Referee's comments to wes-2024-79

This work validates the two-scale farm modeling framework by Nishino using a large set of LES simulations. The topic is relevant, especially as wind farms are growing in size and start interacting more with the top of the boundary layer. Results are interesting and well explained.

The only main point I would like the authors to revisit it the interpretation of η_{TS} and η_{FS}. The farm-scale efficiency, η_{FS}, is basically the average efficiency of the turbines in the farm normalized by the maximum efficiency of an isolated turbine derived using Nishino's approach.

According to Nishino's model, the farm is modeled as a distributed sink of momentum, like the equivalent roughness model proposed by Frandsen. It means that the η_{FS} will encompass in an average and distributed sense the effect of the thrust of individual machines. This does include effects that are very much "local", like wakes, but in a spatially-averaged sense and thus unaware of the specific layout.

The turbine-scale efficiency, η_{TS}, represent *de facto* a correction on top of Nishino's model to account for specific farm layouts. The fact that it is >1 sometimes it is not surprising in my opinion, being the farm-scale counterpart, η_{FS}, based on an average layout. Naturally, some layouts will have η_{TS}>1, some η_{TS}<1.

From the discussion in the paper, it sounds like η_{FS} should encompass "atmosphere-to-farm interaction" while η_{TS} only local effect. I find this distinction a bit misleading because:

- η_{FS} could be less than 1 even in a non-atmospheric flow as a consequence of wakes
- η_{TS} appears at first glance to include all wake effects, while instead it is only a correction for the departure of the layout from the spatially-averaged one
- $\eta_{TS} > 1$ is interpreted as a "turbine performing better than an isolated one but in a farm" which is a quite a contradicting statement.

Long story short, I feel that η_{TS} was given too much physical importance while it is simply a correction factor for the equivalent-roughness model. Further interpretations like "local wakes", "flow confinements" seem not evidenced-based.

I think it should be made clearer in introduction, discussion, and conclusion that the scale separation is more a convenient modeling tool rather than the result of different physics playing out. See e.g. discussion in Stevens et al. [1].

I think that after rephrasing the discussion based on these suggestions and the one below, the paper can be accepted for publication.

Specific comments

L22: “Measurements” sound too generic, the cited papers refer to operational turbine data. Indeed, there is vast literature of wake observation through remote sensing (e.g. [2,3,4]) that it is worth mentioning.

L31: the fact that there are velocities reduction within the farm “in addition” to wake is quite philosophical. Apart from pressure-induced effects (like blockage, channelizations, speedups), one could argue that all the momentum deficit in the ABL is the result of superposed wakes. Also, the internal boundary layer growth can be seen as merging and vertically expanding wakes. It should be made clearer that the distinction between the “wakes” and the “farm effects” is merely based on the spatio-temporal scales considered and not due to intrinsically different physics.

L47: I suggest revisiting the word “validate” when referring to the two-scale hypothesis. “Assess”, “test” sound more appropriate and less definitive.

L63: τ_w may have not been defined.

Eq. 3: Nishino and Dunstan also have a σ_1 factor in their Cp equation, please justify $\sigma_1 \sim 1$ used here.

L82: “upper limit” with respect to which independent variable? Is the maximum Ct attainable by changing the induction of the turbines (like Betz’s theory)?

L85: $C_T{}'$ should have an i index but it does not. If as stated later it is assumed constant, it is a good point to state it (e.g. “the i-index is dropped because we assume […]”)

Eq. 4: please explain α right after the equation.

L91: is the thrust or thrust coefficient that needs to be uniform across the farm?

Eq 5: please define explicitly C_p. Is it the average power over the farm divided by an available kinetic energy? Is it the average of the individual C_p? Or something else?

Fig. 13: why do you use a β_{LES} (presumably equal to the velocity ratio U_F/U_{F0}) and then a β from Eq. 1 again? I understand that the first two steps are needed to estimate the ζ which is the only unknown of the model. However, there should be information on, for instance, how close the β_{LES} is from the β, which can be an indication of the physical soundness of Nishino’s model based on control volume analysis vs LES.

Fig. 14: The interpretation of these results it is not very compelling. Here we are comparing farms with the same layout, same capping inversion heights and free atmosphere lapse rate, but different capping inversion strengths (i.e. different blockages and momentum entrainment). These are my take aways:

- When using η_w, η_{nl}, results are not really meaningful because they are based on the assumption that the first row is representative of isolated turbine power, which breaks down in case of blockage.
- η_{FS} is capturing most of the energy losses due to blockage and also wakes (which are local effects), but in an average sense and thus not connected to the farm layout. In other words, $C_{p.Nishino}$ is the efficiency of the farm (including wakes!) but for all possible layouts. Calling this “farm-atmosphere interaction losses” is misleading. $C_{p.Nishino}$ would be less than 1 even

in a non-stratified, uniform inflow, just because of wakes. The fact that $C_{p.Nishino} \sim C_p/C_{p,Betz}$ simply means that the layout considered happens to have losses similar to the average layout adopted by Nishino.

- η_{TS} is only a small correction that accounts for local layout effects not considered in the global Nishino model. I don't agree that this means that the "turbines perform better than if they were isolated" It simply means to me that this particular layout has slightly lower losses than the average layout considered by Nishino.

Fig 16.: I would make this figure bigger, as it is arguably the most important. It shows that the η_{TS} capture changes in the layout (which is evident) and should show that η_{FS} should track the changes in efficiency due to stability. The latter is not very clear since values are similar across different capping inversion heights. I suggest adding the number of not of each bar.

L 326: The conclusion that flow confinement is causing the $\eta_{TS} > 1$ are not supported by specific evidence here. The local-scale efficiency larger than 1 simply means that the turbines do better than those in an average layout. The average layout can be interpreted as an infinitely large fetch of rough elements exerting the same thrust as the turbines over a unit area. η_{TS} will be greater or lower than one for every departure form this idealized average layout. If it is flow confinement or other effects, it was not shown.

Section 4.4.: the error analysis of the analytical model could be made more comprehensive. A linear regression between all the farm efficiencies from LES and model with error metrics (e.g., R^2) should be shown instead of only the overall error (Fig. 20b)

References

[1] Stevens, R. J., Gayme, D. F., & Meneveau, C. (2015). Coupled wake boundary layer model of wind-farms. Journal of renewable and sustainable energy, 7(2).

[2] Hirth, B. D., & Schroeder, J. L. (2013). Documenting wind speed and power deficits behind a utility-scale wind turbine. Journal of Applied Meteorology and Climatology, 52(1), 39-46.

[3] Hasager, C. B., Vincent, P., Badger, J., Badger, M., Di Bella, A., Peña, A., ... & Volker, P. J. (2015). Using Satellite SAR to Characterize the Wind Flow around Offshore Wind Farms, Energies, 8, 5413–5439.

[4] Zhan, L., Letizia, S., & Valerio Iungo, G. (2020). LiDAR measurements for an onshore wind farm: Wake variability for different incoming wind speeds and atmospheric stability regimes. Wind Energy, 23(3), 501-527.