The article discusses the near wake deviation of a utility-scale offshore wind turbine as a result of yaw error, which has not been previously studied in the literature. In particular, the authors note a "delay" in the onset of this wake deviation.

Overall, the authors have done some interesting work, and the manuscript is well-organized and concise. Methodologies are clearly explained and figures are used appropriately to enhance the discussion in the text. I approve publication of the article, considering that corrections will be made according to the recommendations given below.

Major comments

Lines 65 and 80: Eqs. (1) and (2) describe the shape of the *wind speed* distribution, even though lidar measurements are later fit to these models and the lidar is only capable of measuring *line-of-sight* velocities. More parameters are needed to account for the azimuth and pitch angles of the lidar beam. This is not a major concern when the lidar angles are small (i.e., less than 10 degrees), but it can introduce a significant source of error for larger angles. I strongly encourage the authors to either (a) update Eqs. (1) and (2) to account for lidar azimuth and pitch, or (b) provide a rigorous justification as to why these angles are small enough to be neglected in this particular experiment.

Line 159: What is the purpose of Eq. (2) if it isn't used? Here, I do not agree with the authors' decision to abandon Eq. (2) in their analysis. Eq. (1) is a function describing the shape of the far wake, while Eq. (2) is a function describing the shape of the near wake. Since the authors make a point of focusing their analysis on the near wake, Eq. (2) is the appropriate fitting function to use in this context. One can easily see from Fig. 5 that Eq. (1) is inadequate for describing the shape of the near wake. I strongly recommend that the authors do the analysis using Eq. (2). It might be harder to employ Eq. (2), but it is the proper equation to use.

Minor comments

Lines 14-17: To support the statements given here, it would be nice if the authors cited any relevant literature surrounding the use of yaw control for wind farm optimization.

Lines 27-29: The use of the first-person pronoun "I" is inappropriate here since the paper under review and Trujillo et al. (2011) both have multiple authors, including the overlap of two authors. I suggest that this sentence begin "Trujillo et al. (2011) developed a wake tracking technique which…"

Line 31-32: Other studies have in fact studied the near wake. For example, Aitken et al. (2014) discusses an experimental study that covered both near wake and far wake measurements. The authors should be more specific by noting that their unique contribution here is their specific focus on near wake deviation due to yaw misalignment.

Line 39 (and elsewhere): I recommend that the authors use the conventional abbreviation "D" for rotor diameter, to avoid spelling out the word "diameter" every time.

Line 60-61: Rather than saying "measured by an additional system" the authors should be more specific and mention that the inflow profile is measured by a met tower upwind of the turbine. Presumably, the ambient profile is not the same upwind and downwind of the rotor, particularly since the met tower is

located almost 1 km upwind of the turbine. This is a deficiency that introduces error into the estimation of the wake properties, which the authors should acknowledge.

Line 66: It is not just that the Gaussian function can be selected for its flexibility. The authors should mention that there are theoretical and experimental justifications for using a Gaussian to fit the wake profile. See, for example, Pope's book from 2000 titled "Turbulent Flows" and:

Magnusson, M., 1999: Near-wake behaviour of wind turbines. J. Wind Eng. Ind. Aerodyn., 80, 147–167, doi:10.1016/S0167-6105(98)00125-1.

Line 69-70: Gaussian functions cannot have a "half-width" since they extend to infinity. It would be more accurate here to mention the full-width at half-maximum (FWHM) or just say that σ_y and σ_z are parameters that determine the extent of the wake boundary.

Line 70: Magnitude of correlation coefficient can be less than or equal to 1.

Lines 75-80: Eq. (2) is presented without context as though it is entirely original, when in fact it is effectively a two-dimensional analog to Eq. (8) in Aitken et al. (2014). It behooves the authors to acknowledge this previous work.

Line 85 (and elsewhere): Why is the name of the wind farm enclosed in brackets and lowercase? Why is not written as "Alpha Ventus" wind farm?

Line 125: Figure 4 is somewhat misleading since it does not seem to be drawn to scale. The text states that the tower is located 8D or about 900 m from the turbine, but Figure 4 makes it look like the tower is less than 500 m from the turbine. I suggest redrawing the figure to scale.

Line 144: Figure 6 depicts measurements of wind speed and direction. The instruments used to take these observations have some kind of measurement uncertainty. The measurement uncertainty should be quantified in the text, and Figure 6 should be modified to include error bars showing this uncertainty.

Line 156-157: The language here seems vague: "The results looked qualitatively more similar…" What results are being referred to here? More similar in comparison to what? Similarly, the phrase "improvement of success" is also vague. Improvement over what? And what does "success" mean? That the fitting algorithm converged to a sensible solution?

Line 160-161: What does it mean for a snapshot to have been tracked "successfully"? Why is 70% chosen as a cutoff point? This number seems completely arbitrary—are the authors able to justify this cutoff somehow?

Line 167: Is there any significance to 2.6 degrees? Was this close to the average yaw error (the difference between the wind direction measured by the tower and the yaw angle of the turbine) during the experiment? If so, that would be a neat result to point out.

Line 185-187: The wake deviation delay seems analogous to the fact that the maximum velocity deficit is attained 1-2D behind the turbine, as noted in:

J.F. Ainslie. Calculating the field in the wake of wind turbines. Journal of Wind Engineering and Industrial Aerodynamics, 27:213–224, 1988.

and in Sanderse's literature review on the aerodynamics of wind turbine wakes. It would be interesting to point out the similarity between the deviation delay and the velocity deficit since both cases show that, in the real world, the impulse delivered by the rotor on the flow cannot occur instantaneously.

Typos

Line 88: Change "minutes" to "minute".

Line 108: Change "which" to "whose".

Line 195: Change "analog" to "analogous".