Referee's comment to wes-2024-62

General comments

The paper proposes a tool for wind farm EAP estimation and optimization which is based on previous works. Overall, the work is more an implementation (with some questionable changes) of existing models and methods and not novel enough to be considered a scientific contribution. There are several reasons why the paper should not be accepted:

1. There is therefore little novelty in the methodology presented.
2. The few adaptations done to the models, in particular to reduce the dimensionality from three to two and a proposed added TI model in presence of meandering present some flaws.
3. The results of the validations do not clearly proof any improvement compared to past models
4. The proposed optimized layout as such a small AEP gain compared to the unknown uncertainty of the model that would not be accepted by industry.

In particular, regarding point 2, the application of the flow equations at the hub-height plane is questionable. This is equivalent to assuming a 2D planar wake, which is however profoundly different from a real one which is 3D or, at least axisymmetric if shear is neglected. Ishihara's and Zong's model were not formulated and calibrated for a 2D wake, so their application here is flawed.

If instead the authors meant to model only the hub-height flow of a 3D (or axisymmetric) wake, then there may be an underlying fundamental theoretical mistake. For instance, Zong and Porte-Agel derive their model including the vertical dimension, thus integrating mass and momentum equations not only in x and y, buy also in z. If one wants to evaluate the flow at hub height, they still need to integrate the equations in x, y, z and then evaluate the resulting model at $z = 0$. Getting rid of one dimension before solving the flow equations is a gross error in partial differential equation theory. This can be shown as follows.

The continuity equation integrated over a volume spanning from freestream ($x = -\infty$) to the generic x, and in the spanwise region $y \in [-L, L]$ reads:

$$\int_{-L}^{L}\int_{\infty}^{x}\left(\frac{\partial u}{\partial x}+\frac{\partial v}{\partial y}+\frac{\partial w}{\partial z}\right) dx\, dy = \int_{-L}^{L} u(x,y)dy - 2LU_\infty + 2\int_{\infty}^{x} v(L,y)dx + \int_{-L}^{L}\int_{\infty}^{x}\frac{\partial w}{\partial z}dxdy = 0$$

The momentum equation in conservative form is:

$$\frac{\partial uu}{\partial x}+\frac{\partial uv}{\partial y}+\frac{\partial uw}{\partial z} = -\frac{\partial p}{\partial x} - |f| + \nabla\ \cdot \tau_x$$

Where τ_x are the turbulent and viscous stresses in x. Integrating over the volume yields:

$$\int_{-L}^{L} u^2(x,y)dy - 2LU_\infty^2 + 2U_\infty\int_{\infty}^{x} v(L,y)dx + \int_{-L}^{L}\int_{\infty}^{x}\frac{\partial uw}{\partial z}dxdy = -\int_{-L}^{L}|f|(y,z)dy$$

We here have used the Gauss' theorem and the fact that we assume τ_x at the boundaries of the domain to get rid of the stress term. Substituting the integrated continuity equation multiplied by U_∞ finally gives:

$$\int_{-L}^{L} |f|(y,z)dy = U_\infty \int_{-L}^{L} u(x,y)dy - \int_{-L}^{L} u^2(x,y)dy + U_\infty \int_{-L}^{L}\int_{\infty}^{x} \frac{\partial w}{\partial z} dxdy - \int_{-L}^{L}\int_{\infty}^{x} \frac{\partial uw}{\partial z} dxdy$$

Or:

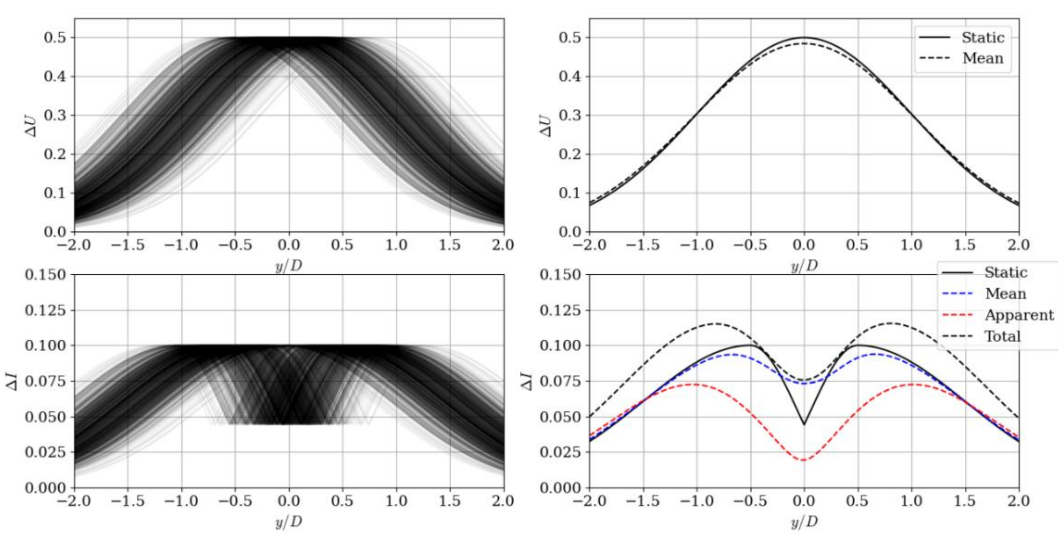
$$\int_{-L}^{L} |f|(y,z)dy = \int_{-L}^{L} \Delta u(x,y)u(x,y)dy + \int_{-L}^{L}\int_{\infty}^{x} \left(U_\infty \frac{\partial w}{\partial z} - \frac{\partial uw}{\partial z}\right) dxdy$$

Thus when considering just the hub height plane instead of x, y, and z (like in Zong's model) or equivalently x and r (like Ishihara and Qian do) the equation changes significantly because:

- $\int_{-L}^{L} |f|(y,z)dy$ is not the thrust of whole the turbine but just its hub-height integral
- $U_\infty \int_{-L}^{L}\int_{\infty}^{x} \left(\frac{\partial w}{\partial z} - \frac{\partial uw}{\partial z}\right) dxdy$ appears and it is unknown a priori

An alternative approach to get rid of the z dimension is to perform a vertical average like in Letizia and Iungo, 2022. In that case, though, the formulation becomes still quite complex and includes dispersive stresses and vertical fluxes of mass and momentum that need to be modeled.

Moreover, the proposed meandering correction for added turbulence intensity based on the simple analogy with the velocity deficit is not justified sufficiently. The authors are missing what Keck et al. 2015 call "apparent turbulence intensity", which is due to the velocity fluctuations caused by meandering of the static (or stable, or MFOR) velocity deficit. This contribution is only a function of the wake shape (mostly its spanwise gradient) and adds up to the static turbulence intensity. The figure below shows a simple Montecarlo estimation of the meandering contribution, obtained by generating 1000 Ishihara-based velocity deficit and added TI profiles that meander base on a wake center which has a standard deviation in time of 0.25 D (left column). While the velocity deficit exhibits the expected spreading (right top), the total TI is even higher when meandering is introduced due to the apparent TI.



Regarding point 3, the claim that the proposed model does better than previous model is not supported by the evidence. In Fig 9 the proposed model does worse than the ones without meandering, while for larger averaging sectors the wake meandering seems to slightly improve results. This points to the fact that the meandering correction, which is smoothing the wake deficit, is likely not representing a physical effect, it is rather reducing the wake strength for larger wind direction bins and possibly leading to some error cancellation. If there was a physical effect being captured, results should improve regardless of the bins choice. It may also be that the way the experimental results are treated for narrow bind sector is somehow killing natural meandering, leading to the experimental results to be biased instead, but his has not being discussed. On the other hand, a model user does not know when and when not to use the meandering correction based on these results.

Other minor comments are provided below just for reference.

Specific comments

- L 10: The Frandsen model is not Gaussian but top-hat. You could add the Gauss-Hybrid-Curl instead [King et al, 2021].
- L 30: consider removing the statement on the Niayifar's models being the best since the Zong's model is used in the work and is again described as the most accurate at L 35.
- L 38: Medici et al. advocate for the internally-driven meandering hypothesis similar to the vortex shed by a cylinder, while Larsen et al., 2008 are the ones arguing that large atmospheric eddies cause meandering. Please amend this paragraph.
- L 46: "hubheight">>"hub height"
- L 50: if the Gaussian shape is assumed it cannot be derived from momentum equation. Momentum equation is instead used to calculate the depth and spread of the Gaussian based on the thrust. Please correct the sentence.
- Eq 1: ϕ is function of both x and y, since $\sigma(x)$, so either $\phi(x, y)$ or $\phi\left(\frac{y}{\sigma}\right)$. Please correct.
- Eq 4: the definition of ϕ should be placed right after Eq 1.
- Eq 13: same as Eq 1, ϕ is function of both x and y.
- Eq 15 and 16: the sign of the y in the limit of k_2 should be negative because we are considering the lower side of the wake. The original formulation used r which is always positive by in this case is different. The k_1 and k_2 should also be 0 if y is negative and positive, respectively, and also this condition is missing.
- L 120: the first and second sentences could be removed. There is no need to emphasize that all the models proposed before Zong are "unsound".
- Figures 5, 6 and 12 are not very informative, the algorithm follows the general "bin" method for AEP estimations and layout optimization presented in countless studies.
- Table 2: the experimental data also are collected based on bins, but from the table it seems that arbitrary 1 and 5-deg bins are used for the simulation instead but not for the experimental data analysis. Ther use of 8% and then 7.7% TI is unclear too.
- L 301: It is not clear which part of the meandering model is fitted to the data. The model shown in Section 2.3 seems to have all the constant predefined so it should be explicated which won has been changed to match the data.
- L 330: the explanation of the mismatch based on the wake rotation is not supported sufficiently. The reference by Qian and Ishihara. 2020, talks about the reduced turbulent production for partial wake overlapping but not based on rotation but on the magnitude of the velocity gradient. The wake rotation is indeed seen to decay quite fast behind the turbine even for low TI (Iungo et al, 2013, Fig. 2b).
- L 380: the expression "boundary conditions" is more suited for the solution of some PDE systems. In optimization context the term "constraint" is more common.
- L441: the optimization without meandering has higher wake losses than which "reference", the optimized with meandering or the real layout? And how can an optimization make the objective function worse?

References

Letizia, Stefano, and Giacomo Valerio Iungo. "Pseudo-2D RANS: A LiDAR-driven mid-fidelity model for simulations of wind farm flows." Journal of Renewable and Sustainable Energy 14.2 (2022).

King, Jennifer, et al. "Control-oriented model for secondary effects of wake steering." Wind Energy Science 6.3 (2021): 701-714.

Larsen, Gunner C., et al. "Wake meandering: a pragmatic approach." Wind Energy: An International Journal for Progress and Applications in Wind Power Conversion Technology 11.4 (2008): 377-395.

Keck, Rolf-Erik, et al. "Two improvements to the dynamic wake meandering model: including the effects of atmospheric shear on wake turbulence and incorporating turbulence build-up in a row of wind turbines." Wind Energy 18.1 (2015): 111-132.

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