Additional comments on *Behavior and Mechanisms of Doppler Wind Lidar Error in Varying Stability Regimes*: Clarification of WindCube v2 wind speed computation.

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Section 3.2 Ten-minute time averaged velocities

The WindCube v2 10-minute average wind speed reported by the device in the STA data is **a scalar average** of the 1 Hz horizontal wind speeds:

$$\overline{V_{scalar}} = \frac{1}{600}\sum_{i=1}^{600} V_i = \frac{1}{600}\sum_{i=1}^{600}\sqrt{u_i^2 + v_i^2}$$

The WindCube v2.1 uses a weighted linear combination of scalar and vector averaging:

$$\overline{V_{hybrid}} = \frac{1}{3}\overline{V_{vector}} + \frac{2}{3}\overline{V_{scalar}}$$

This is to point out that there **are not any WindCubes (except a few early v2.1's) reporting pure 10-minute vector averages in the STA data, as in your paper.** The vector-averaged data is available today in VSTA files on board the device, and can be computed from the 1 Hz LOS RTD data, but typical uses use the average wind speeds in the STA files.

Vector lidar and pointwise measurements (implicitly mimicking a sonic anemometer) or full vector wind field averages are shown (dashed lines, Figure 8, et al). Are the *pointwise* 10-minute averages computed using vector-averaged u, v, and w components? I assume yes, but this should be clarified.

It would be very interesting to compare the 10-minute scalar- and vector-averaged pointwise measurements to 10-minute scalar- and vector-averaged lidar measurements. Here's why:

In the Rosenbusch et al (2021) article, the differences between scalar and vector wind field reconstruction for pulsed DBS lidars (with WindCube scan geometry and timing) when compared to scalar-averaged cup anemometry were shown, theoretically, to depend on the correlation between *u, v,* and *w* turbulent components, thus likely strongly influenced by stability, just as you've divided your data. The data used in Rosenbusch was restricted to comparisons between scalar *cup* averages and scalar and vector lidar averages. The theory developed in Rosenbusch et al implies that 10-minute vector averages of pulsed DBS lidars with WindCube scan geometry <u>should not</u> exhibit systematic, WFR-caused biases when compared to vector-averaged pointwise measurements (though RWF biases may exist, as you observe).

On the other hand, it shows that the 10-minute scalar-averaged lidar measurements <u>should</u> exhibit systematic high biases when compared to scalar-averaged pointwise measurements, and that this bias should vary in different stability regimes. As in your paper, this is shown in propagation of the turbulent decomposition through the WFR algorithm developed by Jennifer Newman. **This contradicts (or at least restricts to vector WFR) an observation in your paper:**

> Line 545-546 : *"For the most part, the error mean biases can be attributed to RWF effects and the velocity perturbation terms do have close to zero mean, but important deviations from that assumption do arise"*

The scalar WFR case should show systematic biases due to the velocity perturbation term, especially in the convective cases.

Your dataset is ready to make these scalar-to-vector, and scalar-to-scalar comparisons between the lidar and pointwise measurement. I think it would be a valuable addition to the paper. Adding the scalar-averaged pointwise ("cup-like") measurements would expand the scope of your results to another sensor type and constitute a more comprehensive first result using this simulation data. I believe it would increase the impact, as well, due to the ubiquity of cup anemometry in wind energy. Treatment of uncertainties for cup anemometry (and for lidar) is covered in multiple IEC standards (61400-12-1, -15-1, -15-2, 50-3, 50-4, et al). This topic of sensor uncertainty and error is of great importance for the wind energy industry, and I think your simulation framework is a breakthrough.

One last thought is that adding the 10-minute scalar averages would also allow for direct propagation of the 1 Hz errors to 10-minutes through the scalar averaging equation, a way to connect those two sections of the paper more strongly. This would require an interesting treatment of the covariance between the neighboring 1 Hz measurements, which share 1, 2, or 3 LOS measurements, essentially the covariance of a moving average (and not only the wind itself).

Best regards,
Andrew