Review of *The revised FLORIDyn model: Implementation of heterogeneous flow and the Gaussian wake* by M. Becker et al.

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May 3, 2022

The authors propose several modifications to a dynamic engineering wake model and compare its results to large-eddy simulations for a small wind farm using three dynamics cases: a start-up case, a yaw-misalignment change case and wind direction change case.

The article is well written and contains detailed information about the proposed modifications. However, the description of the large-eddy simulations should be improved and the difference between the two models should be quantified instead of providing mostly qualitative statements. More detailed comments are listed below; they need to be addressed before the article can be considered for publication in Wind Energy Science.

Main comments

1. Introduction: It is nice that you have added a list of articles in the introduction. However, I do miss an early work on the Dynamic Wake Meandering model from Larsen et al. (2008) [1], which presents a concept (steady wake solution + turbulent meandering flow) that has been used by several authors that you refer to.

2. Section 2.1: While I am familiar with the near and far wake regions, I have not sure what you mean by the *potential core* (root vortex?), please clarify the physical meaning.

3. Section 2.2: You mention that overlapping wakes from multiple turbines are summed using a root-sum-square. I guess one could also use a different wake summation method here (as for example a linear sum)?

4. Section 2.3.1: You mention *The Gaussian FLORIS model does not have defined borders and it is three-dimensional.* I understand that you refer to the outer boundaries of the wake deficit. To be complete, you could add that the Gaussian wake model has a region near the turbine where the near wake is undefined (in PyWake [3] a constant value is used here based on the maximum deficit), which one could interpret as a border and a different wake region compared to the Gaussian wake region that represents the far wake.

5. Section 2.3.2: You write *An OP considers itself influenced by a foreign wake if the closest foreign OP is less than* $\frac{1}{4}$ *D away.* Where is the value of $\frac{1}{4}$ D based on?

6. Section 2.5: You mention the problem for using C_T values higher than one. You could overcome this by using an alternative relation between C_T and the axial induction, see for example eq. (2) and corresponding discussion of Madsen et al. (2019) [2].

7. Section 3.1: I lack information on the SOWFA simulation setup. What type of atmospheric inflow is applied? Is it a pressure driven neutral inflow, a conventionally neutral inflow (neutral at the surface but with a temperature inversion), are Coriolis forces applied? In addition, it is worthwhile to mention that the actuator line model is coupled to a blade element momentum method (or is this not the case?). You could possibly add a reference to

the numerical setup of SOWFA regarding the sub grid model and other important simulation details. The same questions apply to the nine turbine case presented in Section 3.2. Finally, you write *The wind direction is constant along the x axis.* I guess you refer to the *mean* wind direction, as the LES inflow will describe the distribution of inflow wind directions. This may seem pedantic, but it is an important detail.

8. Section 3.2: How is 60° inflow wind direction change realized in SOWFA? This is not a trivial setup in CFD as it could violate mass conservation. Could it be that you employ a setup similar to Stieren et al. (2021) [4]? Please clarify.

9. Do you use the same yaw controller in both the SOWFA and FLORIDyn simulations in the yaw angle and wind direction change studies? If this is not the case, then this could be a source of the observed differences.

10. Section 2.2: In the discussion you mention several causes for the differences between SOWFA and FLORIDyn. I think you could also mention that the wake superposition method could be investigated, as this is normally a major source of error (at least for steady-sate wake models). In addition, I lack a quantitative analysis, as all statements about the comparisons are qualitative.

11. I would change the title of Section 3.3 (Performance) to Computational Performance or something that makes it clear that you investigate the computational effort and not the performance in terms of accuracy.

12. I lack a sentence in the conclusion regarding the change of using freestream wind speed as travel wind speed instead of the local wind speed, and how this has led to simpler but also a too fast reacting model.

Minor comments

1. Line 206: *wolrd* should be *world*.

2. Line 225: You write *actuator disc theory*, I would refer to this as *1D momentum theory*, but I do understand that both terms can be used.

References

[1] Larsen, G. C., Madsen, H. A., Thomsen, K., and Larsen, T. J. Wake meandering: a pragmatic approach. *Wind Energy*, 11(4):377–395, 2008.

[2] Madsen, H. A., Larsen, T. J., Pirrung, G. R., Li, A., and Zahle, F. Implementation of the blade element momentum model on a polar grid and its aeroelastic load impact. *Wind Energy Science*, 5(1):1–27, 2020.

[3] Pedersen, M. M., van der Laan, P., Friis-Møller, M., Rinker, J., and Réthoré., P. DTUWindEnergy/PyWake: PyWake, 2019.

[4] Stieren, A., Gadde, S. N., and Stevens, R. J. Modeling dynamic wind direction changes in large eddy simulations of wind farms. *Renewable Energy*, 170:1342–1352, 2021.