obukhov length or bulk Richardson number) is recommended? What is the benefit of investing in sonic anemometers?

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?

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).

The English language used and grammar is sometimes not very clear, and I recommend a revision of the language.

Specific comments

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.

L58-59: what are the other challenges? Please describe them even if not addressed in this work.

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?

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.

L100-101: please clarify in the methods section if heat fluxes are computed with the true vertical coordinate system, as indicated here.

L120: Does the MP5 experience extra turbulence with south winds due to the vicinity of the Acciona wind farm?

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?

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).

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.

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

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

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?

L307-308: can you also provide the mean difference value for the 2m sensor?

L311: are differential temperature sensors routinely used, or have been verified? Provide literature, please. Potentially include this in final recommendations.

L320: provide table 2 already grouped in five classes, please.

L324-325: it is not clear to me why those two arbitrary classifications (table 1 and table 2) should match.

L354: which height measurement is recommended for the sonic method?

L393: please compare the results obtained here with those in Sanz et al 2015 for offshore locations.

Figure 4: use of a density line instead of a histogram would be clearer.

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.

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.

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.

Figs 9 and 10: please use the same scale for the lower row histograms

Fig 12: the y-axes and legend of the two panels can be unified.

Technical corrections

L19: would -> should

L23-24: sentence difficult to read

L28 and others: some references appear in Spanish (using “y” instead of “and”), e.g. L28

L28: a reference uses name and surname

L30: define AEP acronym

L35: parameter -> parameters

Eq. 1 and line 80: please include ‘ in the fluxes for clarity.

L84 and others: I find the “un/stable” wording difficult to read

L106: why use parenthesis around “potential”?

L106: I suggest using “potential temperature vertical gradient computed by finite differences at two levels”

L107: as propose -> as proposed

L107: no need to cite twice the reference

L112-113: sentence difficult to read

L147: measurements -> Flux measurements

L181: has -> have

L203: allowing -> allow

L220: cite equation 2

L224: specific heat -> specific heat capacity of the air. Please specify the value used.

L295-296: incomplete or unclear sentence

L304: does not -> do not

L322: propose -> proposed

L342: de -> the

L353: taking -> taken

L375: classified -> classify

L395: could -> can

L402: incomplete sentence