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.

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

$$\begin{aligned}
\overline{\left(U' + \Delta u'\right)\left(U' + \Delta u'\right)} &= -2\nu_T \frac{\partial \left(\overline{U} + \overline{\Delta u}\right)}{\partial x}, \\
\overline{\left(U' + \Delta u'\right)\left(V' + \Delta v'\right)} &= -\nu_T \left(\frac{\partial \left(\overline{U} + \overline{\Delta u}\right)}{\partial y} + \frac{\partial \left(\overline{V} + \overline{\Delta v}\right)}{\partial x} \right), \\
\overline{\left(U' + \Delta u'\right)\left(W' + \Delta w'\right)} &= -\nu_T \left(\frac{\partial \left(\overline{U} + \overline{\Delta u}\right)}{\partial z} + \frac{\partial \left(\overline{W} + \overline{\Delta w}\right)}{\partial x} \right)
\end{aligned}$$

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

$$\begin{aligned}
\overline{(2U'\Delta u' + \Delta u'\Delta u')} &= -2\nu_T \frac{\partial \overline{\Delta u}}{\partial x}, \\
\overline{U'\Delta v' + V'\Delta u' + \Delta u'\Delta v'} &= -\nu_T \left(\frac{\partial \overline{\Delta u}}{\partial y} + \frac{\partial \overline{\Delta v}}{\partial x} \right), \\
\overline{U'\Delta w' + W'\Delta u' + \Delta u'\Delta w'} &= -\nu_T \left(\frac{\partial \overline{\Delta u}}{\partial z} + \frac{\partial \overline{\Delta w}}{\partial x} \right),
\end{aligned}$$

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

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

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.

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 $\kappa^* = 2.83\kappa$, 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-length-scale turbulence closure is discussed.

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.

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?

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).

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?

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.

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.

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).

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.

11. Section 4.3: I lack information on how this test case was performed:
    (a) Which wind turbine was used, NREL-5MW?
    (b) What was the wind direction, 270°?
    (c) What was the wind farm layout (or spacing) for each wind farm case?

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?

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.

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.

Minor comments

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

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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