March 6, 2025

The authors are very appreciative of the reviewer's time and efforts in evaluating the manuscript. We have made several improvements and clarifications to the manuscript due to the suggestions that were received. Please find our responses to the earlier comments below.

Reviewer 3

The paper proposes a parabolized RANS approach for modeling the effects of turbine generated flow structures in the wake on recovery based on a triple decomposition approach. The methodology is original, innovative, and pertinent to the growing research community in active wake mixing. However, I believe the presentation of the papers and the analysis of the results could be significantly improved based on the comments below.

Major Comments

1. Large-eddy simulation data is used to show the agreement of the proposed model to a high-fidelity model. However, the LES setup is insufficiently detailed to allow reproducibility of the results, more specifically the following questions are unanswered.

    (a) Section 2.1 mentions that representative conditions are based on floating lidar measurements after a selection process and then Table 1 mentions the resulting WS, TI, etc. obtained from the LES. However, it is unclear how the LES has been set up to match the measurements (which is not a trivial process). Furthermore, an incomplete reference is made to Brown et al. 2025, but I could not find this paper anywhere. Please detail.

    The authors apologize for the incomplete reference to Brown et al. (2025). This reference has been properly completed and the manuscript in review can be found here: `https://wes.copernicus.org/preprints/wes-2024-191/`

    (b) The authors mention that the work focuses on larger offshore wind turbines under stable atmospheric conditions (line 70), however it is not discussed whether the LES is a low TI neutral case or effectively a stable case. Details of initialization and precursor setup are important but missing from the manuscript.

    Yes, thank you for pointing out these omissions. In addition to the reference to Brown et al. (2025) for more details, we have added the following sentences: *To generate the precursors, small velocity and temperature perturbations were introduced near the surface to accelerate turbulence development. The low-TI conditions were produced by imposing negative ground surface temperature rates and adjusting the surface roughnesses, followed by 10000s of flow time. As such, the generated conditions were stable atmospheric boundary layers.*

    (c) The authors mention that AMR-Wind can include mesoscale, Geostrophic, Coriolis, actuator line models etc., but the exact setup used is not detailed.

We have updated the language to specify which of the types of available forcing are actually used in this study: *AMR-Wind includes all the necessary physics modules to simulate atmospheric boundary layers (ABLs); included in this effort are ABL forcing, Boussinesq buoyancy, Coriolis forcing, body forcing to maintain the precursor-derived inflow condition in the presence of the turbine's blockage, and body forcing from coupling to OpenFAST for turbine representation using actuator line models (these are the same forcing terms used in Brown et al. [2025] and Hsieh et al. [2025], for instance).*

In addition, we have provided the input files for all the simulations as Supplemental Material so that they are archived with this manuscript.

(d) Is there a reason why the domain lengths are different for different wind speeds?

There is no significant reason behind the different domain sizes between the Med WS and Low WS/High WS cases. This difference was an artifact of evolving test goals, however, the smaller domain is not believed to meaningfully impact the results since the outflow plane is still > 13 rotor diameters from the turbine for the shorter domain.

2. The performance of the RANS vs. LES model in both the baseline and the actuated cases is shown through a qualitative visual comparison of velocity profiles in the form of red and blue lines in Figures 6 - 10. Discrepancies are mostly attributed to the effects of the hub / nacelle and veer / shear in the LES.

   (a) Considering that inclusion of veer and shear are left for future work, would a comparison to an axisymmetric LES of just the turbine rotor not have facilitated a more direct evaluation of the performance of the current model? Please elaborate why the current approach was chosen.

   We agree with the reviewer that, as the LES is currently performed, the comparison could be deemed unfair. A comparison to an axisymmetric LES of just the turbine rotor would have yielded a direct comparison. However, the objective of this work is to provide a usable, proof-of-concept framework that illustrates how a RANS model with a linear stability model can capture most of the phenomena of interest in the LES data. This was adequately demonstrated in the manuscript. The long-term goal, as stated in the conclusions discussing future work, of such a framework is to build it up from common principles towards being able to capture increasing physics complexity, such as shear effects, veer, and asymmetry. As such, it does not serve the current manuscript to remove physical phenomena and perform an ideal LES with uniform inflow and symmetry boundary conditions. This would be discarding the important physics of wind farm LES without informing the potential failure modes of the proposed framework to point to future improvements. Given the simplicity of the resulting flow of an axisymmetric LES without boundary conditions, it can be fully expected that a calibrated axisymmetric RANS model as proposed in the manuscript would fully be able to capture the physical quantities of interest. The current work has the merit of highlighting that the current approach performs well in comparison with complex LES data while also highlighting future improvements.

Though noted in the conclusions, other parts of the manuscript do not clearly lay out these goals and, therefore, additional discussion along these lines was added to Section 3.1.

(b) The performance evaluation would be more objective and comprehensive if quantitative numerical error metrics (e.g. MAE, enhanced recovery, ...) were introduced. This would facilitate the comparison of performance in different wind conditions as well.

In table 4 and section 3.2 of the revised manuscript, we have now included quantitative error comparisons between the RANS with linear stability model and the AMR-Wind LES calculations. Table 4 compares the hub-height streamwise velocity, i.e., the maximum wake deficit, at the downstream positions of x/D=8 and 10. The results are consistent with earlier wake profile comparisons in the manuscript: For the Med and Low WS cases with helix and pulse forcing, the majority of the velocity errors are below 5%, and as expected, the largest differences compared to AMR-Wind occurred for the High WS cases.

(c) Discrepancies between the RANS and the LES are rather large for some of the plots presented, yet they are only very briefly discussed in the text. A somewhat more detailed and objective analysis of the performance of the model would be advised.

Additional material has now been included regarding the differences between the RANS and the LES results. In section 3.1, we discuss the comparisons for the high WS case, and note that the potential core region is overestimated in the RANS model, while it is correctly modeled for the Low and Medium WS case. This leads to discrepancies in both the centerline velocities and rotor averaged averaged velocities. This discrepancy in the High WS RANS model also impacts the later comparisons when the large scale structures are also included (section 3.2). We believe that these discrepancies can be reduced through improvements in the RANS model and additional calibration across a wider variety of wake cases in future work.

3. The parabolized RANS model is described in detail, however some aspects would benefit from further clarification.

(a) I was expecting a body force in the momentum equation 5a to represent the turbine force on the flow. Only later, it became clear that the RANS domain only accounts for the region downstream of the turbine. This should be made more explicit in the paper. Does this imply that the current model is limited to the simulation of a single turbine wake? If so, please mention this explicitly, and discuss in more detail practical applicability of the current model.

The reviewer has raised an excellent point regarding the applicability of the current model. This point has now been clarified in section 2.2 of the manuscript, where we now state that the RANS and linear stability model applies to the wake of a single turbine immediately downstream of the rotor. The inflow and the rotor dynamics are not included in this formulation, and the behavior of more complicated phenomena, such as the merging of multiple wakes, is not considered in this work. However, the intention of the authors is to extend this model in future work so that it can be used for wind farm configurations with multiple turbines. As discussed in the response above, effects like shear, veer, and other flow asymmetries need to be developed first, after which it can then be applied to more complicated configurations.

(b) The impact of wave components on the mean field is represented by the term F_{CS}. The wave field is computed from an analytical linear stability analysis of an axisymmetric piecewise-constant wake profile. However, it is not trivial to understand how the turbine pitch actuations (Table 3) are linked to these modes and hence impact the coupled RANS solution. Are these encoded into the azimuthal wavenumber and temporal frequency of Eq. 15 (and also, is this why the wave component consist of a single exponential basis function rather than a series expansion)? Could this be made more explicit?

In section 2.6 of the revised paper, we present more details regarding the blade pitch actuation parameters and the instability modes used in the current analysis. The connection between the two was discussed more fully in Cheung et al (*Energies*, 2024), but the relevant details are included here for completeness. In that study, different blade pitch actuation strategies were applied to an OpenFAST turbine model simulation using different pitch amplitudes, azimuthal mode numbers, and the desired Strouhal frequency St=0.30. An analysis of the resulting blade loads showed that there was a corresponding fluctuating streamwise blade force that appeared at the same azimuthal mode number and Strouhal frequency. Furthermore, through a spectral POD analysis, we can see that these fluctuating streamwise blade forces then excite a similar response in the near wake (see Yalla et al, 2025). While the radial distribution of the fluctuating blade forces due to the AWM actuation strategy may not exactly match the eigenfunction solutions of equations 22, we believe that it is sufficient to pitch the blades at the specified azimuthal mode number n and temporal frequency ω (or Strouhal number St) to excite the desired instability mode.

The reviewer is also correct in noting that this study considered the impact of a single instability wave, at a single Strouhal number and a specific azimuthal mode number. In the more general case, multiple instability wave components can be included in the analysis, and a summation over all wave components in equation 15 is then required. This would allow for AWM strategies such as the side-to-side actuation to be analyzed, or for the behavior of the higher harmonics to be included in the wake model. However, because nonlinear interactions among instability modes is not within the scope of the current analysis, only a single mode is considered here. This additional clarification is now also explicitly included in section 2.4.2.

Minor Comments

1. The term large-scale coherent structures is used throughout the paper. In an atmospheric boundary layer context, this term is often used for naturally occurring boundary layer streaks, and their impact on wind turbines and wakes has been studied in several papers in literature (see, e.g. Zhang & Stevens https://doi.org/10.1007/s10546-019-00468-x) To avoid confusion, I would propose to add a disclaimer that the structures here refer to turbine-induced structures in the wake only.

   The introduction has been updated to clarify that the focus is on modeling coherent structures generated from the turbine. Moreover, it is mentioned that large-scale coherent structures can arise from other sources, such as the naturally occurring boundary layer streaks in an atmospheric boundary layer, which also affect wake dynamics, and that the modeling framework developed in the paper may be relevant for these processes as well.

2. Table 1: All cases are Low TI, so why include it in the naming convention? This gives the impression that also medium / high TI cases are included in the investigation. They are also inconsistently referred to throughout the manuscript (e.g. Sometimes as Low WS/Low TI, sometimes as Low WS). This should be simplified.

   We simplified the naming of the cases to remove all mention of Low TI since all of the cases were run with the same TI.

3. Considering AMR-Wind is a relatively new code, it would be useful for the community to share the setup files for guidance and reproducibility.

   The public AMR-Wind documentation contains extensive code and user documentation. Of particular interest to the users are a set of walk-through documents that provide concrete, detailed input files for a range of cases. In addition, we have provided the input files for all the simulations as Supplemental Material so that they are archived with this manuscript.

4. The definition of the wave component at the bottom of page 4 is implicitly defined. I am assuming this is a typo and the tilde on the right hand side should be omitted.

   This typo, and other similar typographical mistakes, have been corrected in the revised manuscript.

5. Please introduce all symbols explicitly and uniquely, e.g., σ_k and σ_ϵ are not defined in Eq. 5, x_0 is not explicitly defined, θ is used both as the pitch angle (Eq. 1) and the azimuthal coordinate (Eq. 16), w is never explicitly defined as the azimuthal velocity component (though it is used in the linear stability analysis, etc.

In the revised manuscript, we have corrected several mistakes and clarified the mathematical notation. This includes:

- Describing $C_{1\varepsilon}$, $C_{2\varepsilon}$, C_{μ}, σ_k and σ_ε in the RANS model equations as calibration constants and defined in Sec. 2.3.1.
- Providing an explicit definition of x_0.
- Including a schematic of the coordinate system used in figure 2, and consistently using ψ as the azimuthal coordinate.
- Defining w as the azimuthal velocity
- The azimuthal mode number is now consistently defined as n.

Typos

The manuscript contains quite some remaining typos and textual inaccuracies. I list the ones I noted down here below, but expect there are more. Please revise thoroughly.

1. Section 2.4.1: constatnt - constant
2. Line 261: he wave component - the wave component
3. Line 263: the Frobenius norm of (the difference between?) two successive solutions
4. Line 265: python - Python
5. Line 111: two-dimensions - two dimensions

These typos and grammatical errors have been corrected in the revised manuscript