Response to the first referee's review of the paper *"3D Shear layer simulation model for the mutual interaction of wind turbine wakes: Description and first assessment"*

Davide Trabucchi, Lukas Vollmer and Martin Kühn

Referee's general comment

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.

Reply: We thank the referee for the review of our paper and we agree with some reservations about the near wake prediction used to initialise the 3D shear layer (3DSL) model and about the definition of the eddy viscosity model. Before addressing these issues, we would like to clarify the scope of the model, because it could help to better understand the reason of different level of detail applied to the different submodels and boundary conditions in the 3DSL model.

The 3DSL model aims to improve the eddy viscosity shear layer models. They belong to the category of engineering models and are intended for the estimation of the energy production of a windfarm or, in their dynamic formulation, for the prediction of the loads of a wind turbine operating in wake. They are implemented for instance in the commercial software WindPro as alternative to the Jensen/PARK and the Larsen models [Thøgersen et al., 2011].

The use of eddy viscosity shear layer models demands simple and practical approximations to comply with the limited computational time imposed by industrial applications. In this regard, they solve an axisymmetric wake, consider only the streamwise and radial wind components (no streamwise vorticity), use an initial condition to predict the near wake and an empirical eddy viscosity model. The 3DSL model keeps the same basic physical assumption and does not add much complexity. Certainly these models require more parameters and are not as simple as analytical models.

The novelty of the 3DSL approach presented in this paper is two-fold. First the shear layer equations are solved in three dimensions, instead of two dimensions. Second, a new numerical method has been developed to solve those equations. For the demonstration purposes of this new methodology in this paper we applied particular near wake and eddy viscosity models. However, the 3DSL solution is independent of the models used here.

In this sense, any near wake model could be used to initialise the 3DSL model. We chose to develop a simple model based on the disk actuator theory because the one suggested by Ainslie [1988] or by Werle [2015] had a poor agreement with the near wake reproduced with the large eddy simulations used as reference in this paper. To improve the near wake issue, we are working on an approach which includes a prescribed pressure field in the induction zone downstream the wind turbine rotor similarly as in the ECN model WakeFarm described by Schepers and van der Pijl [2007]. However the research about the near wake development is beyond the scope of this paper.

Issues concerning the eddy viscosity model are more delicate. These models have a strong empirical component: When the physics is too complex to be accurately described, practical solutions are implemented including a proper experimental calibration. In our case, we chose to keep the same calibration suggested in the Ainslie model following the approach by Keck et al. [2012] to model local changes of the eddy viscosity.

To summarise, in our paper we presented a new solution method for a three dimensional shear layer problem. Our purpose was to apply the new methodology with well known parametrisations and compare with the two dimensional solutions. To do this, we needed additional submodules which we took from the literature and slightly adapted to our needs. This paper shows the need of not only extending the dimensions of the solution scheme, but also formulating new boundary conditions and eddy viscosity submodels. However, these tasks were out of scope of this paper.

In the future revision, we will reformulate part of the introduction and of the conclusions in order to make our reasons more evident. Furthermore, we will address your specific comments as detailed in the next section.

Referee's 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 $x = 10\,D$ 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.

  Reply: This is true: The inaccurate prediction of a single wake affects the error calculated with the "wake addition" method. The deviation between the 3DSL simulation of a single wake could be added to provide a term of comparison for the error resulting from the "wake addition" and "wake interaction" methods.

  As explained before, we intended to further develop the common eddy viscosity shear layer models. We felt confident to use the 3DSL model for both the "wake interaction" and "square addition" method without separately considering the error linked to the simulation of a single wake, because we demonstrated the equivalence of the 3DSL model and the Ainslie model for the simulation of single wakes in section 3.1.

- 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.

  Reply: The difference is due to different downstream development of the eddy viscosity factor. Figure 1 of this response displays the eddy viscosity factor affecting the wake of the upfront turbine applied in the wake interaction (top panel) and in the wake addition (bottom panel) methods respectively. Until two rotor diameters downstream the second turbine ($X = 8\,D$) the eddy viscosity factor is the same for both the wake interaction and addition methods. Afterwards, a new block of simulation is started for the former method and the filter function F is reset according to the downstream position relative to the second block of simulations. In the wake addition methods no reset of the filter function F is required. This is the reason of the different results for the single wake provided by the wake interaction and the wake addition methods. The same explanation applies to the single wake of figure 9.a in the paper.

  In the revision of the paper, we will consider the radius development of single wakes and we will apply the filter separately for the wakes generated by different turbines. This is supposed to minimise the reported difference between the two methods.

  Corrigendum: In the paper the eddy viscosity factor was not defined in accordance with the corresponding graphics. To correct this, Equations 11 and 12 in the paper will be corrected as in the following equations:

$$f_{y,z}(x) = F(x)\, r^2_{y,z}(x)$$

$$\epsilon_{y,z}(P) = \frac{k\, f_{y,z}(x)\, u'_{y,z}(P)}{\Phi_m(z_H/L_{MO})} + \kappa\, u_*\, z_H$$

- 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.

  Reply: For sake of simplicity in the paper we followed the example by Ainslie [1988], who indicates the length of the near wake to be about 2 D. In the revision, we will be more precise about that implementing the empirical approach used by Lange et al. [2003] and also implemented as option in WindPro [Thøgersen et al., 2011].

- 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.

  Reply: The turbulence mixing length should be a representative scale of the flow geometry [Pope, 2000]. In order to apply the 3DSL, we needed an eddy viscosity model, but this was

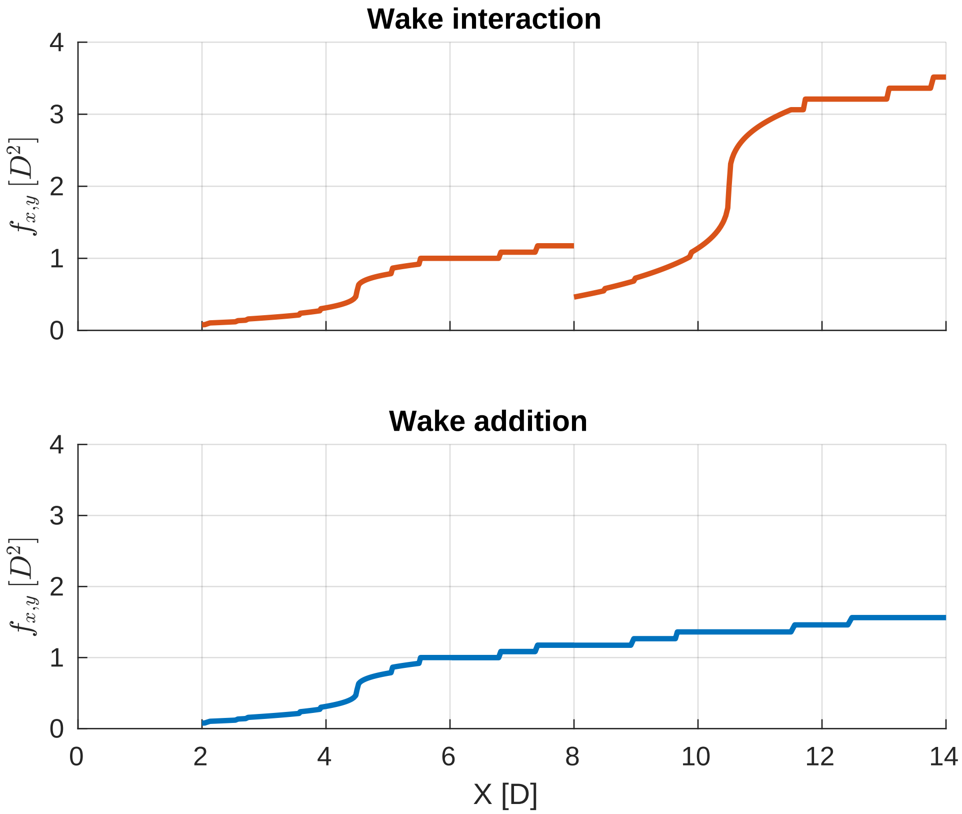

Figure 1: Development of the eddy viscosity factor $f(y, z)$ through the simulation blocks of the wake interaction approach. Here, the bottom and top horizontal axes define the downstream distance from the first upstream turbine (x)

out of the scope of the paper. Therefore we opted for a practical solution based on [Ainslie, 1988, Keck et al., 2012]. For this purpose we initially tried considering a length which could describe the whole wind deficit composed by all interacting wakes at a certain cross-section

The results did not support this choice much, therefore we decided to consider individual wakes to characterise the flow geometry and we used the radius of the turbine to describe their half width as first approximation. To be more accurate, we could have used a wake expansion rate from the literature [Frandsen et al., 2009, Niayifar and Porté-Agel, 2016]. Alternatively also the empirical rules suggested by Larsen [2009] for a simple semi-analytical wake model could have been used to estimate the turbulent mixing length as half width of the wake. We will apply the former of the two mentioned solutions in the revision.

Concerning this point, we would like to remind again that the focus of the paper is the solution of the three dimensional shear layer equations, and not the development of a new model for the eddy viscosity. In principle any eddy viscosity model among the ones suggested in [Ainslie, 1988, Larsen, 2009, Madsen et al., 2010, Keck et al., 2012] could be implemented for the methodology introduced in the paper.

- Please elaborate how the function $g(y, z)$ is determined.

  Reply: $g(y, z)$ is just a help function used to rename the streamwise gradient of u_D. This gradient is evaluated from the known values of u_D, v, w at the previous step applying a central finite difference scheme. More details will bi added in section 2.2.

- 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.

  Reply: The assumption of a potential flow on the vertical cross-section is implicit in common shear layer axisymmetric wake models since they only model the streamwise and radial wind components, i.e. the flow has no tangential velocity (cmp page 4, lines 3 to 7). In our work we aimed to extend the common axisymmetric shear layer models to enable the simulation of multiple wakes and at the same time we wanted to conserve their nature.

- 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.

  Reply: The definitions based on a Gaussian shaped wake are suitable for single wake conditions. We applied them for instance in [Trabucchi et al., 2016] where a single wake was object of the simulations. Here, we needed another way to define a representative length scale for the deficit composed by several wakes. The results of Figures 8 and 9 in the paper showed better results for a turbulent mixing length closer to the width of individual wakes.

  We think that the development of the eddy-viscosity factor is more illustrative to explain the results of the different methods.

- 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.

Reply: The point here is that C_T depends on the inflow velocity $U_{RE,i}$ which is dependent on D_i. Since the latter depends on C_T to satisfy the conservation of mass, an iterative process is needed. We will reformulate the corresponding paragraphs in section 2.5 to explain clearer the need of the iteration.

- 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).

  Reply: This is a very delicate matter for which an accurate induction model within wakes would be required. Here, we chose a practical solution similar to the approach of the ECN model implemented in WakeFarm and explained in chapter 9.5.4 of [Schepers, 2012]. When the inflow wind speed is retrieved from the wake of an upstream turbine, first we use the rotor equivalent wind speed at the rotor downstream position to define C_T. Then, on the same plane we apply a virtual deceleration, i.e. $(1 - a)$, and a virtual expansion of the inflow such that $D_i = (1 - a)D$. In other words we compress the upstream induction zone to an infinitesimal length at the rotor plane. We will reformulate section 2.6 to clarify the methodology applied.

- 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.

  Reply: We will add the linear velocity deficit superposition in the revision of the paper.

- Equation 2: The first terms in the continuity and x-RANS equations are divided by u_i , while other terms are not.

  Reply: v and w are also normalised dividing by u_i in the 3DSL model. The inaccurate definition of the equation will be corrected in the next revision.

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