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

Overall comment:

Thanks to the authors for thoroughly addressing my points and performing a major revision. I appreciate the time the authors spent to consider each point in both of the reviews. The discussion of the model, its motivation, and derivation is much clearer to this referee with the revisions made. However, I still have uncertainty about the wind farm data comparison and I recommend another revision to address these questions.

General comments:

1. Overall, given the lack of detail (due to understandable IP constraints), the field SCADA data comparison is not ideal for proving that the new model has addressed the issue of flow field heterogeneity. In fact, the most convincing use case of the heterogeneous wake model proposed is presented in a separate paper [1]. This referee recommends including the LES test case in this manuscript.

2. Related to Point 1, there still appear to be unexpected results in the comparison with SCADA data that require more investigation and/or explanation. The proposed method does seem to have good merit, and in some cases comes with substantial improvement compared to the homogeneous model (by getting localized u_∞ estimate improvements), but less attention is given to situations where performance is not affected or worse than homogeneous FLORIS. It would be hard for a FLORIS or other wake model user in the community to understand when to use the heterogeneous model versus the homogeneous in a general model setting based on this paper, especially given the poor performance at low wind speeds.

Specific comments:

1. Abstract: The addition of quantitative results in the abstract is helpful, but the discussion has been made selectively. The heterogeneous model does reduce MAE but in fact it increases MAPE and this should be mentioned in the abstract to not appear to be selective by the authors. Based on the discussion, whether the method improves MAE or MAPE compared to homogeneous methods will likely be site-specific (e.g. based on the wind rose) due to poor performance for low wind speeds.

2. Line 28: The reference to Schreiber seems out of place. Data-driven wake model parameter corrections have also been proposed by [e.g. 2,3,4], among others.

3. Brogna *et al*. (2020) should be discussed in the introduction as prior work in the domain.

4. Equation 3: Missing parenthesis.

5. Line 294: What does it mean for “the flow-field grid points to conflict in the rotated grid?” I don’t quite understand this sentence or the stated rotation limiting case. Would this be a case where the rotated grid folds back on itself and has different original points in the same rotated x-y space?

6. The added discussion of the TI model is helpful!

7. Figure 13: Can the FLORIS predictions without wake losses be added to this figure for visual comparison?

8. Some of the newly added sentences have typographical errors (e.g. Line 295), I suggest the authors check over them in detail.

9. Thank you to the authors for including the results of FLORIS without wake losses included. I want to be sure I understand the results you’re presenting.

    a. Comparing Tables 3 and B2:

        i. The wake losses make no difference to turbine specific MAE for velocity of <5 m/s (expected)

        ii. Including wake losses reduces FLORIS MAE only slightly for 5-11 m/s for the heterogeneous model and has no impact on the homogeneous model (unexpected)

        iii. Including wake losses significantly reduces MAE for >11 m/s where we would not expect significant wake interactions (as the rated wind speed is reached), this is also unexpected. There is no impact for the homogeneous model and more impact for the heterogeneous model.

    b. Comparing Tables 2 and B1, including wake loss modeling degrades FLORIS’s performance in the heterogeneous model.

    c. Comparing Tables B1 and B2, why does the heterogeneous model have lower MAE for the farm than homogeneous but higher MAE than homogeneous when MAE is turbine specific?

    d. Given that including or excluding wake effects in FLORIS seems to have a very small impact on the MAE (there is virtually no impact on homogeneous FLORIS MAE), that seems to indicate to this referee that this is not an ideal test case for a wake model.

References

1. Fleming, Paul, et al. "Overview of FLORIS updates." *Journal of Physics: Conference Series*. Vol. 1618. No. 2. IOP Publishing, 2020.
2. Teng, Jian, and Corey D. Markfort. "A Calibration Procedure for an Analytical Wake Model Using Wind Farm Operational Data." *Energies* 13.14 (2020): 3537.
3. Howland, Michael F., et al. "Optimal closed-loop wake steering–Part 1: Conventionally neutral atmospheric boundary layer conditions." *Wind Energy Science* 5.4 (2020): 1315-1338.
4. Shapiro, Carl R., et al. "A Wake Modeling Paradigm for Wind Farm Design and Control." *Energies* 12.15 (2019): 2956.