

## REVIEW OF WES-2020-105

*authors:*

Stefano Macrí  
Sandrine Aubrun  
Annie Leroy  
Nicolas Girard

*Experimental investigation of wind turbine wake and load dynamics during yaw manoeuvres*

### Summary:

The work presented in the manuscript describes metrics for a few key phenomena of interest under the dynamic variation of a wind turbine's yaw position. The subject is an important one that is likely to receive more attention in the coming years, as wake steering and active wind plant controls become more common. The main takeaways from the work seem to be that wake center and wind turbine thrust values are invariant to operating conditions, but that some of the characteristic time scales exhibit hysteresis with positive and negative yaw dynamics. The work is interesting, but requires some additional clarification before being appropriate to publish. See comments below.

### Major points:

- page 2 – The authors justify the use of porous discs in order to eliminate many of the aerodynamic phenomena in the wakes related to the rotation and geometry of the rotor blades. Additional explanation is needed to understand how well the results presented in the paper reflect real wind turbine wakes. At the bottom of page 4, “the similarity law with the full scale condition” does not discuss the customary dimensionless parameters for dynamic scaling. Please explain how results from this work apply to real wind turbines.
- – “Time delays, multiples of the aerodynamical time scale  $\tau_0$  were applied and a conditional averaging of the collected velocity fields was then performed” This requires further explanation, does this mean only a single PIV image pair was collected for each dynamic yaw maneuver or that images were taken at integer multiples of the time scale?
- Not clear how HIT2 cases are different except for max measured TI in the wake. Are these just different ensembles of observations from the same case?
- page 9 – Does the formula in Eq. (2) take into account the fact that the projected area of the rotor disc perpendicular to the flow changes with yaw angle?
- On page 9, the authors state that “Theoretically, due to the absence of rotational entrainment in the wake of a porous disc, the absolute value of the wake deviation angle is identical for negative or positive yaw angles.” This argument does not take into account the coriolis force for wind turbines operating in the atmospheric boundary layer.

- In Section 4, results are presented as a list, rather than a single continuous and coherent narrative. This should be changed.
- Authors state that “No dependence on flow conditions can be detected.” in Section 4 when there are obvious differences as shown in Figure 5. This statement requires additional clarification.
- Sensitivity of estimated wake center to identification methodology is a well known issue in the wind energy research community. Given this, presenting the results of only one method are questionable, especially in the case of outliers. A figure showing the estimated wake center locations with different methods would be helpful. Also, the vertical component of the wake center should be included for completeness. See Eliot Quon. SAMWICH Box: A Python-Based Toolbox for Simulated And Measured Wake Identification and CHaracterization. <https://github.com/ewquon/waketracking>
- “The HIT1 P2 configuration presents a higher deviation than the other ones with the same porosity level, without any straightforward physical grounds.” This point is unclear. Do the authors mean that greater than other tests with the P2 mesh or than all tests with the P1 mesh? Is this evidence of the sensitivity of the center tracking method?
- The authors make a point, “The ABL P2 configuration presents a discrepancy between its trend (especially at  $\alpha = 20^\circ$ ) and the other results at the same porosity level. This is because the flow inhomogeneity together with the higher level of ambient turbulence make the velocity deficit generated by the higher porosity disc rather small and unsuitable to properly track the wake center. For these reasons, the ABL P2 configuration will not be discussed further.” Is this discrepancy within the uncertainty bands of the measurements? The modeled ABL seems like the most meaningful and representative case tested in this work to the wakes of real wind turbines. These data should be kept and discussed in the context of the rest of the analysis.
- Last point in Section 4.1 – it is not clear to which discrepancy the authors refer. Data in figure 5a for HIT2bP1 and HIT2cP1 appear very similar.
- Last point in Section 4.2 – Authors state that “the thrust gain for a  $30^\circ$  yaw angle compared to the  $0^\circ$  case is around 13%, irrespective of the flow conditions” is a bit misleading. It is not immediately clear that the thrust gain and the yaw angle are for different turbines. This is also true for the right subfigure of Figure 5. The axis labels are a bit confusing. At first glance, it looks like a ratio of the thrust coefficient of a wind turbine under yaw to that of the same turbine without yaw. Update labels to make more clear.
- Figure 5 – These figures would benefit from error bars. Maybe place cases side-by-side in groups for each value of  $\gamma$ ?
- Equation 4 – For clarity, authors should specify that for positive yaw maneuvers,  $\gamma_{start} = 0$ , and for negative yaw maneuvers,  $\gamma_{end} = 0$
- It would be informative to see higher order statistics of the key phenomena of interest. PDFs of wake deviation angle and thrust coefficient would help readers and researchers understand that these are stochastic quantities.
- If the fit functions in Eqs. (7) and (8) are applied to the estimated yaw center and change in thrust of the downstream turbine, shouldn't  $\tau_{lag}$  represent the convective time between the upstream turbine and either the measurement location or the downstream turbine? Also, the transient dimensionless duration is missing 10% of the fit, and thus will systematically underestimate the actual duration. This will lead to misleading values of  $\Delta\tau_{ratio}^*$ .
- The measured overshoot of thrust coefficient is not modeled or discussed at all (see Figure 7). This seems like an important physical element of this phenomenon. Consider modifying the models in Eqs. (7) and (8) if the overshoot appears consistently. Are the curves of  $C_T$  in Figure 7 averages over many maneuvers?
- Tables 3 and 4 – Do these values represent least-squares fits to average values, or are the instantaneous data fit and fit values averaged afterward? The tables would benefit from higher-order statistics or measurements of uncertainty.

- page 14, line 288 – I think  $\tau_m$  should be  $\Delta\tau_m$
- Figure 8 needs legends and should use different symbols as figures above, as the points represent different groupings of the test cases.
- Authors state that “Theoretically, the fitting coefficient  $\tau_{lag}^*$  lag (Tables 3 and 4) can be interpreted as a time delay before the transient starts. Unfortunately, there is no clear relationship between this parameter and the  $\tau_{start}^*$  start. This illustrates the difficulty of capturing the actual transient start for the present study.” It seems like  $\tau_{start}$  should be  $\tau_{lag}$  plus the time required for the function to reach 0.5 based on the parameter  $c$ .

### Minor points:

- page 1 – paper
- page 1 – which is increasingly studied
- page 1 – started to be envisaged.
- page 1 – Remove “ity”
- page 1 – Remove the work “in” from XXXX
- page 6 – Table 2 – units should be written in normal text rather than math font.
- page 6 – Remove ical
- page 6 – kHz
- page 6 – “performed in order to perform” is redundant. Please rephrase.
- page 6 – Text subscripts should be changed (e.g.  $T_{filtered}$  instead of  $T_{filtered}$ )
- page 7 – Assumptions are made by the authors in fact. Replace “can be” with “is”.
- page 8 – treated – considered?
- page 8 – it is not clear what ‘usual error propagation methods’ are. Please be specific.