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

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Responses to comments by Reviewer 1

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

General comments

This manuscript investigates a specific type of wind and turbulence measurement by two Doppler lidars, namely by forming a virtual mast by overlapping two coordinated Range Height Indicator (RHI) scans. Aim is provide vertical profiles of the wind and turbulence by remote sensing, i.e. without the need of a tall tower and might therefore be more cost efficient, more flexible and able to cover higher altitude.

To test this method, these dual-lidar measurements are compared with in-situ mast measurements (sonic anemometers) in a very complex environment, in general not suitable for single lidar measurements (in particular regarding turbulence), as homogenous flow conditions cannot be assumed. For this study, measurement data from a well-known Perdigao-2017 campaign is used.

Overall, the manuscript is very well written and structured. The introduction covers the many layers in terms multiple Doppler lidars usages, type of scans, type of terrain, and type of intercomparisons. As such it is clear where to put this study. The campaign and instruments are well introduced and constraints and error sources of the dual-lidar measurements are well explained, providing the relevant formulae. The results are well presented, both in graphs and in tables. This manuscripts provides a real, quantitative picture on how well two coordinated Doppler lidars can provide wind and turbulence in a real complex terrain. Also, the recommendations of the minimal sampling rate are very valuable.

I do have some minor and slightly larger comments.

My main comments are:

(A) Abstract", page 1, line 21: "Upon appraisal of the VM accuracy based on sonic anemometer measurements at 80 and 100 m a.g.l., we obtained vertical wind profiles up to 430 m a.g.l."

This point does not really come back in the remaining of the manuscript. Would it be possible to show some examples or interesting cases, in which the ability to measure beyond the mast size becomes very clear?

> RESPONSE:
>
> The primary objective of this paper is to explore and understand the capabilities and limitations of the virtual mast (VM) measurements by comparing them against anemometric measurements. While we mentioned the potential to obtain vertical profiles of the wind up to 430 m a.g.l., this study focused on validating the VM measurements at 80 and 100 m a.g.l., as these were the heights for which we had corresponding anemometric measurements.

(B) Page 7, line 156: "Upon validating their accuracy, we can use the entire dataset in further studies, assuming that the accuracy is consistent at higher levels."

The assumption of zero vertical velocity becomes more stringent for larger elevation angles (higher levels), as the vertical component of the measured radial velocity becomes larger. As such, I am not sure whether the extrapolation conclusions made a basis of a given altitude to higher altitude can simply be done. I am not convinced that one can assume that the accuracy at 80m or 100m will be the same at 400m. I think the role of elevation angle, and the increasing vertical component of measured radial velocity (or the deceasing cos(phi) terms in Eq. (1)) should at least be mentioned in this discussion.

> RESPONSE:
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> We acknowledge the concerns regarding the assumption of zero vertical velocity being less valid at higher levels, which can lead to higher errors in retrieving the $u-$ and $v-$ wind components. However, we had no anemometer measurements above 100 m, therefore, we could not evaluate the vertical velocity influence on the VM results above this height. We have included in the text the potential for increased errors at heights beyond 100 m due to higher beam elevation angles (line 162).

(C) page 20, paragraph 4.2.2. Vertical velocity

In general I think this part is way too short. Especially the sentence "However, no correlation was observed between the w values measured by sonic anemometers and the horizontal wind speed errors of the VMs" brings up many questions. First, what "w values" do you mean? 10-minute averages, nearest sample value, 10-minute variances? Considering the very local behavior of up- and downdraft and turbulence one has to be very careful in this comparison, e.g. considering the spatial mismatch between VMs and sonics. Conclusions based on a simple correlation might not be sufficient. And how do you quantify "no correlation"? Second, why only considering horizontal wind speed? Why would that be representative for the other variables (or why it would be the most sensitive)?

The assumption of zero vertical velocity is the only assumption in Eq. (1), and a major assumption in a dual-lidar virtual mast approach. I agree that with small elevation angles this assumption can be justified, although still in convective conditions with strong updrafts in combination with low wind speeds the vertical component of the radial velocity can be significant. I think it is important to stress that the conclusions drawn in this section are based on those elevation angles corresponding to virtual mast levels of 80m or 100m, but whether they are still true for 400m remains to be seen.

> RESPONSE:
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> The mentioned "w values" are 10 min averages of the vertical velocity component measured by the sonic anemometers. This information has been included in the revised manuscript (line 384).
>
> At first, we focused on horizontal wind speed in the text due to its importance in wind energy applications. We have now included the r^2 values for turbulence measurements as well (line 387). However, regardless of the flow measurement, the r^2 between VM's 10 min measurement error and the 10 min average vertical velocity did not exceed 0.110.
>
> Lastly, we have emphasised that our conclusions are based on the VM measurements at 80 and 100 m a.g.l. (paragraph starting at line 390) and that at higher heights, the vertical velocity can influence the measurements more significantly (paragraph starting at line 394).

1 Specific comments

(a) Abstract page 1, line 22 and page 22, line 428: "vertical wind profiles"

I find this way of phrasing very confusing. Does it mean profiles of vertical wind or vertical profiles of wind? I guess you mean the second one, but please use a less ambiguous way of describing what you mean.

> RESPONSE:
>
> We intended to convey "vertical profiles of the wind". We have replaced "vertical wind profiles" with "vertical profiles of the wind" in the manuscript (lines 22 and 469).

(b) Page 5, line 113: "Thermohygrometer sensors were installed at seven levels: 2 m, 10 m, 20 m, 40 m, 60 m, 80 m, and 100 m a.g.l".

Thermohygrometer might not be a very commonly known term. Maybe explicitly mentioning "temperature" and "relative humidity" sensors would be better. Also, at this point it is not motivated why these measurements are important for this study. Maybe already introduce their role in this study. Finally, you might want to provide more details on this instrument (manufacturer, type), on the same footing as the sonic anemometer.

> RESPONSE:
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> We have replaced "thermohygrometer" with "temperature/humidity sensor" (line 117). Additionally, we introduced the importance of these measurements earlier in Section 2.2 (line 113) and included more details about the temperature/humidity sensor (line 117).

(c) Page 15, Table 7: Repeat the meaning of the symbols m and b, for instance by providing again the fit formula (as provided in the main text). Also, one could add at the bottom "m is unitless".

> RESPONSE:
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> We revised Table 7 (page 15) to include the meanings of the symbols m and b, and that m is unitless in the footnote.

(d) Page 18, line 332: In the definition of the Richardson number (gradient or bulk), as given by Stull 1988 (section 5.6.2 and 5.6.3) that is also used as a reference here, the virtual potential temperature is used, not the potential temperature. This needs to be corrected. By the way, the "thermohygrometer" provides all the means to derive the virtual potential temperature.

> RESPONSE:
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> The equation is indeed for the bulk Richardson number, which we have corrected in the manuscript (lines 338 and 343).
>
> In response to the reviewer's suggestion, we have recalculated the bulk Richardson number (Ri_B) using the virtual potential temperature (Θ_v) at 2 m (Θ_{v_2}) and 100 m ($\Theta_{v_{100}}$) height, and the horizontal mean wind components measured at 100 m a.g.l. (u_{100} and v_{100}) (Stull, 1988):
>
> $$Ri_B = \frac{g(\Theta_{v_{100}} - \Theta_{v_2})\Delta z}{\Theta_{v_{100}}\left[(u_{100})^2 + (v_{100})^2\right]}. \tag{1}$$
>
> The gravitational acceleration is $g = 9.81\,\mathrm{m\,s^{-2}}$, $\Delta z = (100 - 2)$ m, and the wind speed at 2 m a.g.l. was assumed equal to zero. All values obtained from the measured fields (velocity, temperature, relative humidity, pressure) were 10-min averaged before calculating the derived quantities. For simplicity, we

forego the representation of the time-averaging operator (e.g., for potential temperature $\Theta \equiv \overline{\Theta}$). The virtual potential temperature was determined using the relation

$$\Theta_v = \Theta(1 + 0.61r), \tag{2}$$

where Θ is the potential temperature and r is the water-vapor mixing ratio of the air.

The potential temperature was calculated by:

$$\Theta = T\left(\frac{P_0}{P}\right)^{\frac{R_d}{C_p}}, \tag{3}$$

with $R_d/C_p = 0.28571$, the air temperature (T), the local surface pressure (P_0), and the local air pressure (P). Multiple barometers were employed in Perdigão-2017, but none on the 100 m masts, hence, we obtained the local air pressure from the nearest towers with the highest data availability. The selected towers contained measurements only at 2 m a.g.l (P_2), which we took as P_0.

The pressure P in the 100 m tower was calculated using the barometric formula (Lente and Ősz, 2020):

$$P = P_2\left(1 - \frac{\Gamma \Delta z_{asl}}{T_2}\right)^{\frac{gM}{R\Gamma}}, \tag{4}$$

where T_2 is the air temperature at 2 m a.g.l., $\Gamma = 0.0065\,\mathrm{K\,m^{-1}}$ is the standard atmosphere lapse rate (Stull, 2017), $M = 0.028\,964\,4\,\mathrm{kg\,mol^{-1}}$ is the average molar mass of Earth's air, and $R = 8.314\,459\,8\,\mathrm{J\,mol^{-1}\,K^{-1}}$ is the universal gas constant. $\Delta z_{asl} = z_{asl,\,t} - z_{asl,\,b}$ m accounts for terrain elevation differences between the a.s.l. heights of the temperature/humidity sensor ($z_{asl,\,t}$) and the nearest selected barometer ($z_{asl,\,b}$).

Since the humidity sensors on the 100-m towers measured the relative humidity of the air (RH), we calculated r using the vapor pressure (e) and the air pressure (P) (Stull, 2017):

$$r = 0.622\frac{e}{P - e}. \tag{5}$$

The vapor pressure was calculated by:

$$e = \frac{RH e_s}{100}, \tag{6}$$

where e_s is the saturated vapor pressure, given by:

$$e_s = e_0 \exp\left[\frac{L}{R_v}\left(\frac{1}{T_0} - \frac{1}{T}\right)\right], \tag{7}$$

with $e_0 = 0.6113 \times 10^3$ Pa, $L = 2.5 \times 10^6$ J kg^{-1}, $R_v = 461$ J kg^{-1} K^{-1}, and $T_0 = 273.15$ K.

Considering valid measurement periods, the differences between the 10 min averaged Θ_v and Θ at the three 100 m towers did not exceed 3.8 K at 100 m a.g.l. during the entire IOP. This resulted in similar Ri_B values when calculated with either Θ or Θ_v, and changes in the stability classification for only a minority of periods. However, the availability of barometric measurements reduced the number of periods for which we could calculate Ri_B and classify atmospheric stability (e.g., from 99.1 % with Θ to 64.3 % with Θ_v in tse09/T25). Consequently, we decided to retain the Ri_B calculation assuming relatively dry air conditions, using Θ instead of Θ_v. For Θ, we used the approximation $\Theta \approx T + (g/C_p)z$ (Stull, 1988), which nevertheless showed maximum differences of about 3×10^{-2} K compared to the formulation in Equation 3.

In the manuscript, we have clarified this choice and the small impact of assuming relatively dry air conditions in the paragraph starting at line 350.

(e) Page 18, line 332: "converting the mean temperature into potential temperature". Why "mean" is used in this sentence (or not twice: mean temperature to mean potential temperature)? The time averaging of the temperature data, and the conversion to (virtual) potential temperature are two separate steps. Only in the next paragraph it becomes clear that with mean temperature probably 10-minutes averaged temperature is meant.

RESPONSE:

Thank you for highlighting this point. We have altered the paragraph starting at line 343.

Technical corrections

(f) Page 11, line 213: "...except for the y-wind component measured by VM1." I guess "y-wind component" is a typo here, because throughout the manuscript u- and v-components are used.

RESPONSE:

We have replaced "y-wind component" to "v-wind component" to maintain consistency (line 223).

(g) Color usage in the various figures. The different types of blue is hard to distinguish, which is an issue for Figures 5 and 9.

RESPONSE:

We revised the figures to use more distinguishable colours.

References

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