

Review of: “Design and analysis of a spatially heterogeneous wake” by Alayna Farrell, Jennifer King, Caroline Draxl, Rafael Mudafort, Nicholas Hamilton, Christopher J. Bay, Paul Fleming, and Eric Simley

Overall comment:

The submitted manuscript outlines a modification to the FLORIS package to allow for heterogeneous “freestream” flow conditions at each turbine in the wind farm, i.e. heterogeneous wind speed, direction, and turbulence intensity at each turbine location if no turbines were present. Improving wake models in heterogeneous flow, where wake model assumptions break down, is critical for the design and controls communities, and therefore the subject matter is of relevance to this journal. While this referee recognizes the challenge of formulating consistent engineering models which satisfy key conservation equations, I have some concerns about the derivation of the heterogeneous wake model which should be revisited and articulated by the authors, as this would establish confidence that the newly proposed method could be applied in a general model setting. Further, the test problem shown lacks enough detail to be replicated by readers and must be significantly expanded in detail and in explanation as there are occurrences of model success and failure. Since I believe this model has the potential to be useful to the community, but the manuscript submitted should be modified significantly, I recommend a major revision.

General comments:

1. This article would greatly improve with a more formal statement of the research objectives. As discussed in the **Specific comments** points, the Abstract and Introduction are full of comments on issues which affect wake model “accuracy.” Wake models are fundamentally low-order and are typically derived from first principles with explicit assumptions (uniform 2D or 3D flow chiefly among them). It would be helpful to consider this more carefully.
 - a. Define the objectives of the study and model “accuracy” formally in the introduction. There has to be some degree of baseline performance, since FLORIS cannot be expected to capture power production in strongly complex terrain, for example, since the assumptions made at the stage of derivation break down themselves. Is the hope to capture SCADA power data without resolving any terrain or is the hope to capture realistic flow features (e.g. compare well to LES/WRF in complex terrain)? If the latter is not the goal, how can you demonstrate confidence in the former?
 - b. Discuss previous studies which have highlighted issues with uniform inflow formulations and how this study specifically addresses those issues. I recommend expanding the literature review.
 - c. Three previous heterogeneous models are discussed, why are those methodologies not employed or inaccurate such that this study is necessary?
2. Problematically, the proposed method does conserve momentum and gives different values of turbine thrust depending on the size of the control volume drawn around the turbine in complex flow (see discussion below). Many engineering models do not

conserve key quantities, but it is important to derive consistent models from first principles otherwise we will not know when the core assumptions are valid or invalid in a new wind farm or model situation.

3. The new methods would benefit from a validation case of the methods (e.g. a comparison to complex flow RANS/LES rather than just comparing power predictions for one wind farm). It's hard to extrapolate that marginally improved power production modeling for one wind farm generalizes to claim that the newly developed model is an improvement given all of the uncertainties associated with low-order wake models and empirical considerations detailed in the **Specific comments** below.
4. There are a significant number of questions/issues with the results section of this manuscript. I have detailed them below in the **Specific Comments**. If the authors can address these comments the manuscript would greatly improve. Very little information/data is given about the test case and even in this limited test scenario the model 'improvement' is not convincing since it does not outperform homogeneous FLORIS for all cases and there is no explanation given for the varying degrees of success.

Specific comments:

1. Line 5: The abstract should briefly mention the hypothesized causes of heterogenous wind flow. It is not clear to this referee just by reading the abstract the focus of the heterogenous model. Specifically, is this paper addressing heterogeneity due to: 1) site-specific complex terrain, 2) short-time averaging of quasi-homogeneous turbulent flow, 3) wake heterogeneity, 4) etc. This should be stated concisely in the abstract.
2. Line 10: The abstract should explicitly state the key results of the paper. For example, was the new heterogeneous extension to FLORIS successful in the figures of merit of focus for the present study?
3. Introduction:
 - a. The introduction is very brief and can be improved as discussed in **General comments**.
 - b. This introduction assumes significant familiarity with FLORIS. That would be acceptable in a conference paper but not for a journal article, which should be self-contained. For example, the concept of "steady state" time-averaging in the FLORIS name isn't even introduced.
 - c. The introduction should cover wake models more broadly rather than only FLORIS, since this paper is attempting to develop new heterogeneous wake model capabilities for the literature.
4. Equation 1: Define the axial induction factor
5. Line 95: Since this article details modifications to the flow calculation within FLORIS, the authors should *explicitly detail all* of the assumptions within the derivation of the Gaussian wake model to ensure consistency between the analytical wake model formulation and the freestream condition specification in this implementation of FLORIS. For example, the Gaussian wake model (Bastankhah & Porte-Agel (2014)) assumes zero pressure gradients which is then violated in the heterogeneous model.

6. Equation 7: The current proposed method of heterogeneous wind speed, should the local shear coefficient also be modified?
7. Equation 8: How are k_a and k_b affected by complex terrain since the empirical fit to idealized LES calculations performed by Niayifar and Porte Agel assume no terrain
8. Equation 8: It is very unlikely that $k_y = k_z$ in complex terrain. Please perform a sensitivity analysis of the results on this assumption.
9. Equation 10: This equation was also empirically tuned for homogeneous flow and simple terrain and a sensitivity analysis on these parameters must be investigated.
10. Section 3.1 would benefit from a pseudo-code/diagram to improve reader understanding
11. Line 185: The explanation of the selection of interpolation algorithms is insufficient.
 - a. What is the justification of linear Barycentric interpolation? Likely the validity of linear interpolation depends on the complexity of the underlying terrain and should be discussed in more detail.
 - b. *Detailed* comparisons for the extrapolation should be shown in the Appendix and not just mentioned briefly, since often sensors are not widely available at wind farm sites (usually only a few MET towers for many tens of turbines). Therefore, the performance of the extrapolation will likely be critical to model success.
12. Line 200: The 3D velocity field calculation with the power law assumes that the MET towers are in the same vertical location (z) (otherwise the Barycentric interpolation would not be possible I believe). Often MET masts have varying heights, and this may be of interest to consider for the authors.
13. Figures 1 and 2: What is the color axis representing in the sketch?
14. Line 204: What is u_∞ used by the sum-of-squares velocity deficit in the local velocity calculation in heterogeneous flow?
15. Line 204: **Please state the equation for the velocity deficit update explicitly.** For the purpose of this review, I will assume it is as stated below, although if the formulation is different then this discussion may not apply. I assume that this is the formulation also because Figure 6 has a velocity deficit axis which becomes negative.

$u(x, y, z) = U_{init} * (1 - C[\exp(-(y - \delta)^2/2\sigma_y)\exp(-(z - z_h)^2/2\sigma_z)]$ where C is a function of the upwind turbine's C_T which is a function of the *average velocity of the upwind turbine*

$$U_{upwind}$$

The velocity deficit calculation is not consistent with actuator disk theory since $U_{upwind} \neq U_{init}$. The velocity deficit trailing a wind turbine ($u(x, y, z)$ in Equation (4)) is a function of C_T and U_{upwind} . The local calculation here specifies that the velocity deficit trailing a turbine is a function of the turbine thrust coefficient (which is based on the average velocity of the upwind turbine) and the *downwind* velocity.

- a. Illustrative example: A turbine generates a velocity deficit $u_1(x, y, z)$ in a uniform flow field. If in complex terrain, there was a local flow acceleration due to a hill downwind of the turbine, that means the velocity deficit will also increase.

- b. This formulation means that the turbine thrust is not a fixed quantity but depends on the downwind position (since U_{init} is a function of x) and therefore momentum is not conserved (as shown by a control volume analysis). Heterogeneities in U_{init} arise from pressure gradients which are neglected in the Gaussian wake model and FLORIS.
 - c. Perhaps the authors have only used U_{init} in the sum-of-squares calculation?
 - d. The authors should consider Brogna et al. "A new wake model and comparison of eight algorithms for layout optimization of wind farms in complex terrain" (2020) which proposes a modified Gaussian wake model in complex terrain where the spatial U_{∞} evolution is considered in the superposition but not in the velocity deficit calculation aside from modifying the turbine specific C_T .
16. Figure 3: No details of the domain geometry, turbines, etc are shown for this figure and it will be very hard to reproduce. It is unclear to this referee what this figure adds, since it shows different velocity colors but it is unclear whether these heterogeneous speeds are valid/correct with no underlying baseline solution (e.g. from complex terrain LES or LiDAR).
 17. Figure 5: The colormap is confusing or incorrect since the velocity deficit values are not computed
 18. Line 215: Are the 3D velocity deficits (due to the changing inflow angle) included in the local wind direction computation of downwind turbines?
 19. Line 225: More discussion of the sensitivity to grid spacing is warranted. What were the authors' methodology for changing the grid spacing in the y-direction?
 20. Line 230: The current model does not resolve the momentum source/sinks from the complex terrain and therefore does not satisfy momentum conservation even with a fixed spacing in the y-direction.
 21. Line 240: What is the limiting case of wind direction changes that this model can accept?
 22. Line 255: The discussion of turbulence intensity's influence on the power curve deserves some literature review, as this has been studied previously (e.g. "Accounting for the effect of turbulence on wind turbine power curves" Clifton & Wagner TORQUE 2014).
 23. Figure 10: From this figure, the wake losses look very insignificant due to large streamwise spacing. What would be the power production prediction if the wake model was not used and the power of each turbine was computed only using U_{init} ?
 24. Section 4:
 - a. The terrain map should be shown given that this paper aims to represent heterogeneity associated with complex terrain/wind flow conditions
 - b. More details on the SCADA data processing should be given in the Appendix, and ideally, the data would be provided to ensure reproducibility of the results. If the SCADA data must be kept confidential, another test case (with data) must be provided in this paper to ensure reproducibility of results.
 - c. Why was a timestep of 30 minutes chosen for the FLORIS model runs? Have the authors performed a sensitivity analysis on that timescale selection?

- d. Figure 10: The axes are not labeled with the physical coordinates, so the advection time scale of the wind farm cannot be estimated and the results will not be reproducible.
- e. Figures 11 and 12: What do the authors refer to as “atmospheric variations?” Figures of the wind speeds, directions, turbulence intensity, etc. should be included for the MET towers, at least in an Appendix.
- f. Figures 11 and 12: What has the power been normalized by? No details are given on the normalization strategy.
- g. Figure 15: For context, please include a vertical line for each of the cases showing the mean percent error over the datasets overlaid on the histograms.
- h. The wind farm power production per turbine should be included as well. This paper gives no indication of the wake losses at the site.
- i. Table 1
 - i. The heterogeneous model outperforms the homogeneous case when the wind speed is larger (>11 m/s) and there are small wake losses.
 - ii. The model also outperforms homogeneous within 5-11 m/s where wake losses are present.
 - iii. The authors do not give a clear explanation as to why the model performs poorly in low wind speed (when I assume heterogeneity is more significant at the site but I cannot deduce this from data since that data has not been shown). The authors instead show the results in a different metric and claim success. It would be much more valuable for the community to understand and explain why the new heterogeneous model correction sometimes is good and sometimes is bad at this particular site.

Technical corrections:

1. Title: It would be more precise for the title of this manuscript to be “Design and analysis of a wake model for spatially heterogeneous flow”
2. Line 90: Period typo