Review of the paper

"**3D Shear layer simulation model for the mutual interaction of wind turbine wakes: Description and first assessment**"

This paper concerns a new shear layer simulation model to predict multiple wake interactions. The suggested model can be considered as an extended version of the model originally developed by Ainslie, 1988. In its original form of the model, steady-state RANS equations are written in a cylindrical form with an assumption that the flow is axisymmetric. As a consequence, the model was not able to quantify the flow if turbines are not located in a row. To overcome this limitation, the authors in the current study developed a new model by writing the governing equations in a Cartesian coordinate so that the model is basically able to predict wake interactions if turbines are placed in a random manner (not necessarily in a row).

The authors stated the main motivation of this work as the fact that the superimposition (e.g., sum of squares) of single wakes, predicted by existing engineering models, is not supported by a physical background. I agree with the authors that physical interpretation of different superimposition methods (e.g., sum of square, linear sum of velocity deficit) is far from being well understood and more research should be performed in this context. However, I do not believe that this work can overcome this limitations because it is based on several assumptions that question the universality of this work. For instance, the model is quite complicated, with respect to simple analytical models, due to the inclusion of several different parameters to estimate the turbulent viscosity and then the flow field in the far wake region. However, at the end, the model predictions do significantly depend on near-wake characteristics, which are fed into the model as boundary conditions. The bottle neck here is the fact that, as far as I understood, the near wake length is simply assumed to be 2 rotor diameter regardless of incoming flow and turbine characteristics which is a very questionable assumption. Moreover, the magnitude of the velocity at the end of the near wake is based on Betz theory. In other words, this study makes the far-wake simulation much more sophisticated but model predictions still depend on very basic and questionable assumptions for the near-wake region.

I have some other major concerns about the development and validation of the model, listed below. Overall, I believe that the paper is not suitable for publication in Wind Energy Science (WES) within its current form.

Major comments:

- In general, the way that the results of "wake interaction" and "square addition" are compared with the LES data is not appropriate and needs major modifications. The main criticism here is that the predictions of 3DSL are used in both cases but model predictions might be inaccurate even for the wake of a single turbine. In fact, figure 8.a shows that "wake addition" method largely overestimates the velocity even for x=10D at y>0, where the effect of the first turbine is only seen (see figure 6). Therefore, the error can be due to inaccurate predictions of a single wake rather than the sum-of-square approach. Having this in mind, comparison of the 3DSL simulation with the LES data for the wake of a single turbine is useful and should be added to the manuscript.
- Figure 8.a: Following the above comment, I expect to see identical results for both "wake interaction" and "wake addition" methods if there is only the effect of a single wake. However, the results shown in the above panel of figure 8.a do not support this! Please explain the reason.
- It is a well-known fact that near wake length depends on several parameters such as the turbulence intensity of the incoming flow as well as turbine characteristics. As mentioned earlier, the use of a

constant value (2 rotor diameters) for the near wake length for all the turbines questions the validity of the whole simulation.

- The justification for the use of a fixed turbulence mixing length is quite poor. Although this assumption leads to results that are in agreement with the LES data, it is not based on any physical evidence.
- Please elaborate how the function $g(y, z)$ is determined.
- The assumption that the lateral and vertical velocities can be expressed as derivatives of a potential function is poorly justified. This assumption implies that the vorticity in the streamwise direction is equal to zero. The authors need to provide more physical explanation to prove the validity of this assumption.
- Page 5, line 24: Please plot the variation of wake radius as a function of downwind location. Wake radius is defined in this paper as the distance between the wake center and where the wake velocity deficit is 0.1%. Why is this value selected? Some other definitions for the wake width such as the standard deviation of a Gaussian curve fitted to velocity deficit profiles can be used.
- Page 6, line 19: The iteration process to estimate the value of D_i is unclear. If the value of C_T is known then the induction factor and consequently D_i can be easily obtained and no iterative process is needed. Please clarify it.
- Page 7, line 8: I agree that within wind farms, the estimation of the incoming velocity is not a very straightforward task as the velocity changes with the streamwise position. However, the flow on the rotor plane cannot be considered as the incoming flow since the flow velocity at the rotor is definitely smaller than the one of the incoming flow due to the induction flow region upwind of the turbine. Instead, I think you should consider the flow on the rotor as the incoming flow divided by (1-a).
- Equation 18: Sum-of-squares superposition is one of the approaches used in the literature. For instance, Niayifar and Porté-Agel (2015) showed that velocity deficit superposition provides more realistic results if the wake growth rate is adjusted based on the value of turbulence intensity in a wind farm. The results based on velocity deficit superposition can be added for the sake of comparison.
- Equation 2: The first terms in the continuity and x-RANS equations are divided by u_i, while other terms are not.

Minor comments:

- Introduction, line 18: The statement “This process generates a wake which propagates downstream” is invalid. The wake velocity deficit is not generated due to the power extraction. Based on conservation of axial momentum, it is due to the axial force exerted on air flow by the wind turbine. For instance, a disk generates no power but it has a strong wake in terms of velocity deficit downstream.
- Page 10, line 6: “cross-strem” should be replaced with “cross-stream”.
- Page 4, line 3: “his model …” should be replaced with “In his model, …”.
- Introduction, line 20: “addition” should be replaced with “additional”.
- Abstract, line 10: I think it will sound better if “the new model” is replaced with “the new model predictions”.
- Abstract, line 12: “… the new model in comparison to a sum-of-square superposition approach” can be replaced with “… the new model predictions in comparison to those of a sum-of-square superposition approach …”.
- Page 6, line 24: u_{nw} is not defined.
- Page 8, line 17: Replace “x” with a cross symbol.