Review on the manuscript wes-2020-86, entitled "The curled wake model: A three-dimensional and extremely fast steady-state wake solver for wind plant flows", by L.A. Martinez-Tossas et al.

This manuscript deals with further development of the curled wake model by proposing a parabolic solution of the governing equation that allows achieving low computational costs, which is a feature highly sought for wind energy practitioners.

The computational capabilities of the proposed model are highly compelling, yet the description of the model and its assessment can be improved. Furthermore, the overall quality of the manuscript should be improved as well.

My main comments are:

- The authors should state how this model in its core differs from the Ainslie model (Ainslie, J.F., Calculating the flow field in the wake of wind turbines, *J. Wind Eng. Industr. Aerodyn.,* 27, 213-224, 1988). In the Ainslie model, pressure is neglected, the RANS equations are solved parabolically, turbulent stresses are modeled with a mixing-length assumption (actually that model is slightly more complex including a component for ambient turbulence and a component for wake-generated turbulence), in analogy with the proposed model, which is still valuable considering the addition of the velocity perturbation induced by the rotor yaw (Shapiro *et al.* 2018).
- Besides the efforts made by the authors to develop an analytical framework for this model, this model should be considered as a semi-empirical model. The rough approximations used (removing forcing and adding directly the respective velocity perturbations, neglecting the pressure gradients, rough eddy-viscosity modeling) lead to flow predictions not satisfying basic first-principles of fluid dynamics, such as conservation of mass. This is particularly evident if considering null yaw of the wind turbine. If we consider that foundational models, such as the Jensen model, were developed only using the mass conservation, then I think it is reasonable to ask if the accuracy achieved relies only on the "smart" tuning of the mixing length model, which is difficult to generalize (see below more comments on the mixing length model).

These comments should be addressed in a revised version of the manuscript. Below you can find more comments that I hope can help with the preparation of a revised manuscript.

Comments

1. Eq. 1: Where is the forcing of the wind turbine? The turbulent Reynold stresses have the wrong sign. I hope this is only a typo in the manuscript rather than a bug in the code!
2. Eq. 4 is a kind of tricky because: $\overline{a'} = \overline{A'} + \overline{\Delta a'} = 0$, for the Reynolds averaging. Thus, $\overline{A'} = -\overline{\Delta a'}$. Does it make any sense that the mean fluctuations of the background flow are equal and opposite to that of the wake deficit? Please add comments on this, which might help to understand better this modeling strategy.
3. Eq. 5, what is p_w, Δp?

4. For Eq. 5 from Eq. 1, you should state that you are neglecting the molecular viscosity.
5. Line 89, there is no mixing length model in Eq. 9 so far, maybe an eddy viscosity model.
6. L95, Eq 9 is not parabolic, maybe It can be solved parabolically.
7. As I mentioned above in my main comments, I am not sure if it makes sense to build up these equations to then neglect the partial derivatives of the background velocity field, pressure, and proposing to model turbulent fluxes through an "ad-hoc" eddy viscosity model. The author should discuss this in the manuscript.
8. In Eq. 9 for the eddy-viscosity modeling of the turbulent Reynolds stresses, I think you have two options: a) you practically neglect what you wrote in Eq. 7 and you use what you have in Eq. 9 ($\nu_{eff}\left(\frac{\partial^2\overline{\Delta u}}{\partial y^2}+\frac{\partial^2\overline{\Delta u}}{\partial z^2}\right)$) saying that this is an ad-hoc modeling based on the physics, indeed you expect that the main contribution to turbulent fluxes is due to the turbulence connected the wake shear; b) you write all the equations of the turbulent Reynolds stresses with the eddy viscosity assumption and you add the other terms that are missing.
9. Eq 12: I am not sure this specific mixing length model makes sense for several reasons. First, multiplying the mixing length by C means that you have an effective mixing length of $\sqrt{C}l_m$, which can create issues with the model derived from the Monin-Obukhov similarity theory that you have in Eq. 12. I think you should reconsider this approach by adding to the shear-generated turbulence, contributions due to ambient turbulence (atmospheric stability), and wake generated turbulence (Ainslie 1988, Iungo et al. 2018). Furthermore, you should consider rewriting the contraction of the strain-rate tensor including both background flow and wake deficit, and you will find other contributions you are missing in the mixing length model.
10. Eq. 13, you can report the explicit formulation $\overline{\Delta u}=-\frac{2a\overline{U}}{1+2a}$
11. L114, provide a reference for the mixing length in the free atmosphere equal to 15 m.
12. Eqs. 14 and 15, I guess rather than v' and w', they should be $\overline{\Delta v}$ and $\overline{\Delta w}$. Again, more comments on the consequences of replacing the turbine forcing with a velocity perturbation on the momentum and mass budgets might be helpful.
13. Eqs. 14 and 15 seem different from what reported in Martinez-Tossas 2019, please cross-check.
14. Eq. 17. Cross-check the finite-difference scheme, e.g. there is a second-order approximation of the first derivative, so it should be divided by $2\Delta y$ and $2\Delta z$.
15. Eq. 17. Provide the final parabolic equation solved in the code.
16. L 145, forward in time? Maybe forward in the streamwise direction. Please do not mention time to avoid confusion.
17. L152 – how this grid resolution is obtained? Have you done any study on grid sensitivity?
18. Sect. 4.1, How did you set the thrust coefficient of each turbine? Likewise for Sect. 4.2.
19. Sect. 4.3, I recommend providing an assessment at the turbine level, considering the data availability from LES.
20. Fig. 8, add the turbine numbers in the color maps. The LES data might be questionable, considering the difference in power for the turbines at the first row. Is there any specific reason, rather than numerical issues? Is there a better LES/RANS dataset to use for this assessment?
21. L224, you can say parabolic solution of the streamwise momentum equation of the RANS.