Review of the paper “An analytical formulation for turbulent kinetic energy added by wind turbines based on large-eddy simulation” by Khanjari, Feroz, and Archer

The authors propose a new parametrization of the turbulent kinetic energy in the wake of wind turbines. Using LES results and experimental measurements they derive a model based on 5 parameters related to the operational setting of the turbine and spreading of the wake. It is discussed how these parameters are related to the flow physics. The paper is interesting, well written and worth publications.
I have some comments and suggestions that the authors and editor may consider for the final version.

Major

- *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)?

- 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.

- 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.

- 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.

- *Line 275: 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?

Minor

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.

Line 153 and $\sigma^2_U \neq \sigma^2_u + \sigma^2_v$. I do not understand this, why they should be related? I do not see the point you are trying to make here.

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

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