Review – wes-2024-150

A dynamic open-source model to investigate wake dynamics in response to wind farm flow control strategies

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Review - Anonymous Referee #2

1. General comments

This paper is a very good piece of work on the development of a new wake modeling framework including unsteady effects. It is a highly relevant work for the present literature in the domain. I recommend this paper to be published with minor revisions.

Overall the paper is bit unbalanced between a rather short methodological part (Section 2), and a very large results part (Sections 3 and 4).

Some subsections are quite very large (as 4.2) and could deserve to be split in two separated. At line 346 starts clearly a new sub-topic where this could be divided. Also at 386.

Several figures are using unconventional ways of presenting data, and often too much data was put into them. It takes a lot of time for the reader to understand these figures good.

2. Specific comments

- Line 2: I would rather list in the order "like power maximization, power tracking or load mitigation." In order to logically follow historical development of WFFC discipline (which started mostly for power maximization while the later two are more state-of-the-art research).

- Line 4: "OFF": can you already here specify what the acronym stands for? It would help the reader to understand already here that it's a combination framework.

- Line 102-104: I would add again the mathematical notation in each sentence to help clarify these definitions. "Turbine states x_T consist of … . The ambient states x_amb characterize…"

- Line 141: including the vertical deflection (w component) would be useful in future work not only for terrain effects, but also rotor tilting (which is quite common) or floating wind. I would complete here. In fact, it was observed that the absence of vertical deflection in steady state models (e.g. FLORIS) also create discrepancies compared to DWM models with rotor tilt (e.g. FAST.Farm.).

- Line 161-162: It might be a typo or my own misunderstanding, but why should the number of time-steps equals the number of turbines in the farm (both denoted n_t)?

- Line 166: "power coefficient Cp(u) and thrust coefficient Ct(u) tables (u being the wind speed ahead)". For non-initiated readers.

- Line 166: "cosine-loss law for yaw misalignment". Add a reference for it.

- Line 226: Can OFF handle veer? If not, it can be cited for future work.

- Line 241: is it due to the choice of the cosine-loss factor? Which factor was used and why? (Limitations of this cosine-loss law have been published in the literature, as the loss factor should actually be varying with ambient conditions such as shear and veer and control set-points of the rotor).

- Figure 7: color legend scale for Phi_lim?

- Figure 7: can it really be called a “Pareto front”? As this does not really result from a multi-objective optimization between energy increase and yaw travel. I don’t think these points are really non-dominated.

- Line 256: same comment Pareto front.

- Line 273: “wind farm efficiency predicted by the LuT is indeed an upper limit.” But on the Figure 8 (b) one can see that sometimes the simulated efficiency goes higher than the predicted one (between 200 and 220deg). Why does this happen? The above statement should be changed.

- Line 340: What could be a reason(s) for that? How could this be improved in OFF?

- Line 341-342: Is this a (synthetic) smoothing effect that while the power of some turbine is underestimated, the power of others is overestimated? This should be more clearly stated. Furthermore, is this farm-level smoothing expected to be always the case? Maybe in different scenarios, the mismatch of several turbines would add on top of each other for the farm level.

- Line 346: Here I would suggest to start a new subsection (4.3). 4.2 is overall quite large already, splitting in two can be good. At this line a new (sub-) research question is starting.

- Line 361-363: please make uniform the two results presentation and units (one writes f_cutoff the other one no, one expresses in s^-1 the other one in Hz, one gives the full final value in 0.0027Hz the other one no).

- Line 361-363: 0.0027Hz and 0.0019Hz. How can these frequencies be physically interpreted? To me it is a bit hard to link back to real physics of the flow (very low frequencies no?). As these two results are a main core results of the whole paper (already cited in the abstract), I think it would be great to explain them more and make a link with the physical world. I feel a bit frustrated to not manage to grasp it now.

- Figure 13 (a): please add legend (one should not need to read the caption to see the meaning of the colors). The different lines are for different turbines? Also a bit unclear.

- Figure 14: The figure is a bit messy and unclear. Unconventional way of showing data. It takes time for the reader to grasp the meaning of it.

- Line 381-382: Yes, this is the cosine-loss law correction that should definitely be included for future work to improve this issue. This crucial point should already have been mentioned above also (see comment on the limitation of the cosine-loss above). See also: Tamaro et al. 2024 https://doi.org/10.5194/wes-9-1547-2024

- Line 386: here a new subsection could be started.

- Sections 4.3 and 4.4 are very well written and clear (more than 4.2 that could be clarified).