Multi-Point In-Situ Measurements of Turbulent Flow in a Wind Turbine Wake and Inflow with a Fleet of UAS

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1 Review response

We want to thank the anonymous reviewer and Stefano Letizia for their valuable feedback and valid points of criticism to our manuscript.

1.1 Review Comment 2

1.1.1 RC2, General 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.

We agree that the chapter about the measurement system is short. The accuracy of the measurement is additionally provided in the description of the experimental setup. However, the focus of that study is on the application of the measurement system, as you mentioned, detailed information can be found in the references and is out of scope here.

The flight duration is outlined more prominently in the manuscript for clarification. It is rather the first issue of too short flights for statistical significance, which is more relevance for our measurements with 10...12 minutes flight time.

We agree that both turbulence and mesoscale-related unsteadiness contribute to the standard deviation of the wind direction. Since we are limited to 10 min wind data mesoscale-related unsteadiness cannot be captured entirely. However, in the IEC-standard 61400-12-1, the calculation of the turbulence intensity and the mean wind speed is also based on 10 min data.

We agree that the normalization of the turbulence intensity does not follow the Pi theorem since it is already dimensionless. As the added turbulence is more common in literature, we changed the mentioned figures from normalized turbulence intensity to added turbulence intensity and removed the resulting redundant figure.

1.1.2 RC2, Specific Comments

1. *Line 4: it would be useful expand the acronym "UAS" at the first usage.*
   We agree and have revised the manuscript accordingly.
2. *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".*
   We agree and have revised the manuscript accordingly.
3. *Line 40: same as same comment, it should be changed to "detailed design and operation of the WT".*
   We agree and have revised the manuscript accordingly.
4. *Line 80: "conduct" seems incorrect.*
   We agree and have revised the manuscript accordingly.
5. *Line 147: can you please clarify in the manuscript if the orientation of the wind turbine is assessed visually or from the SCADA data?*
   We have revised the manuscript accordingly and added a statement. The orientation of the WT is obtained from the SCADA data and supported visually if short-term changes are observed.
6. *Line 167: "standard deviation of the streamwise velocity" should be indicated explicitly at this line and omitted at line 170.*
   We agree and have revised the manuscript accordingly.
7. *Lines 175-176: can you provide a reference for the choice of the lapse rate thresholds?*
   In general, if the lapse rate is positive, the atmosphere is statistically stable and for negative values, it is unstable.

However, the threshold of 0.5 to differentiate between neutral and stable/unstable conditions is found by Mohan (1998). The reference is added to the manuscript.

8. *Line 180: if Ω is the rotational frequency (i.e. revolutions per second) the angular velocity is $\omega = \Omega * 2\pi$, not divided as in the formula provided. Also, please define Ω explicitly as rotational frequency, not "speed".*
We agree and have revised the manuscript accordingly.

9. *Equation 7: please clarify how the c_T is estimated, since it is generally not measured by the SCADA.*
The c_T is given by the WT operator and details can not be shared due to confidentiality reasons.

10. *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.*
We include an additional statement for clarification. The dots are aligned with the wind direction, and the coordinate origin is locked in the center of the WT but is independent of the orientation of the WT.

11. *Lines 221-222: the reason for a more likely counter-clockwise misalignment of the UAS is not obvious, please expand on this point.*
By this point, we have only described the fact that there is a obvious trend in the plot which results from the alignment of the pattern towards the inflow wind direction and the orientation of the nacelle. As outlined in the manuscript, there is a trend of a yaw misalignment of the WT, which then also causes the orientation of the UAS to be misalignment with regard to the inflow wind direction. Reasons for the misalignments are given in Section 4.2. The statement is adjusted in the manuscript.

12. *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.*
This can only be answered by the operator of the WT and due to confidentiality reasons, we can not comment on this topic.

13. *Lines 290-291: please specify that the peak prominence is smeared by the dissipation of tip vortices that are diffused over 0.5 D.*
An additional specification is added to the manuscript.

14. *Figure 7: the calculation of the boundary of the grey band around the BPF is not clear. Explain better or remove.*
We include an additional explanation for the calculation of the frequency band around the BPF, which is calculated from the maximum and minimum rotation frequencies of the WT measured during the considered time period.

15. *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.*
We changed the statement and only argue with less-pronounced turbulence mixing.

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

    We agree and removed the statement.

17. *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.*

    In our opinion, less added TI in the wake center does not automatically contradict higher momentum fluxes in the center part of the wake. Rather, the inhomogeneous velocity deficit profile in stable stratifications leads to an increased momentum flux in the wake center.

References

Mohan, M.: Analysis of various schemes for the estimation of atmospheric stability classification, Atmospheric Environment, 32, 3775–3781, https://doi.org/10.1016/s1352-2310(98)00109-5, 1998.

Relevant changes to the manuscript

We list here the relevant changes to the manuscript:

1. Introduction:
   - Text modifications in response to referee comments.
2. Experiment:
   - Text modifications in response to referee comments.
   - Fig. 3 is modified in response to referee comments.
3. Methods:
   - Text modifications in response to referee comments.
   - Table 1 is modified in response to referee comments.
4. Results:
   - Text modifications in response to referee comments.
   - Fig. 4 is modified in response to referee comments.
   - Fig. 7 is modified in response to referee comments.
   - Fig. 8 is modified in response to referee comments.
   - Fig. 9 is modified in response to referee comments.
   - Fig. 10 is modified in response to referee comments.
   - Fig. 11 is modified in response to referee comments.
   - Fig. 12 is modified in response to referee comments.
   - Fig. 13 is already included now in Fig. 12 and therefore removed in response to referee comments.
   - Fig. 14 is modified in response to referee comments.
5. Discussion:
   - Text modifications in response to referee comments.

6. Conclusion:

   - Text modifications in response to referee comments.

7. Appendix:

   - Fig. A1 is modified in response to referee comments.
   - Fig. B1 is added in response to referee comments.