Response to the second 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

The authors present an extension of the Ainslie wake model, which account for non-axisymmetric wake shapes and includes explicit (rather than assumed) wake-wake interactions. This is an important step towards an intermediary between existing engineering models and CFD. However, the paper has some shortcomings in presentation and in content.

I miss a description of the evolution of the wake shape in the new model. What does the wake cross section look like in the near wake? Figure 7 shows this in the far wake, but it would be useful with a line plot of the deficit profile in the cross stream or vertical direction at several downstream distances.

The comparison with FLaP in the case of an axisymmetric wake does not add anything significant to the manuscript. For a single, axisymmetric wake the equations reduce essentially to the Ainslie model, and section 3.1.1 is akin to a verification of the numerical scheme against another numerical implementation. It is nice to know that this has been done, but the details can be omitted from the paper.

In my view the paper presents a novel idea, which has the potential to move engineering wake models forward. The weak part of the model in its present state is the near wake model, which is taken directly from FLaP/Ainslie. The authors ought to have critically addressed the near wake formulation, which is overly complicated with a great number of constants and parameters, yet seem responsible for much of the difference between the model and the reference field.

The paper is publishable in its present form, but can be improved with further work. At the very least the authors should adopt the turbulence mixing model introduced at the end, almost as an afterthought, from the beginning of the paper instead.

Reply: Thank you for the comments and suggestions regarding our paper. With the paper we aimed to present a new model for simulation of the mutual wake interaction outside the induction zone downstream the rotors. For this reason, the main focus was more on the solution of the shear layer approximation of the steady Reynolds Averaged Navier Stokes equations for non-axisymmetric wakes than on the development of accurate near wake or eddy viscosity models.

In the paper, we implemented the near wake and the eddy viscosity submodules for an explanatory scope. In real applications, appropriate models of both the near wake and the eddy viscosity should be chosen or developed depending on the purpose of the simulations. Furthermore, all the empirical parameters included in their formulation should be calibrated by means of an optimisation process targeting the error of the most representative variables of the simulation (e.g.

the characteristics of the average wake deficit at a certain downstream distance as for instance in [Madsen et al., 2010]) with regard to enough reference cases. Data for a generally valid calibration were not available to our research, therefore we applied models already available in the literature.

Specifically about the near wake, we think that a thorough study belongs to the outlook of the paper. However, the analysis of the 3DSL dealing with results produced implementing also different near wake models [Werle, 2015, Ainslie, 1988] could be added if requested.

Regarding the turbulent mixing length we will follow your recommendation: We will consider the eddy viscosity model introduced at last in the paper from the beginning. To improve that, we will consider the wake expansion as reported in [Rathmann et al., 2006].

We included the simulations of axisymmetric wakes because we combined the calibration proposed by [Ainslie, 1988] with the eddy viscosity model by Keck [2012] and we wanted to verify their compatibility by means of the comparison against FLaP.

Referee's major comments

- Add a description of the simulation time for a single wind speed and direction inflow case at least for the three-WTG example given. It is an important aspect in adopting a new model to understand how practical it is from an operational point of view and how much investment is needed in terms of coding.

  Reply: We will add these details in the revision of the paper as requested.

- P 4, line 18: can you be more specific on how the downstream step size is evaluated at each cross section? It would help a reader who desired to implement the model and work on refining it.

  Reply: Section 2.2 will be extended including more details about the numerical implementation of the model and about the downstream step in relation to numerical stability.

- P 5, equation (8): $(x - 4.5)^{1/3}$ becomes complex for $x > 4.5$. Are the limits $x >< 5.5D$ meant to be at $4.5D$? If not, then specify in the text how you handle the complex values in $F(x)$, or better re-write the equation so it is clear mathematically. For example, if you end up just taking the real part.

  Reply: The limits in the paper are correct. In equation (8), with $(x - 4.5)^{1/3}$ we intended the real cube root of $(x - 4.5)$. We will make it clear in the revision.

- P5, lines 21-24: are the characteristic turbulence length scales r_y and r_z updated at each vertical cross-section as the wakes are propagated forward? A drawing would help make the calculation of r_y and r_z easier to understand – illustrating the averaging of all deficits corresponding to the same y(z) and finding the width.

  Reply: Yes, the length scales r_y and r_z are updated at each vertical cross-section as the wakes are propagated forward. In the revision we will add the explanatory graphics as suggested.

- P6, line 11: the REWS concept has a specific meaning in the literature on power curves, where it signifies the cubic root of the average kinetic energy flux over the rotor. This is not the same as the average wind speed on the rotor plane. If you do mean the REWS in the power curve sense, then please include a reference to Wagner et al, Wind Energy 8, 993 (2011) at this point and rephrase the parenthesis at the end of line 11 (or replace it with a formal definition in an equation). If you meant instead the arithmetic mean of the wind speed on the rotor plane, then please use a different notation than REWS throughout the manuscript.

  Reply: We agree with the reviewer that the definition of the rotor equivalent wind speed is not accurate in the paper.

  After some thinking, we realised that the inflow wind speed should be defined in the same way as the inflow wind speed used to derive the thrust coefficient curve was evaluated.

  In our paper, we used the thrust coefficient curve provided by the commercial software WindPro. We assume that it was not evaluated using the rotor equivalent wind speed. Accordingly, we will use the hub height local wind speed to define the inflow.

- P 6, equation (13): please provide a reference for this equation.

  Reply: Equation (13) is derived from the actuator disc concept as explained for instance in section 3.2 of [Burton et al., 2011]. We will include this reference in the revision.

- P6, equation (16): include a reference to Figure 2, when discussing the stream tubes.

  Reply: We will implement this suggestion in the revision of the paper.

- P7: add more reasoning for breaking the calculation into blocks. What is the purpose or what problem does this approach solve?

  Reply: The simulation needs to be broken down in blocks because the near wakes of the downstream wind turbines need to be considered sequentially. This will be explained also in the revision of the paper.

- P7: in the description of how the simulation is divided into blocks, a drawing would help the reader and make the conceptual structure of the calculations clearer.

  Reply: The revision of the paper will include the suggested drawing.

References

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