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.

We thank the reviewer for the positive feedback. We have addressed all the comments from the reviewer and modified the manuscript accordingly. The responses to the review are marked in blue.

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).
  We thank the reviewer for pointing out this work. The work of Ainslie focuses on a cylindrical form of the equations with a model for one of the components of the Reynolds stress tensor. The wake is also assumed to be axisymmetric and there is no treatment of the wake in yawed conditions. In this work, we emphasize on the curled wake, a new derivation of the Reynolds stress terms and propose a different approach for the turbulence model. There are some similarities between the Ainslie model and the one proposed in this work, and there are also significant differences. We have included this work in the introduction:
  "Ainslie (1988) developed a parabolic solver for an approximation of RANS equations in cylindrical coordinates. They proposed a mixing length eddy viscosity model that has a component from the ambient turbulence and another from the wake added turbulence."

- 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).

We agree that this model is not analytical. We are proposing a hybrid RANS-analytical model that is focused on minimizing computational cost. This model aims to achieve a computational cost as low as the analytical models but solving a simplified form of the streamwise momentum RANS equation. We have identified this as a hybrid RANS-analytical framework in the manuscript. The main purpose behind a RANS-analytical framework is to minimize computational cost. The tuning of the turbulence model is an essential part of minimizing cost. We have expanded the discussion of the turbulence model and included a section in the appendix with the effects of the tuning parameter in the turbulence model.

Introduction:

"This solver uses a hybrid RANS-analytical framework that aims to minimize computational cost."

2.3 Turbulence model

"Future work should investigate Reynolds stress models which are able to resolve the enhanced mixing and turbulence induced by the wind turbines while remaining computationally efficient for the hybrid RANS-analytical framework."

Conclusions:

"The approach uses a hybrid RANS-analytical framework to obtain the wake velocity based on a parabolic equation for the streamwise component of the RANS equations."

Mass conservation is used in the derivation of the model. However, when solving the momentum equation, mass conservation is not strictly enforced. This model solves an approximate form of the streamwise momentum equation. To be able to have a mass conserving approach, we would need to solve the three components of velocity and an equation for pressure which would be elliptic. This would require a full solution of the RANS equations and cannot be used as a fast 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.

Yes, the sign of the Reynolds stresses was wrong. This was just an error in the manuscript, and it has been fixed. The turbulence model used to represent the Reynolds stresses in the code had the correct sign, so there were no bugs associated to this typo. A wrong sign in the viscous term would make the numerical method unstable.

2. Eq. 4 is a kind of tricky because: a#′ = A#′ + Δ((a((′ = 0, for the Reynolds averaging. Thus, A#′ = −(Δ(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.
   In the case of averaging, the fluctuations do average to zero. The Reynolds averaging is applied to all terms in the equation. This means that every term in the equation is zero. The mean fluctuations of the background flow are all zero. All the interactions between fluctuation are taken into account by the Reynolds stress tensor. The discussion of the Reynolds stress tensor and turbulence model have been expanded in the manuscript.

3. Eq. 5, what is $p_!$, Δp?
   Yes, this is a typo and has been fixed in the manuscript.

4. For Eq. 5 from Eq. 1, you should state that you are neglecting the molecular viscosity.
   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."

5. Line 89, there is no mixing length model in Eq. 9 so far, maybe an eddy viscosity model.
   Yes, the turbulent eddy viscosity approach was used to model the Reynolds stress tensor. The following text has been added to the manuscript:
   "The Reynolds stresses are modeled using the turbulent-viscosity hypothesis (Pope 2020) and the streamwise gradient of the wake deficit is neglected."

6. L95, Eq 9 is not parabolic, maybe It can be solved parabolically.
   The assumptions used to derive the curled wake model equation led to a convection-diffusion equation. The information propagates in the streamwise direction. By neglecting the second derivative of the velocity in the streamwise direction ($\frac{\partial^2 \Delta u}{\partial x^2}$) the equation becomes parabolic.

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.

Building the equations is an important step in understanding the flow throughout the wind plant and the effects of the background flow and the wake deficit separately. The eddy viscosity model is necessary to represent unresolved terms in the equations. We have expanded the discussion of the turbulence model choice and have included an appendix showing details of the eddy viscosity model.

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 (ν"## -$$!(Δ" !'&' + $$!)'Δ'!'&'.) 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.

This point indicates that the explanation for the implementation of the eddy viscosity model was not sufficiently clear. The formulation of the eddy viscosity in the current work does indeed implement an ad hoc model that assumes the stress-like terms found in equation 7 can be represented by way of a mixing length model and a factor that accounts for the additional turbulent diffusion introduced by the rotor and the mean gradients in the wake. A more complete formulation would relate of the stress-like terms to the eddy viscosity (or eddy viscosities), which would require some additional information or different assumptions about the nature of the correlations between background flow and wake flow components. Related the point (9) raised below, the goal of the current work is to develop a parabolic model for wind turbine wake flows that can be solved by marching downstream. The mixing length approach used here does not require that any of the local gradients be calculated in order to estimate the eddy viscosity. Instead, the model assumes that the mixing length from the ABL can be used, and the effects of the aggregate additional turbulent diffusion from the wake are represented by the constant correction factor. A statement to this effect has been added to the manuscript:

"The Reynolds stress model used in the present study was selected due to its computational efficiency. Resolving the spatial variations in the eddy viscosity would require the solution of the full RANS momentum equations and additional transport equations for relevant parameters in the selected Reynolds stress model (van der Laan et al., 2015; Iungo et al., 2018). Future work should investigate Reynolds stress models which are able to resolve the enhanced mixing and turbulence induced by the wind turbines while remaining computationally efficient for the hybrid RANS-analytical framework."

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 $\sqrt{c} l_*$, 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.

The implementation of the mixing length model in the current work is taken without additional modifications that would account for wake-added turbulence and shear generated turbulence, as noted by the reviewer. There is obvious value to reevaluating the canonical formulation of the mixing length for the atmospheric boundary layer when considering other sources of turbulence. The work by Ainslie and Iungo et al., attest to that. However, the curled wake model is a parabolic model that fits between the levels of fidelity seen in analytical wake models and RANS modeling. Multiplying the mixing length by a constant coefficient in the current work is a simple way of saying that the aggregate addition of shear-generated and wake-added turbulence is to increase the effective velocity by a fixed amount. The constant C was the value that minimized the difference between the wind plant power predicted with the curled wake wind farm solver and the observed data. That said, the authors are aware that that the physical representation of the wind turbine wake would be improved by a formulation for the mixing length that accounts for local flow gradients. A statement summarizing the intent and limitations of the current approach has been added to the Formulation section. We have also added a derivation of the equations invoking the eddy viscosity hypothesis for both base and wake deficit solution. The conclusions section now includes a statement that points to the development of better mixing length models in future work:

"Future work will consist of 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 code performance improvements to increase speed."

10. Eq. 13, you can report the explicit formulation (Δ((u(= −/_____ $^{+}{}_{0'+}{}^{-}$,

This formulation uses the information from the upstream plane and adds a new velocity. This formulation has been improved in the manuscript to denote the current plane (n) and the upstream plan (n-1).

11. L114, provide a reference for the mixing length in the free atmosphere equal to 15 m.
    The references for the formulation (Blackadar 1962 and Sun 2011) were included at the end of the line in the original submission. We have moved the reference next to where the mixing length is defined.

12. Eqs. 14 and 15, I guess rather than *v'* and *w'*, they should be Δ(((𝑣(and Δ((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.
    Thank you for pointing this out, this has been modified in the manuscript.

13. Eqs. 14 and 15 seem different from what reported in Martinez-Tossas 2019, please crosscheck.
    The new equations are written in continuous form. Also, the V and W formulations were switched in the paper from Martinez-Tossas 2019 and corrected in the new one. In the limit of the number of vortices (*N*) going to infinity, the formulations in Martinez-Tossas 2019 should converge to the continuous form in the manuscript.

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 $2\Delta z$.

    The reviewer is correct. This was a typo in the manuscript and has been fixed.

15. Eq. 17. Provide the final parabolic equation solved in the code.
    We have re-written the equation in its final form. This is now Equation 20 in the latest version of the manuscript.

16. L 145, forward in time? Maybe forward in the streamwise direction. Please do not mention time to avoid confusion.
    We agree that the term 'time' can be confusing. However, that is the name of the numerical method. We have stated that in the manuscript:
    "This numerical equation is discretized using a ``forward-in-time centered-in-space'' method with the stability criteria shown in Equation 20. We note that the model proposed is steady state and there is no time dependency."

17. L152 – how this grid resolution is obtained? Have you done any study on grid sensitivity?
    Yes, we have included a grid resolution study in the Appendix and have added the following text to the manuscript:

    "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.”

18. Sect. 4.1, How did you set the thrust coefficient of each turbine? Likewise for Sect. 4.2.
    The thrust and power coefficients were determined from a lookup table based on the incoming velocity. The discussion on power and thrust coefficients has been expanded and the following sentence was included:

    “The power and thrust coefficients are obtained from a lookup table based on the local velocity $\langle \ubavg + \uwavg \rangle_{\rm n-1}$.”

    The discussion of the effect of yaw angle on power and thrust has also been added to section 2.3.

19. Sect. 4.3, I recommend providing an assessment at the turbine level, considering the data availability from LES.
    We have expanded the section and included turbine-specific plots of power for two of the cases.
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?
    We have adjusted the simulations and expanded the discussion. It takes too much space to name the turbines inside the colormap. We have included the ordering of the turbines in the figure legend. One of the nice features of the model presented is that we can adjust the background solution. We now use the time-averaged LES precursor simulation as the background flow solution. This dataset is good because it has a collection of different conditions. The comparison is now much better after using the background flow from the LES.
21. L224, you can say parabolic solution of the streamwise momentum equation of the RANS.
    Thanks for the suggestion, we have modified the sentence to:
    “The approach uses a hybrid RANS-analytical framework to obtain the wake velocity based on a parabolic solution for the streamwise component of the RANS equations.”