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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Exploring the complexities associated with full-scale wind plant wake mitigation control experiments" *by* James B. Duncan Jr. et al.

Anonymous Referee #1

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This paper discusses two measurement campaigns: 1) a wind plant control experiment involving changes in pitch offset and yaw misalignment at a commercial wind plant with wake measurements from Doppler radars, and 2) a measurement campaign where wake length and the degree of wake meandering were characterized using Doppler radar measurements for different atmospheric stability conditions. The paper discusses the inability to enact the desired control changes in the first experiment, explaining the shortcomings, and discusses how inhomogeneities in the wind flow can make it difficult to distinguish control impacts on wake behavior from the impacts of atmospheric variations. The second measurement campaign highlights how the length of wakes increases in stable conditions while the amount of meandering decreases, and the

authors explain how this means wind plant control should be more effective in stable conditions.

The paper is written very clearly and is easy to follow. And the paper shows how valuable Doppler radar measurements can be in wind plant control experiments. However, one area that I believe needs to be improved is the explanation of the goal of the wind plant control experiment and how the analysis presented connects to the goal. The paper focuses on analysis of data from an experiment, but it isn't clear what the original objectives were. Was the goal to measure changes in wake behavior, or to look at the impact on power of the downstream turbines? Why were only a few 10-minute control periods used rather than a longer experiment that would more clearly reveal trends? Given the original objectives of the experiment, what would the authors do differently next time?

Furthermore, the authors should connect this work to the existing literature on field validation of wind plant control concepts. More of a review of previous work in the field should be provided and the authors should discuss how the objective of their work fits in with what has already been published, rather than saying that previous work "remains limited." Is there a gap the authors are trying to address with this research?

In addition, there are several areas where explanations and methods should be improved, as explained in the comments below.

Specific comments:

Pg. 2, ln. 17: Double check your references listed. For example, Vollmer et al. 2016 and Fleming et al., 2018 are listed as wind tunnel experiments, but these are numerical simulations.

Pg. 2, ln. 22: Another recent full-scale validation of wind plant control is: Howland et al., Wind farm power optimization through wake steering, Proceedings of the National Academy of Sciences, 2019.

Pg. 2, ln. 25: "To expand upon existing full-scale validation efforts, agreements were made with an industry partner. . ." Please be sure to review the existing full-scale validation efforts and explain how the present work fits in.

Section 2.2: Can you explain if the 12 October experiment is at a different site? Are both sites in similar terrain, or are there significant differences between them that should be pointed out?

Fig. 1: Please explain the meaning of the different colors of wind turbines, including white, to avoid confusion.

Pg. 6, ln. 1: What purpose do the downstream turbines (white circles) serve in this experiment?

Pg. 6, ln. 18: Fleming et al., 2018 deals with numerical simulations, do you mean 2019?

Pg. 7, ln. 6: If the benefit of modifying blade pitch is greater in region 2, then why was the sole half-hour experiment period in region 3? Would it have made more sense to wait for more favorable conditions?

Pg. 7, ln. 9: "To maintain the rated generator speed in region three, the wind turbine follows a pitch schedule to extract the desired amount of momentum at various wind speeds." Blade pitch controllers typically use generator speed feedback to control blade pitch to regulate generator speed. Therefore, if you are adding a pitch offset in region three, what else are you changing in the controller so that the pitch controller doesn't simply compensate for the offset to bring the generator speed back to rated? Is the generator torque or gen. speed setpoint also changed? Could it be that the pitch offset that is added is simply an offset to the "fine pitch" (minimum pitch) angle that the turbine operates at below rated, and that there is no real change to the pitch control above rated? More detail about the intended pitch offset strategy would be helpful.

Pg. 7, ln. 12: "The region three pitch schedule was constructed by fitting a linear model

to the distribution of blade pitch angles. . ." Pitch schedules are generally very nonlinear as a function of wind speed, especially near rated wind speed. Can you elaborate on your choice of a linear pitch schedule model?

Pg. 9, ln. 12: A 1.45 km x 1.8 km averaging area seems too large for determining the local inflow wind direction to the turbines, especially if you are trying to distinguish between the wind inflow to each of the three turbines. Furthermore, given the advection time across the 1.8 km analysis area, the estimated wind directions are likely not very well correlated with what the turbines see at a high temporal resolution. Can you try this with 100 m x 100 m averaging areas, local to each turbine? This could improve your results, or at least make them more meaningful.

Section 3.1.2: Given the large positive mean yaw errors with or without the offset applied, it seems possible that the yaw position reported in the SCADA data is not calibrated properly. It is common for yaw position values from SCADA data to deviate significantly from the true orientation (i.e., 0 degrees -> true north) over time. Was the calibration of the yaw position data confirmed? If not, this should be discussed further.

Pg. 11, ln. 15: "However, nacelle-based measurements are inherently distorted. . ." Another factor to consider is that the flow distortion from the rotor can change as the control changes. Adding a pitch offset could cause the wind speed behind the rotor to change differently than with the original control, complicating the detection of changes in turbine operation as a function of wind speed.

Pg. 12: ln. 10: "Both of these factors might have contributed to the experimental control offsets not being fully realized." Certainly for yaw control, a single 10-minute period might not be sufficient to observe meaningful yaw misalignment changes, given the slow dynamics of yaw controllers.

Section 4.1: I would suggest a revised wake tracking algorithm in light of improvements in the understanding of wake deflection physics. As discussed in papers such as the following, yaw misalignment can cause wakes to have a "curled" shape due to the presence of counter rotating vortices. This means the peak velocity deficit could change with height and averaging across all heights in the rotor disk area is not necessarily the most relevant metric.

-Vollmer et al., Estimating the wake deflection downstream of a wind turbine in different atmospheric stabilities: an LES study, Wind Energy Science, 2016.

-Howland et al., Wake structure in actuator disk models of wind turbines in yaw under uniform inflow conditions, Journal of Renewable and Sustainable Energy, 2016.

-Fleming et al., A simulation study demonstrating the importance of large-scale trailing vortices in wake steering, Wind Energy Science, 2018.

A more meaningful lateral wake center estimate for wind plant control applications can be found using the method explained in Vollmer et al. 2016, where the cubed wind speed is averaged across a hypothetical rotor disk area centered at different lateral displacements. The displacement that results in the lowest value can be considered the wake center position.

Fig. 9: Is Fig. 9 (a) showing the distance from the centerline of the wake after correcting for the skew angle, or from the centerline in the mean wind direction? Please clarify what is being shown.

Pg. 16, ln. 6: "...indicating the observed wake deflection... was opposite of that expected" How might wind veer impact the wake deflection during the experiment period? Could this be an explanation for the unexpected skew?

Pg. 18, ln. 3: "9 degrees counterclockwise... of thetaˆV_inf." Stating what thetaˆV_inf is would clear up any confusion about the sign convention.

Section 4.2.1: The potential impact of streak orientation on wake skew angle is an interesting idea. However, a deeper discussion of how this might cause the skewing of the wake would be appreciated.

Fig. 13: Consider showing the joint probability density of streak skew angle and wake skew angle. This would support your idea of a correlation between the two better.

Section 5.1: SD WTA: For a section title, consider spelling out the acronym.

Section 5: In addition to wake length and wake meandering, what differences have you observed in the relative magnitude of the velocity deficits for stable vs. unstable conditions? This would be a valuable addition to the paper.

Pg. 26: ln. 14: "access to the controller design so any factors inhibiting proper implementation of the turbine control offsets can be identified." I agree that access the controller improves the assessment of wind plant control strategies, and is always desirable, but I think meaningful control assessments can be done without direct access. For example, adding a pitch offset in region 2 (where pitch is typically fixed at "fine pitch") could be achieved without needing to understand the controller dynamics. Furthermore, to implement a yaw misalignment, the yaw controller setpoint could be changed from zero to the desired offset, but full understanding of the controller dynamics is not necessary, and in many cases would be asking too much given the proprietary nature of wind turbine control systems.

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