Exploring dual-lidar mean and turbulence measurements over Perdigão's complex terrain

authored by
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Responses to comments by Reviewer 2

NOTE: Our responses to the Reviewer's comments appear in shaded text.

The manuscript compares systematically wind speed and turbulence quantities obtained from scanning Doppler wind lidar measurements in virtual mast (VM) mode with corresponding sonic anemometer measurements on co-located meteorological towers. The topic is interesting and highly relevant for a wide range of atmospheric boundary layer applications (e.g. wind energy meteorology) where our present measurement capabilities are limited by the availability and height of existing masts. Proving that lidars could extend our corresponding measurement capabilities will therefore open a wide range of new applications. The topic fits very well in the scope of AMT and I think that the manuscript can be considered for publication after some major revisions.

General comments

My two main critics are related to a) the description, handling and interpretation of the vertical velocity component and b) the analysis with respect to atmospheric stability presented in in section 4.1.

(a) It has to be carefully explained how your data have been tilt corrected, because this will strongly influence your results (see also specific comments 7b, 9 and 13). If I understand correctly, you argue that the assumption of 0 average vertical wind speed is backed up by the sonic anemometer measurements on the masts. But if you apply tilt correction to the sonics, that is of course no surprise. Only a wind speed and wind direction dependent analysis of systematic deviations could reveal what portion of the tilt is caused by instrument mis-alignment and what by potential tilt of the streamlines due to the topography. This has to be elaborated in much more detail throughout the manuscript.

RESPONSE:

The quality controlled High-rate Integrated Surface Flux System (ISFS) surface flux data, in a geographic coordinate system, and tilt corrected is available at UCAR/NCAR - Earth Observing Laboratory (2019a). According to the Data Report (UCAR/NCAR - Earth Observing Laboratory, 2019b), sonic anemometer data were tilt-corrected using DTU multistation measurements (Menke and Mann, 2017) to determine the azimuth, pitch, roll, and height of each anemometer, ensuring that the post-processed wind components were represented in geographical coordinates. The DTU multistation, composed of Leica MultiStation MS50, Leica GS14 GSNN Antenna, Leica CRT16 Bluetooth Cap, and 360° retroreflector, measured at four points on each of the installed sonic anemometers: two on the boom and two on the instrument (Menke and Mann, 2017).
Therefore, the tilt correction method that relies on the assumption of zero averaged vertical velocity was not employed in the post-processing of this data. This means that we can rely on all measured wind components of the post-processed sonic data and that no correction to the manuscript methodology is necessary. We have added an explanation of the sonic anemometer tilt correction to the manuscript (line 119).

(b) Stability is for sure a parameter to be investigated here, and I see this part of the analysis as the most important and novel investigation of your study, Unfortunately, is your use of two stability classes in my opinion not appropriate for this purpose. I suggest, to re-perform the analysis with at least 3 stability classes including a near-neutral range. In this context it would be very helpful to see a histogram of the Richardson numbers occurring in your analysis (that is a plot I really miss in the study), that then could guide you to a proper selection of the near neutral range. In case you see also a decent number of very stable and very unstable conditions, you could even consider to extend your analysis to five stability classes.

RESPONSE:

In response to the reviewer's suggestion, we have added the Ri_B histogram and its discussion to the manuscript (paragraph starting at line 354). For consistency, the atmospheric stability classification was performed following a previous multi-lidar work in Perdigão by Menke et al. (2019).
Regarding the stability classes, we acknowledge that there are different formulations of the bulk Richardson number and definitions of stability classes based on their values. However, each method presents some degree of uncertainty, and the "correct" way to classify the atmosphere's stability is still an open question, especially in complex terrain. Therefore, we opted to keep the classification into unstable ($Ri_b < 0$) and stable ($Ri_b > 0$), using the Ri_B formulation of Stull (1988).

(c) As a last general comment I suggest to rework/rephrase the introduction with respect to structure and non-precise scientific writing (I mentioned a few examples in my specific comments).

RESPONSE:

We improved the introduction in the revised manuscript.

Specific comments

1. line 45: dual RHI scanning has recently also been used for the detection and characterization of thermal updrafts in the CBL (Duscha, C., Pálenik, J., Spengler, T., and Reuder, J.: Observing atmospheric convection with dual-scanning lidars, Atmos. Meas. Tech., 16, 5103–5123, https://doi.org/10.5194/amt-16-5103-2023, 2023.); this work also documents the potential of retrieving valid data below a fixed user-defined CNR threshold (comment 9)

RESPONSE:

Thank you for pointing out this study, we have included its reference in the manuscript (line 55).

2. line 73: "University of Porto, 2020"; is there a more proper reference, e.g. once again Fernando et al.?

RESPONSE:

We have changed the reference for Fernando et al. (2019).

3. line 73: "were configured with different scanning strategies"; please rephrase, you can't configure a strategy.

RESPONSE:

Yes, this sentence was altered in the revised manuscript.

4. line 73/74: "enabling the retrieval of multi-lidar measurements"; non-precise formulation, please rephrase; you use multiple lidar measurements to retrieve some other parameters

RESPONSE:

Yes, this sentence was altered in the revised manuscript.

5. line 90: replace "on" by "in"

RESPONSE:

Thank you. This word was altered in the revised manuscript.

6. naming of the towers/virtual masts (table 1 and throughout the whole text): Do you really need the complicated double numbering/labeling; it would be much easier readable if you would go for one clear and understandable abbreviation. My suggestion WS2, WS3, ... for the WindScanners, and maybe T1, T2, T3 for the towers, that would then nicely coincide with the corresponding virtual masts VM1/2/3? As it is it is really complicated to read and requires continuous look up again.

RESPONSE:

The employed tower/WindScanner/VM names are the original names used in the Perdigão experiment, which allows for direct comparison with other works made in Perdigão.

7. line 115: can you elaborate a bit more on the pre-processing;
   a) which criteria was used for spike detection?
   b) what exactly do you mean with tilt correction (Planar Fit?, Double-rotation?, Triple-rotation?). This will have an important influence on the interpretation of the data afterwards.

RESPONSE:

The High-rate Integrated Surface Flux System (ISFS) surface flux data we used was already pre-processed by UCAR/NCAR, available at UCAR/NCAR - Earth Observing Laboratory (2019a), which is quality controlled, in geographic coordinates, and tilt corrected.

(a) The spiking detection of this pre-processed data employed the methodology from Hojstrup (1993), which is detailed in UCAR/NCAR - Earth Observing Laboratory (2024). In this procedure, a data point (x_i) is identified as a spike if it deviates from a forecasted point (x_f) by more that a discrimination level (L) times the standard deviation (σ_i): $|x_i - x_f| > L\sigma_i$.
Running statistics are used to calculate the mean (m_i), auto correlation

(c_i), and variance (ν_i) of the i-th data point. Then, the forecasted point is computed by: $x_f = x_{i-1}c_i + (1 - c_i)m_i$.
The initial discrimination level L is based on the minimum probability of a spike, typically 1×10^{-5}, and adjusted by a level factor, usually 2.5. This discrimination level is periodically updated, every 25 points, based on the auto-correlation of the data.

(b) As mentioned, sonic anemometer data were tilt-corrected by UCAR/NCAR using DTU multistation measurements of azimuth, pitch, roll, and height for each anemometer (Menke and Mann, 2017), ensuring that the post-processed wind components were represented in geographical coordinates (UCAR/NCAR - Earth Observing Laboratory, 2019b).

8. line127-128: I feel that -22dB is a very conservative threshold, can you elaborate on the amount of data you are losing by applying this threshold;

RESPONSE:

The threshold value of -22 dB was determined based on CNR vs. radial velocity plots from the multiple WindScanners. The filter was applied before the dual-lidar processing. While this does result in some data loss, improving the data/noise filtering lied beyond the scope of the article.

9. line 145: "... assuming the vertical wind component is zero (w = 0)" ; how confident are you that this assumption holds in the complex environment of Perdigao? (see also my comments 7b and 13)

RESPONSE:

Based on the sonic anemometer measurements at approximately 100 m a.g.l., the 10 min average vertical velocity did not exceed $3.6\,\mathrm{m\,s^{-1}}$ during the entire IOP. Specifically, the 10 min average vertical velocity was $0 \pm 0.5\ \mathrm{m\,s^{-1}}$ around 59 % of the IOP period at tse04/T20, 82 % at tse09/T25, and 70 % at tse13/T29 (Fig. 1). Consequently, we consider the assumption of zero vertical velocity to be valid for retrieving the wind components from dual-lidar measurements at 80 and 100 m a.g.l. in Perdigão.

10. line 198/199; " is the radial velocity error, assuming that is identical in both lidars"; Do you also assume that the error is constant along the beam?; my experience with the scanning WindCube systems is that they have an individual "focus" area where they are performing better, which could cause both distance dependent variations in the errors, as

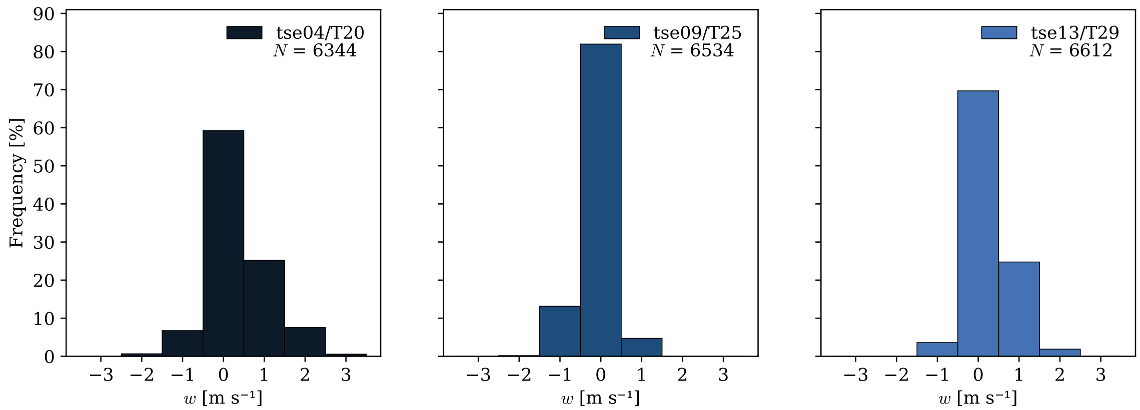


Figure 1: Histogram of the 10 min average vertical wind velocity at tse04/T20 (SW ridge), tse09/T25 (valley), and tse13/T29 (NE ridge) measurements at 100 m a.g.l. during the intensive observational period. N represents the total number of valid 10 min average measurements from the sonic anemometers at 100 m a.g.l. during the IOP.

well as differences between the different lidars. This could have considerable implications on your error estimates. Maybe you can elaborate a bit more on that, I assume that DTU has quite good control on their deployed lidars with respect to this behavior.

RESPONSE:

It is correct that the error is assumed identical for all lidars and independent of distance. We acknowledge that line-of-sight error depends on the individual lidar, the amount of backscatter in the atmosphere, the distance from the instrument, the focus position, and the instrument's temperature. However, a larger contributor to the error is the angle between the beams, and that is the focus of this discussion. We have added a sentence clarifying that in the manuscript (line 208).

11. line 211: "assuming that u and v are not correlated"; aren't u and v closely correlated by the wind direction?

RESPONSE:

For the dual-lidar error propagation of the horizontal wind speed, we assumed that the errors in u and v are uncorrelated ($\sigma_{uv} = 0$). This assumption is valid for an atmospheric boundary layer when ignoring the Coriolis force. However, we acknowledge that in a real flow over complex terrain, this assumption may be more questionable. In the manuscript, we opted for this assumption since it was used solely for an error estimate.

We have clarified this in the manuscript (line 221).

12. line 226: replace ")" by "]"

RESPONSE:

We used ")" to represent that it is an open (and not closed) interval.

13. subchapter 4.2.2 Vertical velocity (lines 359-363): What kind of are you using for the vertical velocity (see also my comment on that before in section 2.2 describing the tower data)? This could distinctly influence your results as the different tilt correction methods (that are basically designed to bring the vertical wind speed on average to zero) would cover potential systematic vertical velocities, e.g. caused by the terrain. For that it would be helpful to look into the non-corrected raw data and a potential systematic wind direction and wind speed dependent bias in the vertical velocities.

RESPONSE:

As mentioned in responses (a) and 7.b), the data were tilt-corrected using, solely, local geometrical measures to ensure the alignment between the wind components' referential and the geographical coordinates. No assumption of zero average vertical velocity was made; hence, there should be no risk of attenuating vertical velocities, or masking of the terrain or thermal effects. For this reason, we believe that it should not be necessary to evaluate biases between the pre- and post-tilt-correction data.

14. line 365 "progressively lower sampling rates": How did you lower the sampling rate, by just picking e.g. every 10th value or averaging over the ten corresponding values and using the mean for further analysis?

RESPONSE:

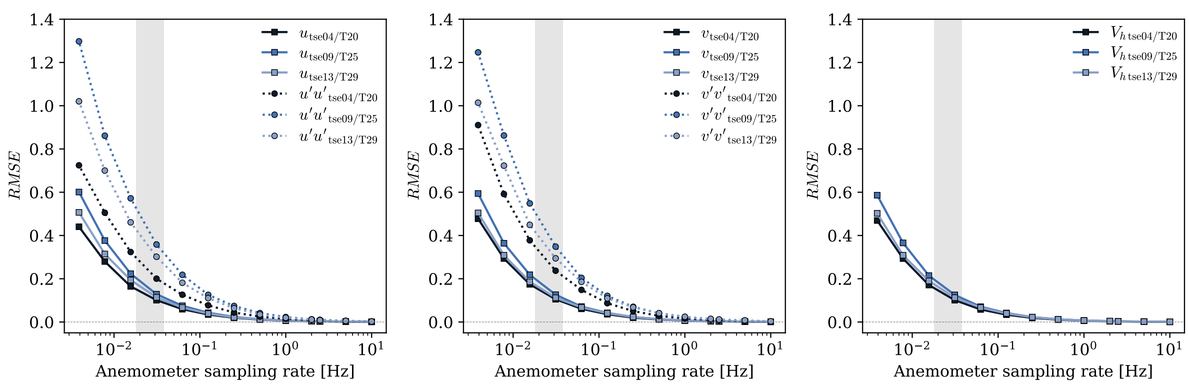
The original 20 Hz wind-component data arrays were structured as [time, sample], with time in seconds and 20 samples per second. For frequencies in the [1, 20) Hz interval, we down-sampled the data by selecting every n-th sample from the original dataset (e.g., for a frequency of 2 Hz from the 20 Hz dataset, every 10th sample was selected, as $u_{2\ \mathrm{Hz}} = u[:,0::10]$). For frequencies below 1 Hz, we selected the n-th time step from the original dataset and the first sample (e.g., for a frequency of 0.5 Hz from the 20 Hz dataset, every 2nd time step was selected, as $u_{0.5\ \mathrm{Hz}} = u[0::2,0]$). After down-sampling, we calculated the variances and averages over 10 min intervals.
An explanation of the procedure was added in the manuscript (line 400).

15. figure 9 and corresponding text lines 369-374: wouldn't it be much more straightforward/"honest" to present this (at least for the velocity) for the horizontal velocity instead of only one component to avoid any potential wind direction influence?

RESPONSE:

We wanted to represent the influence of the sampling rate on the $RMSE$ for both mean and turbulent variables. As shown in Fig. 2, the sampling rate similarly influenced the $RMSE$ of the u- and v-wind components of the same moment, and for the mean flow, the $RMSE$ of V_h exhibited results comparable to those of u and v. Therefore, we chose to present the graph for a single mean and turbulent wind component.

Nonetheless, to address potential concerns regarding wind direction influence, we have included the averaged statistical metrics for the horizontal wind speed due to sampling rate in Table 9, averaged for the three masts at 100 m a.g.l., and in the paragraph starting at line 411.



(a) (b) (c)

Figure 2: $RMSE$ of sonic measurements by the sampling rate, for the mean (u and v) and turbulent ($u'u'$ and $v'v'$) wind speed components and for the mean wind velocity (V_h), on the three 100 m towers at 100 m a.g.l. The $RMSE$ units are [m s^{-1}] for u, v, and V_h and [m^2 s^{-2}] for $u'u'$ and $v'v'$.

16. the references Menke et al., (line 498) and Pitter et al. (line 516) seem to be incomplete

RESPONSE:

Menke et al. (2018) (dataset) and Pitter et al. (2012) (conference paper) references were corrected.

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