Comments to wes-2023-114

General comments

This paper presents some unique results of dual-Doppler retrieval of wind field in the induction zone of a turbine subject to different wake conditions. This work is novel and quite relevant, but some important changes, especially to the physical explanations provided to the observed phenomena, are necessary.

The error analysis is very accurate but some results of it may be misrepresented. SUP and LES do not include all the sources of error, as correctly indicated in Table 2. This should be reiterated when commenting, for instance, Fig. 9 to make sure the reader does not interpret the error bands based on SUP as an estimate for the limits of the difference of the LES validation. For instance, the error band around v is significantly larger than the difference shown by LES but simply due to the lack of pointing accuracy in LES.

Also, not including statistical uncertainty in the following plot may be misleading as this last contribution can easily dominate the overall uncertainty in real experimental campaigns. Statistical uncertainty and convergence are discussed in Section 2.3.2 but the simple comparison with the work of Simley et el. 2016 is not sufficient to justify the current results. The statistical uncertainty is a function of the specific flow conditions (mostly turbulence intensity and integral timescale) as well as number of samples. Please add at least an estimate of the error on the mean to make sure it is ok to neglect it compared to the other uncertainties.

A major drawback is also discussing differences in the measurements that are much smaller than the associated uncertainty (e.g. Fig. 16 and 17). The claimed “asymmetries” in the yaw steering cases are not significant enough to be considered a physical feature. The uncertainty quantification must indeed be used to flag significance of the results, otherwise it becomes just a theoretical exercise. Please either mention that the observed small differences between cases 3 and 4 are not relevant (so everything is really speculative) or remove at all Fig. 16 and 17 and the associated discussion.

A major issue with the interpretation of the results concerns the observer asymmetry of the induction zone. First, a mild asymmetry seems to show in the LES results, but on the other side of the rotor (Fig 6) compared to the lidar observation. Please explain why this is the case. Second and foremost, the physical explanation provided for this asymmetry, namely the “different angles of attacks” between the left and right side of the rotor induced by “wind shear” is not clear. If we consider the same height AGL, then the incoming wind speed is the same on both sides of the rotor. Being the rotational speed the same, this results in identical local inflow, relative speed and angle of attack. Furthermore, it was not found any evidence in the cited references supposedly reporting such effect. The cited papers do indeed show symmetrical induction zones even with shear, as it should be. If there’s a fundamental mechanism creating this asymmetry, please provide a schematic with the angles of attacks as a function of the blade position.

The Reviewer’s opinion is that major revisions addressing the fundamental points listed above are necessary before acceptance.

Specific comments

L 18, "…due to the extraction of kinetic energy by the rotor": this may be a subtlety but technically induction zones are present also around bodies that do not necessarily extract significant kinetic energy from the flow (e.g. in front of an airfoil). It is suggested rephrasing as "…due to the rotor thrust".

L 57: please clarify what "assumptions of the global flow field" refers to.

Table 2: A few improvements are suggested:

- The source of the dual-Doppler error is not necessarily "Non-ideal lidar placement impacts the beam-intersection angles" as an ideal placement leading to 0 error does not exists. It is indeed an "Amplification of single-Doppler uncertainty due to dual-Doppler reconstruction".
- The "averaging period error" could be renamed "statistical uncertainty".
- It would be better to remove the "unavoidable" word in the description of the error due to neglecting w. In fact, one could use for instance continuity or other techniques to estimate w. Also, please explain SUP in the caption.

L 213: Please add reference for the dual-Doppler error (e.g. Stawiarski 2013). Also, the Δx which indicates the angle between the intersecting beams which is the driving factor for the error is not the difference of the azimuth angles $\chi_1 - \chi_2$. Please clarify this point and possibly use a symbol other that $\Delta\chi$ to describe the intersection angle as χ was already used for azimuth.

L 225-239: The main assumptions of SUP, namely the small errors and the 0 correlation between errors, should be clarified.

Table 3: A few improvements are suggested:

- Please explain all symbols in the caption.
- The stability classes indicated here do not match the three given later (stable, neutral, unstable)
- Add z/L in a new column

L 254-259: Please add justification or reference for the choice the stability classes based on L.

L 325-327: The details of the SUP calculation should be moved at L 314 when first introducing the figures.

L 331-332: Please expand further why a high elevation leads to a high uncertainty associated with elevation. Is this just due to the structure of $\frac{\partial u}{\partial \delta}$ being proportional to δ?

L 332-333: It is also not immediately clear why the error due to neglecting w is larger for the lidar more aligned with the wind. Please clarify.

L 333-334: Not only P3 and P6, but also P5 has a preponderant error $\epsilon_{w,1}$which should be explained.

L 341: The location $\frac{x}{D} = -0.16$ is not shown in Fig. 9.

L360-361: There are several ways stable stratification could impact the induction strength, please explain why it is enhancing the velocity decrease in this case.

L367-368: It is not clear why the blades would experience different relative inflow on the left and right side of the rotor. Considering hub-height for simplicity, the inflow velocity U_{hub} is the same on both sides of the rotor if vertical shear only is present. Being the rotational component ωR necessarily the same, the velocity experienced by the blades is identical. Also, the cited reference by Meyer-Forsting shows a symmetrical behavior of the induction in their LES results (below) and is therefore inadequate.

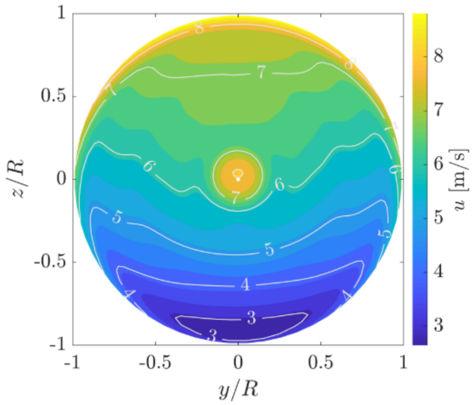

Figure 1. Time-averaged normal/axial velocity at the rotor disc for $\alpha = 0.5$. (LES simulation)

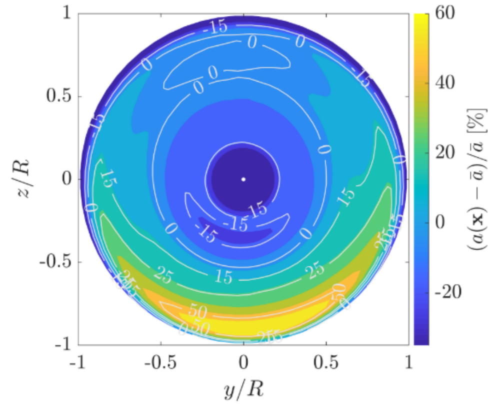

Figure 2. Time-averaged locally induction factor relative to rotor disc average ($\bar{a} = 0.270$) for $\alpha = 0.5$ at the turbine. Here $a(\mathbf{x}) = 1 - u(\mathbf{x})/V_\infty(\mathbf{x})$. (LES simulation)

The explanation based on the interaction of wake rotation and shear is sounder [Madsen et al., 2014] at least in the near wake. Please clarify this point.

L 380: Is the vertical velocity used only to compute uncertainty or also for the dual-Doppler reconstruction? Please explain.

L 388: Please clarify what the 1.96σ bounds mean. Is this the uncertainty from SUP? Why is the error bar not centered on the data?

Figure 12: A deeper analysis on why FLORIS is underpredicting so drastically the induction is needed. The fact that even at the rotor plane the estimated induction is 1/3 that of other models sounds concerning. Please also provide the Ct for this case to allow other researchers to replicate the results.

L 431: Same as comment on L 380.

L 440: It is unclear what "profiles at $\frac{y}{D} \pm 0.5$" means as those specific spanwise locations correspond to a point value in the velocity deficit, not "profiles".

L 442: The concept of "asymmetric induction" here is repeated and not explained. Again, the cited references do not mention any asymmetric induction but focus on the effects of stability and veer on the wake morphology.

L 453-456: You could cite Fleming et al, 2018 to support the fact that wake rotation and counter-rotating vortices sum up in the positive yaw case and cancel out in the negative one.

L 457: Which velocity? The lateral one?

Figures 16 and 17: please swap the plots to show first the positive yaw as in Figure 15.

L 474-475: It is hard to see the claimed stronger induction for negative y. Maybe provide a quantitative parameter like the velocity ratio between inflow and location closer to rotor plane.

L 477-478: The wake recovery followed by the deceleration is only visible for the positive yaw case.

L 496: why is the probe averaging not included as possible source of errors?

L 511: the explanation of the angle of attack as the cause of non-symmetrical induction is not sound (see above). The cited Bastankhah paper shows a symmetrical induction for 0-yaw misalignment (below):

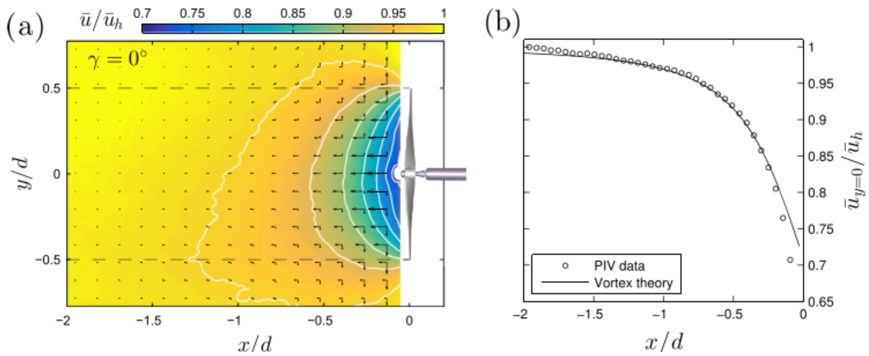

FIG. 6. (a) Vectors of mean normal and tangential velocities induced by the turbine with $\gamma = 0°$ and $\lambda = \lambda_o$ in the upwind region. The background shows the contours of the normalized mean streamwise velocity $\bar{u}/\bar{u}_h$. *White lines* are iso-velocity contours, and *dashed lines* represent the side tips of the rotor. (b) Variation of the mean normalized streamwise velocity in the upwind region along the rotor axis for $\gamma = 0°$ and $\lambda = \lambda_o$.

L 524-525: the non-symmetrical induction one is hard to see (see above).

L 550: the explanation of the shear as the cause of non-symmetrical induction is not sound (see above).

References

[1] Madsen, H.A., Riziotis, V., Zahle, F., Hansen, M.O.L., Snel, H., Grasso, F., Larsen, T.J., Politis, E. and Rasmussen, F. (2012), Blade element momentum modeling of inflow with shear in comparison with advanced model results. Wind Energ., 15: 63-81.

[2] Fleming, Paul, et al. "A simulation study demonstrating the importance of large-scale trailing vortices in wake steering." Wind Energy Science 3.1 (2018): 243-255.