February 13, 2026

We wish to thank both reviewers for their helpful and constructive comments. The reviewers' comments and questions are addressed below.

Reviewer 1

The manuscript "Estimating annual energy production of wake mixing control strategies including comparisons to wake steering" by Yalla et al. presents a methodology for estimating AEP gains under the application of wake control methods. This methodology is applied to a 3x3 wind farm, where it observes AEP gains of 0.1-0.8%. This is an important contribution to the understanding of wake control strategies and a logical next step in the development of such technologies. Most of the results are presented clearly, and the text is easy to follow. Therefore, the paper should be published in the Journal after considering the following remarks and concerns.

Major Comment

My major concerns revolve around the topic of uncertainty, which, given that the observed AEP gains range in very low values, could ultimately decide between positive and negative AEP gains. I understand that a direct determination of the uncertainty is probably not possible. Therefore, the following aspects should be addressed to at least understand and minimize the uncertainty of the presented results, where possible:

The authors agree that uncertainty is a major factor in the AEP results and have taken the reviewer's suggestion to clarify in both the title and manuscript that this work is an AEP estimation strategy based on engineering wake models, and not necessarily a definitive AEP estimate. We do feel that part of the utility of a tool like FLORIS is being able to provide a first-order, engineering approximation of AEP given a limited set of data for wind farm layout and control optimization purposes, the results of which can then be further scrutinized with higher-fidelity methods. This is often the situation faced by the wind industry and is ultimately the goal of the manuscript, and we have strengthened this point in the revised text. We have also changed all results to report only AEP gains relative to the baseline FLORIS model (rather than absolute AEP values), so the focus is primarily on the additional AWM term in the empirical Gaussian model. Finally, we point to recent advancements in uncertainty quantification (e.g., Aerts et al. [2025]) that could be leveraged in future studies to bound AEP estimates from low-fidelity, engineering-type models like FLORIS. Additional comments regarding uncertainty and the training data used in this study are provided below.

1. The entire study is based on one precursor representing a stable ABL in which AWM is assumed to be particularly effective. However, it appears very unlikely that these stable conditions are representative of the flow conditions throughout the entire year, as they are required for the AEP estimation. Consequently, it must be assumed that in a significant part of the year, AWM actually performs worse than assumed in the calculation, hence the AEP gains would be lower. Related to that, the following questions

   1.1 Does the measurement data from the NY Bight include the atmospheric stability? Can it be justified to assume stable stratification throughout the entire year?

1.2 How would the results change when considering also unstable conditions?

The data from the NY Bight includes measurements of wind speed, wind direction, and turbulence intensity (TI), which are shown in Figure 1 and Table 1 in the manuscript. A majority of wind conditions do occur at low TI levels ($\sim$ less than 10%) and Region II wind speeds, indicating that stable conditions are quite common at the NY Bight, as is typical for offshore locations. However, all wind speeds, wind directions, and TI levels from the NY Bight data are included in the AEP analysis, meaning both stable and unstable conditions are included in the results.

Additionally, we clarified the modeling assumptions and the criteria used to select training data in the updated manuscript. Specifically, we assume that AWM power gains: (1) depend strongly on wind direction and pitch amplitude; (2) are approximately invariant across Region II and early Region III wind speeds when turbulence intensity (TI) is low; and (3) diminish beyond a threshold wind speed and/or TI, where AWM is no longer beneficial. These first-order assumptions are guided by prior AWM studies such as the one summarized in Table 2, where the non-zero entries indicate wind conditions where the power gains from AWM have been evaluated (or estimated) and found to be significant enough to justify its use. Higher in Region III, the utility of AWM is certain to diminish due to the lower wake effects. Similarly, AWM is expected to decrease at higher TI because increased ambient mixing reduces the benefit of externally-imposed mixing. Due to the computational expense of the LES, we therefore focused our training data on varying pitch amplitude and wind direction while simulating a single Region II wind speed and low TI level, and we built into the optimization algorithm a switch to disable AWM for wind speeds above 15 m/s or TI above 10%. In the original manuscript these assumptions and the training-data selection were split across Sections II and III; we have consolidated them so readers can clearly see what assumptions were made in the model.

2. Figure 11 validates the tuned FLORIS model with the LES data. Unfortunately, the FLORIS profiles don't match the LES profiles satisfactorily in all cases. For instance, in the second row, at 4D, the wake deficit is almost double as high in the LES as in FLORIS. This might be the reason for the row-averaged power overprediction of FLORIS visible in Fig. 10. This is concerning because the AEP estimates are expected to be very sensitive to these wake profiles, which raises the following questions:

   2.1 How do the wake profiles compare in cases with different wind directions?

   2.2 How does the standard FLORIS model used for the Baseline control cases compare with the LES? If the match is not better, can the authors explain the source of this mismatch? Maybe from using a Gaussian mean flow model that doesn't account for thermal stratification in a stable ABL?

   2.3 The model is trained and validated with the same LES data. Maybe additional LES validation case(s) with unseen wind speeds and wind direction could validate the choice of parameters and estimate the uncertainty coming from the wake model.

The wake profile comparisons in Figures 10 and 11 and the surrounding discussion were not intended to serve as a validation between FLORIS and the LES, but instead to highlight where the current emperical Gaussian wake model lacks degrees of freedom needed to represent flow features from the LES. The primary validation metric in this paper is Table 5, which indicates that FLORIS can provide a reasonable estimate of total farm power despite pointwise inaccuracies in the predicted flow fields (in part because farm power is an aggregate quantity that benefits from averaging across turbines). More generally, we do not expect FLORIS to match the LES wake profiles because it does not explicitly account for veer, shear, non-axisymmetric wake structure, induction effects, or strict conservation properties. This is true even for the baseline wakes as shown in Figure 1 of this document. As a result, agreement in farm power can occur even when the detailed flow fields differ. We have reworded this section in the updated manuscript to more clearly state the intention of these comparisons to the reader and to motivate future improvements to FLORIS and related reduced-order models, such as the skewed wake correction model of Abkar et al., Energies (2018) mentioned by the second reviewer.

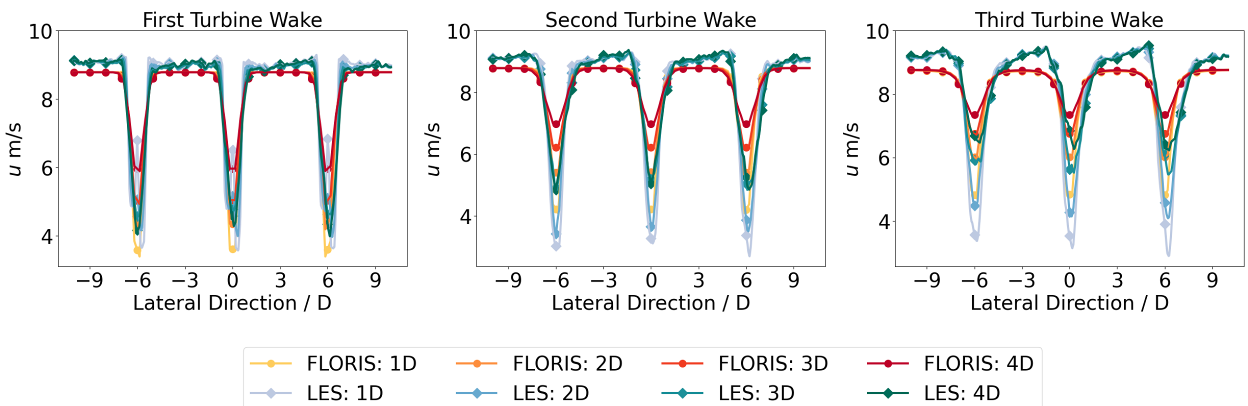

Figure 1: Hub-height profiles of the axial velocity in the wake of first, second, and third turbine rows. Shown are the LES and FLORIS results for the $6D$-spaced wind farm oriented at 225° with baseline controls.

With regard to the last comment, we trained the model using the full set of LES cases rather than reserving a subset for testing, given the high computational expense of LES and the limited number of simulations available. We agree that additional simulations and a dedicated testing set would be valuable for more robust uncertainty quantification and model refinement, and we view that as an important direction for future work. The focus of the present manuscript is on establishing a practical framework for estimating AEP gains due to AWM from a limited set of high-fidelity training data.

3. In lines 332/333, recent studies of synchronizing the helix over multiple rows are used to justify that waked turbines can also apply AWM. However, the cited study from van Vondelen also shows that unfavorable alignment can also reduce the power gains. Therefore, I think a mean flow model would require further adaptation when arguing with synchronization. The question is, therefore, whether the optimizer decides to apply AWM also to mid farm turbines? If yes, it must be assumed that the results are off and consequently increase the uncertainty of the AEP estimates.

The number of turbines the optimization algorithm is allowed to actuate is controlled by the parameter N_{AWM}, which was set to 9 in the original manuscript. This means midfarm actuation was allowed, and often occurred because the addition mixing term in the empirical

Gaussian model will reduce the wake deficit behind second row turbines leading to an increase in power for the third row turbines. Although the study from van Vondelen suggests AWM can be beneficial on downstream turbines, the reviewer is correct that this depends on careful synchronization between turbine controllers. The empirical Gaussian model used here does not provide a detailed enough description of the wake to study synchronization effects between turbine controls. Instead, we have updated the AWM results so two choices of N_{AWM} are reported: $N_{AWM} = 9$ and $N_{AWM} = 3$ (see Figure 2 in this document). When $N_{AWM} = 9$, all turbines are allowed to actuate if it benefits the overall farm power, including midfarm actuation. When $N_{AWM} = 3$, only three turbine are allowed to actuate to maximize farm power. In fully aligned conditions, setting $N_{AWM} = 3$ corresponds to the actuating only the frontline turbines. We thank the reviewer for this comment and feel this change provides important context for the model. These updated results also provide a degree of uncertainty quantification by demonstrating the sensitivity of the AEP results to the number of actuated turbines.

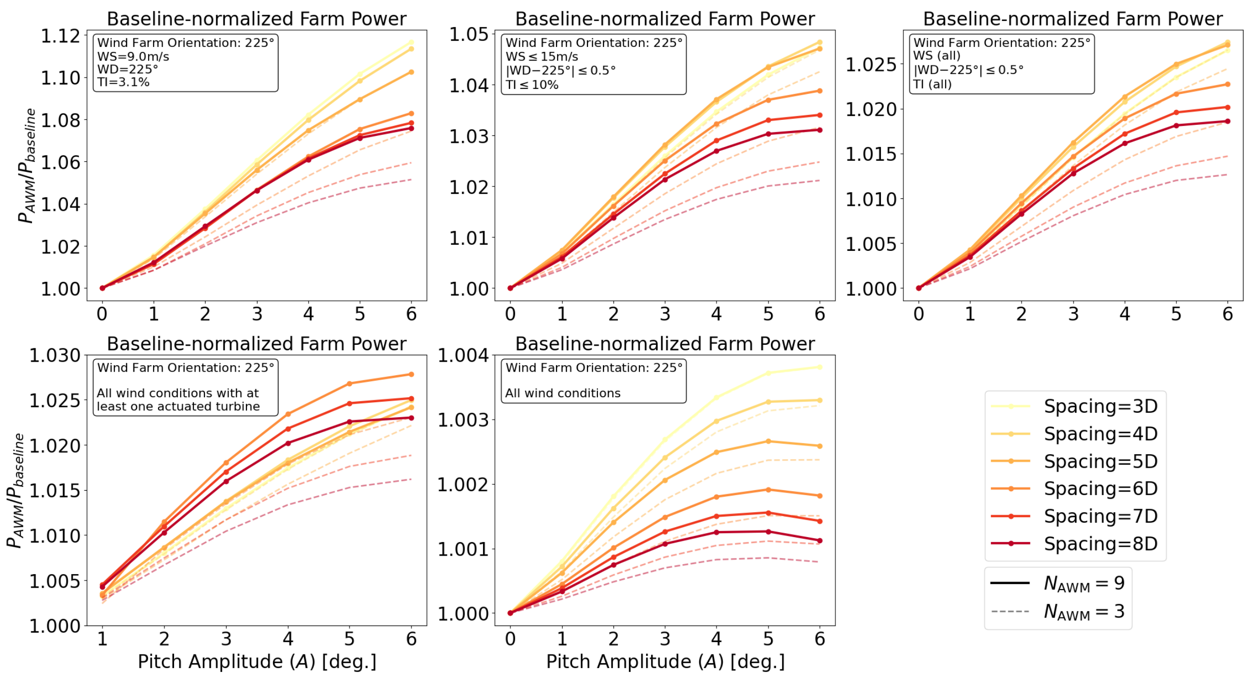

Figure 2: Baseline-normalized total farm power for the wake mixing cases as a function of the AWM pitch amplitude. The wind farm is oriented at 225° and the turbine spacing is varied from $3D$ to $8D$. In each panel, a different set of wind conditions from the Weibull distribution is considered, as indicated by the annotation in the top left corner of the panel.

Considering these points, I would welcome clarification in the title that this is an AEP estimation strategy, but not a generally valid AEP estimate.

This change has been made. The title of the manuscript has been updated to: "A FLORIS-based approach for estimating AEP gains from wake mixing control strategies with comparisons to wake steering."

Further Comments

5. Line 112 states that the LES domain is 10 km high. Is that really the case, or is a 0 too much? If yes, why? And if yes, what's the inversion layer height and the lapse rate above the free atmosphere? Vertical profiles would help.

   This was indeed a typo, the domain is 1 km high. Thank you for noticing this mistake. The manuscript has been updated to read: "The wind farms consist of nine IEA 15 MW turbines arranged in a 3×3 array situated inside a domain of size $10 \times 10 \times 1$ km."

6. Line 113/General: Kasper 2025 also uses a 1km high ABL, where a 3x3 wind farm might be representative for the momentum entrainment mechanisms in a larger wind farm. However, it has been shown that more shallow ABLs fundamentally change these mechanisms, leading to global blockage and atmospheric gravity waves. Do the results shown in the manuscript allow for conclusions on a large-scale wind farm subject to deep array effects?

   We have not studied the interaction between AWM and global wind farm blockage effects or atmospheric gravity waves, which may be important mechanisms for understanding deep array effects of AWM. Those are assumed to be secondary effects to the primary momentum entrainment mechanisms studied here. The LES results do suggest some interesting deep effects may occur, such as the Helix mixing extending to multiple turbine rows, but a larger LES campaign will be needed to accurately address the benefits of AWM in larger wind farms.

7. Line 60/236: What is the full citation of Frederik et al.(2024)? I was not able to find that publication, but it is where the FLORIS part of the work is based on.

   To the author's knowledge, the previous work by Frederik was only presented as a presentation at NAWEA/WindTech 2024, which is used as the citation here.

8. Lines 98-100: It is assumed that relative power gains translate from one wind direction to others. What is that assumption based on? Can it be validated?

   As state in an earlier response, we are assuming that AWM power gains: (1) depend strongly on wind direction and pitch amplitude; (2) are approximately invariant across Region II wind speeds when turbulence intensity (TI) is low; and (3) diminish beyond a threshold wind speed and/or TI, where AWM is no longer beneficial. We are not assuming that the relative power gains are constant between wind directions. Therefore, the LES are performed using a Region II wind speed (9.0m/s) and low TI level (3.1%), but three different wind directions are simulated that span the full 90° sector

9. Table 3/Lines176: Helix method is only investigated in one configuration, in which it actually outperforms the Pulse. Why is it then not considered in the other configurations to investigate if it might even lead to larger AEP gains?

   The decision to primarily use the Pulse method in this study was based on the previous work of Frederik et al. [2025] and Brown et al. [2025], who found a significant benefit to using the Pulse method over the Helix method in a two-turbine array in similar wind conditions to the one used here. The LES cases in this study took several months to complete and were all initiated at the same time. The improvement of the Helix method over the Pulse, particularly in the third row of turbines, is a major finding of the study and indicates that the Helix should be included in future wind farm studies of AWM, even in highly veered conditions.

10. Fig. 6,7,8, Lines 206: I agree there is not a lot of interaction between the turbines visible. At these small wind farms in the given ABL conditions also deep-array effects can probably be neglected. Could the contributions of this paper also be drawn from three turbines with different streamwise and lateral alignments to reduce computational costs?

    The answer is likely yes, and this is a good observation by the reviewer. Based on the high expense of the nine-turbine runs, a question of practical relevance is how well the power gains simulated for a three-turbine cases correlates to the power gains simulated for the nine-turbine cases, at least for wind directions that are fully aligned with the wind farms. Figure 3 compares the row-averaged 9-turbine results with a 3-turbine array for both a baseline case and pulse case with $A = 4°$ and $St = 0.3$. The results support the hypothesis that the power gains on the first three rows of turbines are similar to within a certain percentage regardless of the size of the farm beyond. Besides the likely statistical non-convergence of the power due to the different realizations of the turbulent flow in the three-turbine and nine-turbine LES domains, another explanation for the observed differences between the three-turbine and nine-turbine results could be the interaction of AWM with farm-wide blockage effects.

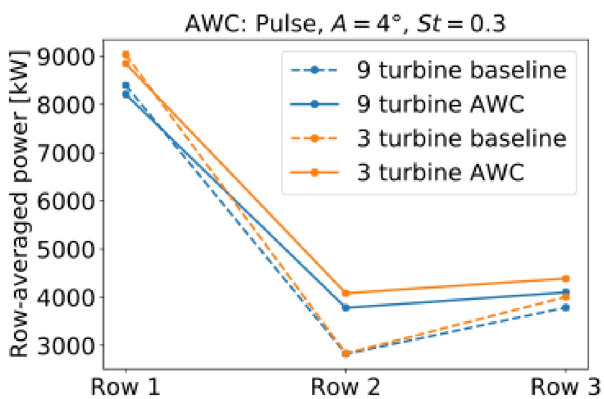

Figure 3: Comparison of power between a 3-turbine and (row-averaged) 9-turbine LES case. The turbines are spaced $5D$ apart and aligned with the incoming wind direction at 225°.

11. Fig. 11 shows many lines per figure that only differ in terms of very similar colors. Could the FLORIS and LES lines of the same case be plotted in the same color, but with different line styles? If that doesn't help the readability, maybe consider distributing the lines over multiple plots or plotting less downstream distances

    After some experimentation, we updated this figure to:

    (a) Distinguish between the FLORIS and LES results using a multi-hue, sequential warm/cool color scheme.
    (b) Further distinguish between the FLORIS and LES results using circle and diamond markers.
    (c) Increase the line width of all curves.
    (d) Reduce the number of downstream distances shown from 5 to 4.

    The updated results are shown in Figure 4 of this document. We feel these changes improve readability and are consistent with the WES color-blind figure guidelines. We also tested

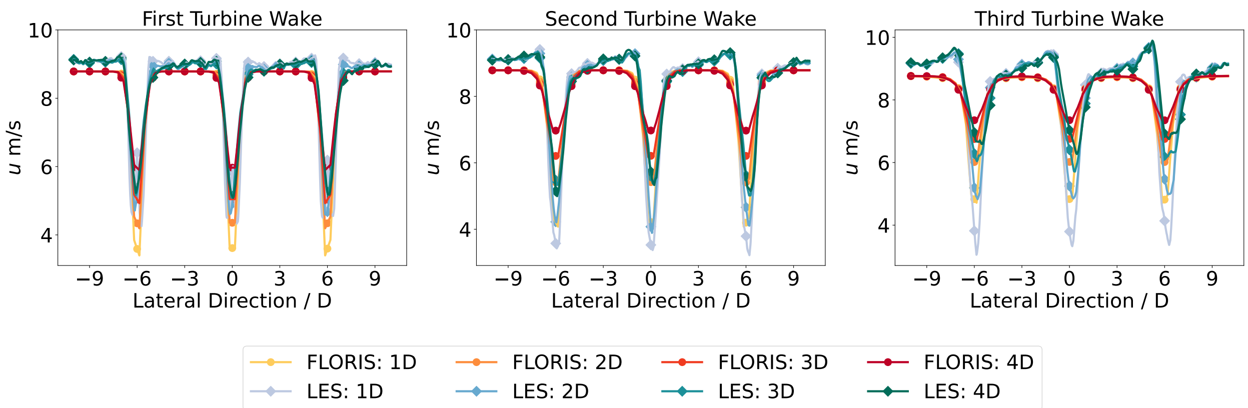

Figure 4: Hub-height profiles of the axial velocity in the wake of first, second, and third turbine rows. Shown are the LES and FLORIS results for the $6D$-spaced wind farm oriented at 225° with the pulse method at $A = 4°$ and $St = 0.3$.

using identical colors for FLORIS and LES with different line styles, but found this reduced readability.

12. The manuscript would profit from a more direct comparison between the Wake Steering and AWM results, especially because it is explicitly mentioned in the title. That comparison could focus on realistic scenarios. For instance, it seems unreasonable that any operator will ever apply AWM with 6 ° pitch amplitude, and also turbine spacings of 3D seem rather short. Could the same methodology of Fig. 14 maybe be applied to wake steering and then shown in the same plot as AWM to get a direct comparison?

    We have updated the results in Figure 14 of the original manuscript to include two wake steering cases, in addition to the two AWM cases that were included previously. The new results are shown in Figure 5 in this document. To address an earlier comment about uncertainty and a point by the other reviewer comment, we have removed the total AEP result in favor of only reporting baseline normalized AEP numbers.

13. Can wake steering and AWM be combined in the same AEP optimizing routine? Then the AEP would profit from AWM in fully waked conditions and from wake steering in partial wake overlaps. Intuitively, that should further increase the AEP gains.

    Combined control methods that harness the benefit of both wake steering and wake mixing are of interest to the authors, and will be considered in future work. With the current capabilities of FLORIS, optimizing wake steering and wake mixing is (perhaps surprisingly) very distinct tasks and writing a joint optimization routine would take significant work. We have included this as an area of future work in the conclusion.

Minor Comments

- Lines 38-39: The following might be relevant for the underlying physics of wake mixing:
  - van der Hoek et al.: "Maximizing wind farm power output with the helix approach: Experimental validation and wake analysis using tomographic particle image velocimetry" (https://doi.org/10.1002/we.2896)

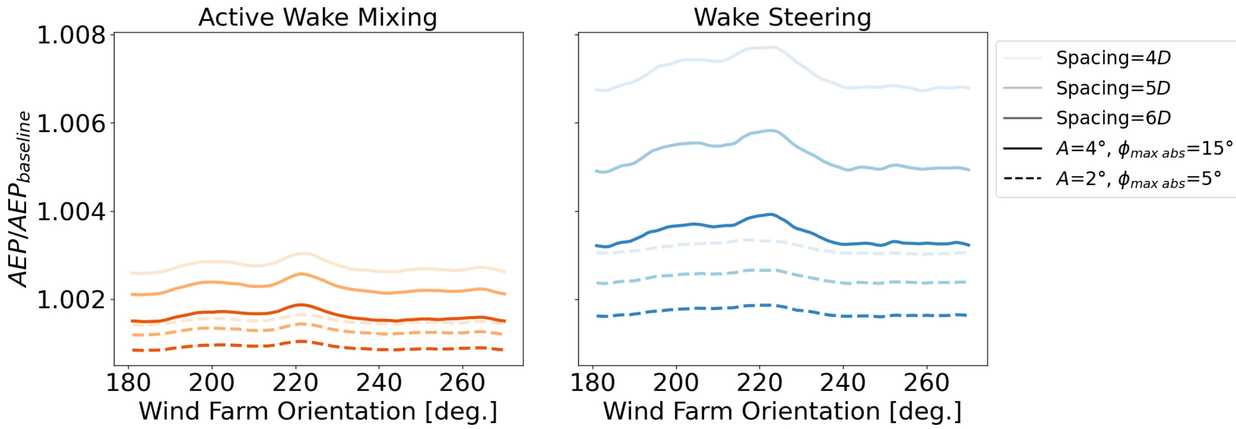

Figure 5: Baseline-normalized AEP as a function of the wind farm orientation angle for turbine spacings of $4D$, $5D$, and $6D$. Shown are two AWM cases ($A = 2°$ and $A = 4°$), two wake steering cases ($\phi_{max\ abs} = 5°$ and $\phi_{max\ abs} = 15°$).

  - Coquelet et al.: "Dynamic individual pitch control for wake mitigation: Why does the helix handedness in the wake matter?" (DOI 10.1088/1742-6596/2767/9/092084)
  - Gutknecht et al.: "The impact of coherent large-scale vortices generated by helix active wake control on the recovery process of wind turbine wakes" (https://doi.org/10.1063/5.0278687)

  Thank you for these additional references.

- Table 3: I would suggest moving the Pulse with St=0.15 from the bottom of the table up to the cases with the same pitch and wind farm orientation cases to facilitate the comparison of the Power gains with the cases in the same conditions.

  This change has been made in the updated manuscript.

- Fig. 5: I recommend plotting different pitch amplitudes in different line styles, to highlight that the lines do not directly compare with each other. This should also be emphasized in the text.

  This figure has been updated so that different cases are shown with different line styles as well as different marker styles (see Figure 6 in this document).

- Line 231: Citation refers to NREL, but the name was recently changed to NLR. Should that be considered here? It might be worth contacting the Editorial board of the Journal for that.

  The authors will work with the journal to ensure that all sources are properly cited. We personally feel that historical publications under "NREL" should remain unchanged, while "NLR" should be used for all future citations.

- Line 272: "All of the LES cases were used" sounds like also the helix and pulse 0.15 case were included in the tuning; however, I assume they were not. This might need clarification in the text.

  This sentence has been updated to clarify that only the baseline LES cases and LES cases of the pulse method at $St = 0.3$ were used for tuning the FLORIS model. The update text is included below:

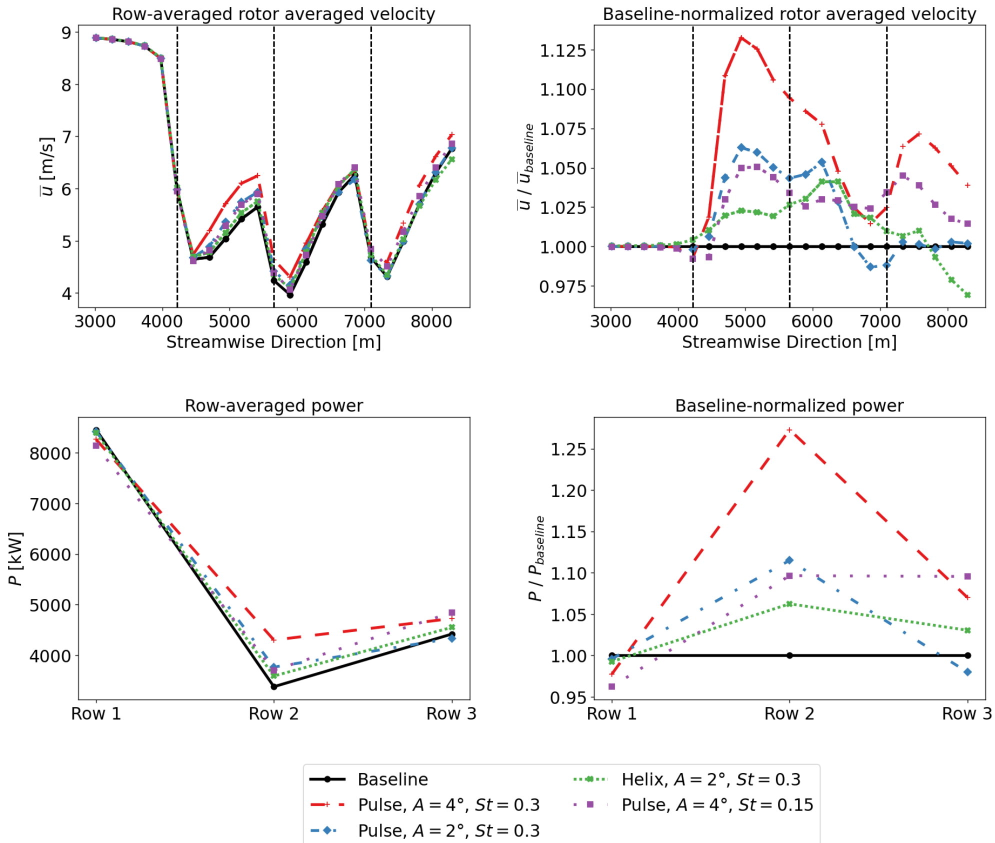

Figure 6: Row-averaged rotor-averaged velocity and power. Both the absolute and baseline-normalized values are shown. Included are results from the $6D$-spaced wind farms oriented with the wind direction at 225°.

"All of the baseline LES cases and the LES cases of the pulse method at $St = 0.3$ were used for tuning the FLORIS parameters. This includes the four baseline LES cases, which were used in the first step of the calibration, and the five AWM LES cases of the pulse method at $St = 0.3$, which were used in the second step of the calibration."

- Line 291: "...does not does..." I guess that's a typo

  The typo has been corrected in the updated manuscript, thank you.

- Lines 305-315: Description of assumptions feels a bit hard to follow due to quite convoluted sentences. Could these assumptions be outlined in a clearer way?

  Following a previous comment, we have updated the manuscript to clarify the main assumptions in the FLORIS model. The three assumptions are that AWM power gains: (1) depend strongly on wind direction and pitch amplitude; (2) are approximately invariant across Region II and earlier Region III wind speeds when turbulence intensity (TI) is low; and (3) diminish beyond a threshold wind speed and/or TI, where AWM is no longer beneficial. These assumptions are aided by Table 2, where the non-zero entries indicate wind conditions where the power gains from AWM have been evaluated (or estimated) and found to be significant enough to justify its use. Higher in Region III, the utility of AWM is certain to diminish due to the lower wake effects, and an upper cutoff point of WS = 15 m/s selected for this study. Regarding TI, AWM is expected to decrease at higher TI because increased ambient mixing reduces the benefit of externally-imposed mixing. AWM is therefore only applied for TI less than 10% in this study, and is assumed to produce no power benefit for TIs above this value.

- How about the loads? (Sorry, I couldn't resist asking that ;-))

  What about loads indeed! That is an excellent question and one that is key to providing a full technoeconomics assessment of AWM. The FLORIS model does not provide an avenue for assessing turbine loads, although a low fidelity modeling tool with this capability would be very beneficial. We've therefore focused the discussion on AEP and included an investigation of loads as future work in the conclusion. If the reviewer is interested, we have performed a comparisons of DELs for different AWM strategies in a recent study found here: https://doi.org/10.5194/wes-10-2449-2025

References

Frederik Aerts, Koen Devesse, and Johan Meyers. Bayesian uncertainty quantification of engineering models for wind-farm atmosphere interaction. Wind Energy Science Discussions, 2025:1–32, 2025.

Kenneth Brown, Gopal Yalla, Lawrence Cheung, Joeri Frederik, Nate deVelder, Dan Houck, Eric Simley, and Paul Fleming. Comparison of wind farm control strategies under a range of realistic wind conditions: wake quantities of interest. Wind Energy Science, 2025.

Joeri Frederik, Eric Simley, Kenneth Brown, Gopal Yalla, Lawrence Cheung, and Paul Fleming. Comparison of wind farm control strategies under a range of realistic wind conditions: turbine quantities of interest. Wind Energy Science, 2025.