

## Reviewer's comments to WES-2026-8

### Main comments

The authors present an interesting statistical analysis of utility-scale turbine wakes under different and carefully characterized atmospheric conditions. The work is relevant to improving the understanding of wake physics and wind farm modeling and adds some novelty to existing literature. A few minor revisions are recommended to improve readability and rigor.

In Section 3.2, it may be beneficial to add a  $-$  in front of  $A$ , so that  $A$  becomes a positive-definite constant. This would avoid awkward definitions like “the amplitude  $A$  is greater than  $-0.1U$ ” to indicate a shallow wake.

In Section 3.1, the lidar filtering process should be expanded and cite previous work (e.g., [Beck and Kuhn, 2017](#)).

The possibility that the lidar could have an azimuth offset with respect to the turbine axis should be mentioned. Alternatively, the pointing accuracy of the lidar should be quantified. This is important to ensure that the wake deflection (which is on average 30 m at 4 D or 3.7 degrees) is real. Also, for the asymmetric wake model where the center is a weighted average between the two peak locations, the asymmetry may just induce an apparent deflection if one side of the wake is consistently deeper. This should be discussed as well.

From a more pragmatic standpoint, it should be acknowledged that the near-wake physics, in spite of its scientific interest, is less of a concern for industrial applications, where turbines are spaced significantly further along the prevailing wind directions.

### Specific comments

L 28: Please clarify that these are canonical atmospheric conditions and that there are many exceptions (for instance, during low-level jets).

Eq. 3: This approach only includes the part of induction that is converted to power, which will probably cause an underestimation of thrust. Drag from tower, hub, and blade roots all contribute to thrust but not power. Electrical losses cause an underestimation of thrust. Please clarify this point and possibly cite [Iungo et al., 2018](#).

L 113: Please explain the implications of measuring inflow quantities at 85 m which is less than hub height.

L 146: “The scan ends” is ambiguous, you can say that “the beam are blocked by the ground at 700 m”.

L 149: Please specify the duration of the 6,099 periods (27 s? 10 minutes?).

L 152: How are “free-stream atmospheric conditions” defined?

L 170: Please give more details on the data filtering, namely the way the “neighborhood” is defined and the “threshold” for the median deviation.

L 205: Please provide a reference for the F-test.

L 237: Can this be said more concisely as “when the difference between the centerline and maximum velocity deficit is less than 5%”?

Fig. 7: The  $\mu$  symbol should be introduced earlier. Also, please expand on the effect of the second wake past 4D and how it is smoothed out in these statistics by the quite large wind sector.

L 273: “The induction zone of the second turbine impacts the wake dynamics of the first turbine additionally” should be removed. There is no wake dynamics provided in this section but only mean quantities. Furthermore, the induction zone is probably too thin for the present lidar and grid resolution to isolate its effect.

L 302: Please add discussion also of the wake width.

Fig. 11: Please explain better the legend either in the caption or the section.

L 415: “wake tilt” may be mistaken for vertical deflection, while it sounds here we are talking about the skewness or stretching of the velocity deficit in the cross-stream plane. Please clarify.

L 424: Please add a reference to a simple wake model that uses turbulence to predict wake deflection.

L 520: The link between (tip?) vortex dynamics and wake asymmetry is unclear. Earlier it was claimed that the asymmetry arises from a combination of wake rotation and shear, with stability enhancing its persistence. Please resolve this ambiguity.