

Second Review of "Estimation of turbulence parameters from scanning lidars and in-situ instrumentation in the Perdigão 2017 campaign" by Wildmann et al. 2019 (amt-2019-171)

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In this second version of the manuscript the authors included many of my comments from the first review and I think that in particular the rearrangement of the introduction and of the result sections 4 and 5 improved the clarity of the manuscript. I have few minor comments left. The page and line numbers refer to the manuscript version with the tracked changes in the authors' response.

1. p. 2, l. 15: What are turbulence models? It should rather be "turbulence parameterisation" or "numerical weather models".
2. p. 3, l. 16: What is a valley system? Do the authors mean a valley wind system?
3. p. 9, l. 3: The integral length scale describes the scale over which turbulence remains correlated (e.g. Kaimal and Finnigan, 1994). I suggest adding this verbal description.
4. p. 10, l. 27: In my first review, I asked for the number of point in the square sub-area (comment 24) to which the authors responded in their comments. However, I think that this information should be added to the manuscript as well, as it is helpful for the interested reader.
5. p. 10, l. 28: Like in the previous comment, the possible implication of the 30-min averaging intervals should be mentioned in the manuscript as well (comment 25 in the first review).
6. Fig. 6: As no data for CLAMPS are shown in (a), a legend should be plotted for each of the subplots only including the variables which are actually shown.
7. Fig. 7: The caption does not fit to (a) and (b). I believe (a) and (b) are switched, i.e. (a) is showing the results for the RHI and (b) the results for CLAMPS?
8. Figs. 7 and 8: In the captions it says "the color scale represent the density of probability of

a measurement point” (Fig. 7) and ”the probability of occurrence of a measurement point” (Fig. 8). This should be uniform.

9. p. 21, l. 25 and Fig. 11: Maybe I am missing it, but I cannot find the information where the RHI profile of dissipation rate come from. Are they averages over same area across the valley or are they individual grid point values?
10. p. 25, l. 9: ”.. and 0700 UTC (Fig. 12a).
11. p. 27, l. 1: ”...wake induced turbulence being trapped under the inversion...” I don’t see enough evidence for this in the data (comment 57 in the first review) and this should be rephrased.

References

Kaimal, J. C. and Finnigan, J. J.: Atmospheric boundary layer flows: their structure and measurement, Oxford university press, 1994.