Referee's comments to wes-2023-6

Main comments

This study represents an important contribution to the topic of experimental wind energy, proposing a new approach to measure the flow around a wind turbine.

The literature is fairly review and the authors cite some of the most relevant studies in the field.

The reader is referred to previous publication for the experimental strategy, although it would be nice to introduce a paragraph discussion experimental uncertainty and data quality control protocols in this manuscript to give a more comprehensive overview of the work.

A missing information appears to be the flight duration, which is relevant for two reasons: *(i)* the longer the flight, the higher the statistical significance; *(ii)* the duration of the flight influences the contribution of mesoscales into the turbulent fluxes. Regarding the latter point, simply providing the standard deviation of wind direction is not sufficient since both turbulence and mesoscale-related unsteadiness contribute to this quantity. It is recommended adding a few lines discussing this point.

A rather theoretical but worthwhile objection can be raised about the use of the "normalized turbulence intensity" in Figures 9b, 12b, and A1. The reason for normalizing the mean wind speed through the incoming freestream lies on the Pi Theorem and the Reynolds analogy assumption, viz. velocity fields observed at different inflows become statistically homogeneous when divided by the incoming mean wind speed, provided that the Reynolds number is high and the geometry of the system stays unchanged. It is hard to justify a normalization of the turbulence intensity based on the freestream value, being this parameter already non-dimensional. The authors themselves normalize the momentum flux in a different way (i.e. dividing by u_0^2) and are forced to use the more common added turbulence in several occasions. It is recommended that all the plots using I/I_0 are changed to added turbulence.

The paper can be in the Referee's opinion accepted if these minor comments are addressed.

Specific comments:

Line 4: it would be useful expand the acronym "UAS" at the first usage.

Lines 39: the near wake is more correctly affected by the local aerodynamic forces on the rotor, so it may be better to say "[…] is closely related to the design and operation of the WT".

Line 40: same as same comment, it should be changed to "detailed design and operation of the WT".

Line 80: "conduct" seems incorrect.

Line 147: can you please clarify in the manuscript if the orientation of the wind turbine is assessed visually or from the SCADA data?

Line 167: "standard deviation of the streamwise velocity" should be indicated explicitly at this line and omitted at line 170.

Lines 175-176: can you provide a reference for the choice of the lapse rate thresholds?

Line 180: if Ω is the rotational frequency (i.e. revolutions per second) the angular velocity is $\omega = \Omega \cdot 2\,\pi$, not divided as in the formula provided. Also, please define Ω explicitly as rotational frequency, not "speed".

Equation 7: please clarify how the c_T is estimated, since it is generally not measured by the SCADA.

Line 213-214: it is not clear how the dots in Fig. 4 can be aligned with “wind direction and the WT” if there is a significant yaw misalignment most of the times. Please clarify.

Lines 221-222: the reason for a more likely counter-clockwise misalignment of the UAS is not obvious, please expand on this point.

Figure 5: independently from the UAS measurements, the nacelle orientation shows negative bias with respect to the wind direction from SCADA. Please comment on this difference.

Lines 290-291: please specify that the peak prominence is smeared by the dissipation of tip vortices that are diffused over 0.5 D.

Figure 7: the calculation of the boundary of the grey band around the BPF is not clear. Explain better or remove.

Line 310: the statement “vortices remain stable for longer distances” to explain turbulent mixing may be confusing considering the previous discussion on tip vortices. Please revise.

Lines 377-378: why convective conditions are more frequent in complex terrain? Please clarify or remove.

Lines 388-390: please expand on the lower added TI in the wake center for stable conditions, which seems to contradict the higher momentum fluxes.