Review of manuscript "Modeling the effects of active wake mixing on wake behavior through large scale coherent structures" by Cheung et al.

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.
    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.
    c. The authors mention that AMR-Wind can include mesoscale, Geostrophic, Coriolis, actuator line models etc., but the exact setup used is not detailed.
    d. Is there a reason why the domain lengths are different for different wind speeds?
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.

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.

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.

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.

   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?

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.

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.

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.

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.

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.

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.

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