Reply to the reviewers of “An analytical formulation for turbulent kinetic energy added by wind turbines based on large-eddy simulation”

22 January 2025

Please note that the reviewers’ comments are in *italic*, our responses in regular font, and the changes to the manuscript in blue color.

Reviewer #3

Major points

- *On line 9, the authors say “The ultimate goal is to insert the proposed formulation, after further improvements, in the WRF model for use within existing or new wind farm parameterizations”. The parametrization proposed in the paper captures the variations of the TKE at a scale much smaller than that of WRF (see for example the peaks in Fig.1 b). How this wealth of information can be integrated on coarse grids as those commonly run in WRF (where maybe you have 2-3 points per diameter)?*

  We did not have room in the abstract to expand on this, but the idea is to integrate Eq. 11 over the volume of each grid cell of the mesoscale model in which the wake is present. We expanded on this in the Conclusions:

  “in order for the proposed formulation to be effectively used for this purpose, the total ΔTKE in each grid cell of the mesoscale model needs to be calculated, but the volume-integral of Eq. 11 cannot be obtained analytically. As such, numerical integration is required, which may add a small computational cost to the simulation.”

- *I think it would benefit the wind energy community to discuss which turbulent scales you are trying to model. I think the wind turbines add a coherent component which has time scales relatively larger than the incoherent turbulent scales.*

  We apologize but we are not expert at coherent versus incoherent turbulence and therefore do not feel qualified to entertain such a discussion. But our formulation does not differentiate between the two, it accounts for all the added TKE that has been reproduced with the LES, regardless of coherence.

- *I appreciate a lot the effort of the authors in developing this new parameterization, however, deriving it from a LES which is also dependent on a turbulent model introduces an uncertainty. This process would be perfect if we could run a DNS but of course we can’t due to the high Re number. Even if we could run a DNS, the actuator disk model (even the actuator line) may add further uncertainties. To my knowledge, changing the sampling point of the velocity in the actuator model, the spreading (as extensively discussed by Martinez in several papers) or the Smagorinsky constant changes the results. I would recommend the authors to add a paragraph in the final manuscript where they address uncertainties in the simulations used to derive this surrogate.*

  We agree and added the following in the Conclusions:

  “Another limitation of our formulation is that its calibration relies on LES results, which introduce several uncertainties, from the sub-grid turbulent model to the sampling method and the spreading of the actuator line model (Martínez-Tossas et al., 2017). Using only real measurements would not remove all uncertainties either, as measurements have their own intrinsic uncertainties, plus each experiment tends to be specific to the chosen setup and therefore difficult to generalize. Even if we used Direct Numerical Simulation (DNS), which we cannot do yet due to the high Reynolds number of the wind flow, resolving the blades correctly would still require an actuator-line model or similar parameterization, which would add some uncertainty."

- *A major source of tke is due to the tower and nacelle as shown by Santoni et al. 2017 (Wind Energy) and others. It affects the stability of the hub vortex, the breakup of the tip vortices and the fluxes. Is it irrelevant for the model here proposed, or it could be incorporated through a modified Ct for example? It would be nice if the authors could share some thoughts.*

  Thank you for bringing the paper by Santoni et al. (2017) to our attention. We were not aware of it. It appears that Santoni et al. (2017) used a rather "stocky" design for the tower and a very elongated shape for the nacelle, both of which are not realistic for real-size wind turbines. Also, they simulated a wind tunnel case, not a real atmosphere case. Thus it is premature to extend their conclusions to real cases. As such, we think it might be premature to insert a treatment of the nacelle and tower in our formulation. In addition, while a couple of our LES datasets included the effect of the nacelle, none included the effect of the tower, thus we would not be able to calibrate any correction or additional parameter to incorporate their combined effect anyway.

- *Line 275: [radial] expansion rate of the wake TKE is independent on the turbine operation but is only a function of the amount of background turbulence. I do not follow this point, if the turbine is operating in off-design conditions it will introduce a lot of turbulence in the wake. This will affect the mixing, fluxes and as a consequence the expansion rate. Maybe this effect I am referring to is taken into account by ε_r ?*

  This point follows directly from Figure 2b, where k_r is clearly independent of C_T. Recall that we defined k_r as the "radial expansion rate", i.e., the rate of change of the variance of the radial Gaussian curve with x, as from Eq. 15:

  $$\sigma_r(x) = k_r\,(x - x_0) + \varepsilon_r D,$$

  and

  $$k_r = \frac{\partial \sigma_r}{\partial x}.$$

  We did not consider any off-design conditions because we had no data for such cases.

- *Line 135: the definition of tke is a bit confused. I would suggest saying "where u', v' w' are the fluctuating velocity"... Otherwise, you use them in Eq.5 but define later. There is also a typo in my draft on line133 "andw". I am not sure the overbar is defined. Please check.*

  The definition was changed as follows:

  "where a bar $\overline{(\cdot)}$ indicates a mean and a prime $(\cdot)'$ refers to a fluctuating component, i.e., the difference between the instantaneous and the mean wind component, e.g., $u' = u - \bar{u}$"

- *Line153 and $\sigma_U^2 \neq \sigma_u^2 + \sigma_v^2$. I do not understand this, why they should be related? I do not see the point you are trying to make here.*

  The sentence in parenthesis was removed.

- *Line 291 I would also suggest because it increases the mixing, and smooth the peak down*

  The sentence was modified as follows:

  "because high turbulence causes a shorter wake than low turbulence, increases mixing, and smooths down the peak."

- *Line 317 I think you refer to A(x) here, because alpha is a constant.*

  Correct, thank you for catching the typo, which is now fixed.

References

Martínez-Tossas, L. A., Churchfield, M. J., and Meneveau, C.: Optimal smoothing length scale for actuator line models of wind turbine blades based on Gaussian body force distribution, Wind Energy, 20, 1083–1096, https://doi.org/10.1002/we.2081, 2017.