Exploring dual-lidar mean and turbulence measurements over complex terrain

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## 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?

(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.

(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.

## 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.

(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.

(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”.

(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.

(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.

## 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.

(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.