Review of *The curled wake model: A three-dimensional and extremely fast steady-state wake solver for wind plant flows* by Luis A Martínez-Tossas et al.

Reviewer: M. Paul van der Laan, DTU Wind Energy

The authors employ an existing parabolic Reynolds-averaged Navier-Stokes (RANS) model of a single wind turbine wake including wake steering and they extend the model to 3D wind farm flows. A model derivation is presented and results of three test cases are discussed using field measurement and large-eddy simulations. The article is well written and provide interesting content. However, I do lack a model verification in the form of a grid refinement study and technical information regarding the model setup is missing. The validation study is interesting but could be improved with quantitative statements about the actual differences. I have listed main and minor comments below, which should be addressed in order to accept the article as a publication for Wind Energy Science.

The authors would like to thank the reviewer Paul van der Laan for the excellent review and attention to detail in all the formulations. We have added an appendix with a grid refinement study and expanded the information about the model setup. We added more quantitative comparisons to the manuscript. The responses to all comments are below marked in blue.

Main comments

1. It is nice that you provide a derivation of the model. I have derived the model following your steps, but I lack some information to arrive at the final form (Equation 9):

   (a) Equation 5: What is Δp_w? Should it be Δp?

   Yes, this was a typo and has been fixed in the manuscript.

   (b) Equation 7: In order to arrive at this equation one also needs to neglect the viscous term, which you forgot to mention.

   Yes, we neglect the viscous term in the formulation, and this has been included in the manuscript:

   "We assume that the viscous effects are small (high Reynolds number limit), and are neglected in the rest of derivation."

   (c) Section 2.2: You forgot to mention that you use the Boussinesq hypothesis for the Reynolds-stress:
   $\overline{u_i'u_j'} = \frac{2}{3}k\delta_{ij} - \nu_T\left(\frac{\partial \overline{u}_i}{\partial x_j} + \frac{\partial \overline{u}_j}{\partial x_i}\right)$

   The turbulent kinetic energy k could be absorbed in the pressure terms that you are neglecting, so we can write:

$$\overline{u'u'} = -2\nu_T \frac{\partial u}{\partial x}, \qquad \overline{u'v'} = -\nu_T \left(\frac{\partial \overline{u}}{\partial y} + \frac{\partial \overline{v}}{\partial x} \right), \qquad \overline{u'w'} = -\nu_T \left(\frac{\partial \overline{u}}{\partial z} + \frac{\partial \overline{w}}{\partial x} \right)$$

Following your proposed decomposition we get:

$$\overline{(U' + \Delta u')(U' + \Delta u')} = -2\nu_T \frac{\partial \left(\overline{U} + \overline{\Delta u} \right)}{\partial x},$$

$$\overline{(U' + \Delta u')(V' + \Delta v')} = -\nu_T \left(\frac{\partial \left(\overline{U} + \overline{\Delta u} \right)}{\partial y} + \frac{\partial \left(\overline{V} + \overline{\Delta v} \right)}{\partial x} \right),$$

$$\overline{(U' + \Delta u')(W' + \Delta w')} = -\nu_T \left(\frac{\partial \left(\overline{U} + \overline{\Delta u} \right)}{\partial z} + \frac{\partial \left(\overline{W} + \overline{\Delta w} \right)}{\partial x} \right.$$

!

Subtracting the mean U-momentum contributions by assuming that the Boussinesq hypothesis holds for the mean background flow we obtain:

$$\overline{(2U'\Delta u' + \Delta u' \Delta u')} = -2\nu_T \frac{\partial \overline{\Delta u}}{\partial x},$$

$$\overline{U'\Delta v' + V'\Delta u' + \Delta u' \Delta v'} = -\nu_T \left(\frac{\partial \overline{\Delta u}}{\partial y} + \frac{\partial \overline{\Delta v}}{\partial x} \right),$$

$$\overline{U'\Delta w' + W'\Delta u' + \Delta u' \Delta w'} = -\nu_T \left(\frac{\partial \overline{\Delta u}}{\partial z} + \frac{\partial \overline{\Delta w}}{\partial x} \right),$$

The author's response is included in the response to comment 1. d) below.

(d) In addition, when substituting the above in Equation 7 of the article and using Equation 8 we get:

$$\frac{\partial \overline{\Delta u}}{\partial x} = -\frac{1}{\overline{U} + \overline{\Delta u}} \left[\left(\overline{V} + \overline{\Delta w} \right) \frac{\partial \overline{\Delta u}}{\partial y} + \left(\overline{W} + \overline{\Delta w} \right) \frac{\partial \overline{\Delta u}}{\partial z} \right.$$
$$\left. + 2\nu_T \frac{\partial^2 \overline{\Delta u}}{\partial x^2} + \nu_T \left(\frac{\partial^2 \overline{\Delta u}}{\partial y^2} + \frac{\partial^2 \overline{\Delta v}}{\partial x \partial y} \right) + \nu_T \left(\frac{\partial^2 \overline{\Delta u}}{\partial z^2} + \frac{\partial^2 \overline{\Delta w}}{\partial x \partial z} \right) \right]$$

This is not the same as the final result presented in Equation 7 of the article, where the terms related to the streamwise gradients seem to be neglected:

$$2 \frac{\partial^2 \overline{\Delta u}}{\partial x^2} + \frac{\partial^2 \overline{\Delta v}}{\partial x \partial y} + \frac{\partial^2 \overline{\Delta w}}{\partial x \partial z} = 0$$

Please clarify. If my derivation is correct and this assumption for the Reynolds-stresses is indeed required, then you should add the assumption to the article.

Yes, the derivation is correct. The term can be written as

$$2\frac{\partial^2 \Delta u}{\partial x^2} + \frac{\partial^2 \Delta v}{\partial x \partial y} + \frac{\partial^2 \Delta w}{\partial x \partial z} = \frac{\partial}{\partial x}\left(\frac{\partial \Delta u}{\partial x} + \frac{\partial \Delta v}{\partial y} + \frac{\partial \Delta w}{\partial z} \right) + \frac{\partial^2 \Delta u}{\partial x^2}$$

Using the continuity equation $\frac{\partial \Delta u}{\partial x} + \frac{\partial \Delta v}{\partial y} + \frac{\partial \Delta w}{\partial z} = 0$, the remaining term is $\frac{\partial^2 \Delta u}{\partial x^2}$. This means that there is only one term in the equation that has been neglected. This term was neglected by assuming that it was small and it has been included in the derivations. We have also included the turbulent eddy viscosity hypothesis in the manuscript:

"the Reynolds stresses are modeled using the turbulent-viscosity hypothesis"

We have attempted different approaches for the derivations. The goal of this work is to take into account the turbulent stresses through the use of an effective turbulent viscosity and the gradients of the wake deficit solution only. A new appendix has been added with a re-formulation that invokes the turbulent-viscosity hypothesis for the base and wake deficit solutions individually.

2. Equation 12: When you introduce an additional constant in the effective eddy viscosity, you are actually scaling your mixing length scale by a factor $\sqrt{C}$ and you can write an effective mixing length in the same form as proposed by Blackadar using an effective von Kármán constant and an effective maximum length scale:

$$\ell_{\mathrm{m,eff}} \equiv \ell_{\mathrm{m}}\sqrt{C} = \frac{\kappa z\sqrt{C}}{1+\frac{\sqrt{C}\kappa z}{\sqrt{C}\lambda}} = \frac{\kappa^* z}{1+\frac{\kappa^* z}{\lambda^*}},$$

with $\kappa^* = \sqrt{C}\kappa$ and $\lambda^* = \sqrt{C}\lambda$. Hence, the constant C is not an independent constant. If you use C =8, then κ^* = 2.83κ, which seems very high to me. In addition, the maximum length scale λ also represent a boundary layer height that needs to be adjusted for each flow case, see for example van der Laan et al. (2020), where a similar limited-lengthscale turbulence closure is discussed.

The reviewer is correct in pointing out that the scaling factor used in the formulation of the mixing length has a significant impact on the value of the eddy viscosity and the diffusion represented in the parabolic equations that are solved. In the current work, the model assumes that the mixing length from the ABL can be used directly, and the effects of the aggregate additional turbulent diffusion from the wake are represented by the constant correction factor C. This approach has the effect of increasing the turbulent diffusion, which is required to reach a sensible wake recovery rate. An alternative approach to this problem would require that the mixing length take into account the mean flow gradients introduced in the wake. Regarding the maximum length scale λ, the current formulation of the curled wake model calculates the mixing length using gradients in the mean ABL flow. In the formulation used here, λ is a constant that is representative of the maximum mixing length in the free atmosphere. No additional formulation is included to vary the mixing length with boundary layer height or atmospheric stability.

3. Section 2.1.1: Here you mention that any background flow can be chosen. However, you do state that the background flow should also satisfy the RANS equation of *U*-momentum (Equation 6), so this is a requirement of the background flow that is worth to mention explicitly. For example, using experimental field data could have included effects of Coriolis and/or atmospheric stability that you are not considering in Equation 6.

Yes, the background flow should also satisfy the RANS equations. We agree that the Coriolis term should be included in the equations. We have re-derived the equations and included the Coriolis term to have a more complete description of the flow.

4. How do you model the thrust coefficient for yawed cases? Do you use an analytical relation between the thrust coefficient and the yaw angle?

   We use a relation of cosine squared for the thrust and power coefficients. This has been added to the text:

   "The power and thrust coefficients are computed using the tabulated value at zero yaw angle using Equation 17. This relation has been used in previous work, but new research indicates that these functions are not necessarily powers of cosines and can be turbine specific. The model presented allows any function to be used to relate the power and thrust coefficient as a function of yaw angle and future work will be focused on improving the functional relations between thrust, power and yaw angle."

5. Have you performed a grid refinement study to verify the model? You mention an order of grid spacing for x and y to obtain numerical stability in Section 3 but how does the flow solution behave with grid refinement? This is an important question that should be addressed in order to accept the article publication. This also applies to the vertical spacing (z).

   Yes, we have performed a grid refinement study and have added to the manuscript in the appendix. This statement has also been added to the text:

   "Our tests have shown that the implementation has a converged and stable solution when using a grid resolution on the order of $\frac{D}{\Delta y}$10 in the spanwise directions (y and z) and $\frac{D}{\Delta x}$ 20 in the streamwise direction. A grid convergence study is shown in Appendix A.

6. What is the actual chosen grid spacing and how large are the domain dimensions? Is the grid spacing uniform or is there also grid stretching?

   We use a grid spacing in all simulations that satisfies the conditions from the grid resolution study. Yes, the grid spacing is uniform. We have added the following statement in the manuscript:

   "All the simulations and results presented were performed using uniform grid spacing."

7. You use a logarithmic inflow in your model representing a neutral atmospheric surface layer. At the same time you apply a mixing length model with a maximum set length scale, representing an idealized (stable) atmospheric boundary layer. This combination does not make sense to me. I would either use a logarithmic inflow with a mixing length that represent the atmospheric surface layer (κz) or I would use an idealized atmospheric boundary layer velocity profile with the mixing length profile including a maximum set value.

   Yes, we use a logarithmic inflow to represent the atmospheric boundary layer. The model used (Blackadar 1962) does what the reviewer suggests. The model used behaves as κz near the ground and has a maximum value of λ=15m.

8. The computational effort in order of seconds for a single flow case is impressive. However, a wind farm layout/control optimizer would most likely use AEP in the objective function and calculating the AEP would require in order of 10^3-10^4 flow cases. Hence, the presented model is still quite expensive for an optimization process where the AEP is required for each iteration.

   Yes, we agree that it is possible to make the solver faster. The current implementation has not been optimized for performance, but based on the analysis in section "3.1 Computational Cost" the order N of computational cost would allow for optimization and shared memory parallelization. We have included these statements in the manuscript:

Section 3.1:

“We note that this version of the model has not been optimized for performance and future work will include code optimization and shared memory parallelization.”

Conclusions:

“Future work will focus on comparing the model with RANS, improving the turbulence model without compromising computational cost, improving the near wake, implementing a vortex decay model, using the solver for yaw-angle optimizations in a wind plant, and improving code performance to increase speed.”

9. How large is the wind direction bin used to bin the SCADA? If this bin is small, i.e. 5°, have you considered applying wind direction uncertainty as a Gaussian filter of several models results for different wind directions? See for example Gaumond et al. (2014) and van der Laan et al. (2015).

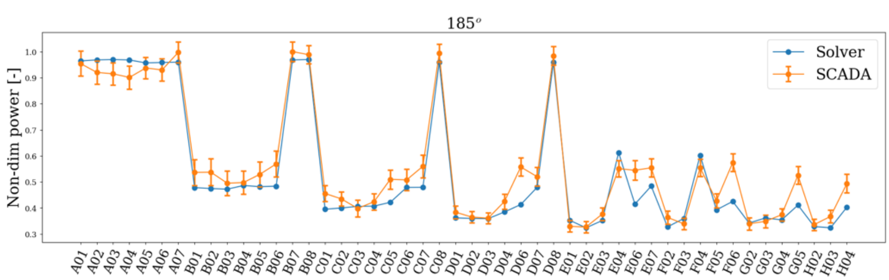
All operational data from the Lillgrund wind plant was organized by wind speed, turbulence intensity, and wind direction into bins of width 1 m/s, 2%, and 5°, respectively. Organizing operational data in this way collects similar observations into subsets and supports easy comparison with model data. The authors are aware of the wind direction uncertainty studies suggested by the reviewer, as well as recent work by Simley et al. (2020). However, the work presented in the current manuscript focuses on the development of the wind plant solver. Uncertainty propagation and quantification studies for the curled wake model will be undertaken in subsequent work. We have added the following sentence in the manuscript:

“The SCADA was organized by wind speed, turbulence intensity, and wind direction into bins of width 1 m/s, 2 \%, and 5^o.”

“Future work will focus on including wind direction uncertainty in the curled wake model \citep{Gaumond2014,van2015b,simley2020design}”

10. It would be more fair to plot the standard error of the mean as error bars in Figure 4, so the standard deviation normalized by the square root of the number of bin samples.

We agree that there are other ways to plot the data. We prefer to show standard deviation to show the range of the experimental data. The results are not intended to show the uncertainty in the data, but more the range of operation. Here is one of the plots with the standard error for reference.



11. Section 4.3: I lack information on how this test case was performed:

    (a) Which wind turbine was used, NREL-5MW?

Yes, this is the NREL 5MW. The following statement has been added to the manuscript:

"The turbine aerodynamics properties and control system are derived from the NREL 5MW reference turbine."

(b) What was the wind direction, 270°?

Yes, the wind direction is 270 and the following sentence has been modified to include this: The simulations use a precursor simulation from a neutral atmospheric boundary layer with roughness height of $z_0=0.15$[m], wind direction of 270^o and wind speed at hub height (90[m]) of 8 [m/s].

(c) What was the wind farm layout (or spacing) for each wind farm case?

The spacing for the cases with 4-by-4 turbines was 10D and 2.5D in the streamwise and spanwise directions and 10D and 3D for the cases with 4-by-3 turbines. We have updated the table with the cases to include the spacing.

12. The article could benefit from more quantitative statements when validating the model. How large are the difference with either measurements or LES? What would these differences mean in terms of AEP?

We agree that quantitative statements are needed to improve the comparison of the model with data. We have reported errors for some of the results in the manuscript. The main objective of the paper is to present this new hybrid RANS-analytical framework for wind turbine wakes. Future work will focus on thorough comparisons with uncertainty quantification for AEP calculations.

13. Figure 8: The difference between individual wind turbine powers are actually quite large and I would not consider this a good agreement. This does not mean that your model is not performing well, but it could be that the LES model has not reached a steady-state as you also point out.

We agree with this statement. The precursor simulation used for the LES had streaks that persist for a long time. We have used the LES precursor with the streaks as the background flow solution to the model. Now the results from the model agree much better with the LES. We also included standard deviations in the power plots to show the range of power from the LES.

14. It would make sense to me to compare the model with an elliptic RANS AD model, which would be a more fair comparison with respect to using LES. You could consider this for future work.

Yes, we agree with the reviewer. We have compared with LES and SCADA because it is what we have available and offer the highest fidelity for model validation. We are planning on comparing the model to RANS simulations and improving the formulation based on RANS. This has been added to the conclusions:

"Future work will focus on comparing the model with RANS, improving the turbulence model without compromising computational cost, implementing a vortex decay model, using the solver for yaw-angle optimizations in a wind plant, and improving code performance to increase speed."

Minor comments

1. Line 140: Equation 1 should be Equation 7.

Thank you, this has been corrected.

References

Gaumond, M., Réthoré, P.-E., Ott, S., Peña, A., Bechmann, A., and Hansen, K. S.: Evaluation of the wind direction uncertainty and its impact on wake modeling at the Horns Rev offshore wind farm, Wind Energy, 17, 1169, 2014.

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