Review of *Theory and Verification of a new 3D RANS Wake Model* by Philip Bradstock and Wolfgang Schlez

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

The authors present a parabolic Reynolds-averaged Navier-Stokes (RANS) model for 3D wind farm flows. The assumptions and limitations are discussed and an example wind farm simulation is presented. I appreciate the effort of making a scientific publication of a commercial wake model. However, the current content of the article is more suited as a technical report. In addition, the title is somewhat misleading because the model is not verified. A model verification of a RANS model would involve a grid refinement study. I also miss a validation of the model. If both a proper verification and validation are added then it has the potential to be published as scientific article in Wind Energy Science. Therefore, I recommend a major revision. In addition, I have listed major and minor comments below that need to be addressed:

Main comments

1. In the abstract you mention: *The WakeBlaster model is verified, calibrated and validated using a large volume of data from multiple onshore and offshore 10 wind farms.* I cannot find a reference to this work and it is also not included in the present work. I would recommend to include the calibration, verification (grid refinement study) and validation in this work. Otherwise, the scientific content is not sufficient for a publication in Wind Energy Science. In addition, please note that the abstract should include a motivation, a short summary of the work, and the main results and conclusions, so it cannot contain conclusions based on previous work.

2. How are the results post processed when the annual energy production is evaluated? For example, do you include a Gaussian filter to represent wind direction uncertainty? (As introduced by Gaumond et al. (2014) and applied for RANS in van der Laan et al. (2015a)).

3. Since the authors are both employees at ProPlanEn, a commercial entity that is selling the presented model, it would make sense to mention this in the Section Competing interests.

4. Can WakeBlaster handle (complex) terrain? If not, I would mention that the model can only used for wind farms in flat terrain and offshore conditions.

5. Pages 2-3, Lines 58-60: I do not understand what you mean by *In order to account for the unsteady terms, it uses eddy viscosity turbulence closure, where the eddy viscosity is calculated from the combined wake and ambient wind speed shear profiles.* I guess you mean turbulent fluctuations instead of unsteady terms. Unsteady terms can be handled by including a transient term, as can be done in Unsteady RANS (URANS).

6. The reference to Abramovich (1963) is not very accessible. In addition, eq. (2) of the article is the boundary layer equation without viscous effects and I do not understand how this equation can be extended to three dimensions because the original boundary layer equation describes a streamwise U and vertical velocity, which in your coordinate system is W not V. I would suggest to start Section 2.1 with the 3D RANS equations including external forces (e.g. wind turbine

thrust force) but without viscous effects:

$$\frac{\partial U_i}{\partial x_i} = 0, \tag{1}$$

$$U_j \frac{\partial U_i}{\partial x_j} = f_i - \frac{1}{\rho}\frac{\partial P}{\partial x_i} + \frac{\partial \overline{u_i' u_j'}}{\partial x_j}$$

where x_i are the Cartesian coordinates, U_i is the mean velocity vector, P is the mean pressure, f_i are the external forces and $\overline{u_i' u_j'}$ are the Reynolds-stresses. The equations in full form can be written as:

$$\frac{\partial U}{\partial x} + \frac{\partial V}{\partial y} + \frac{\partial W}{\partial z} = 0, \tag{2}$$

$$U\frac{\partial U}{\partial x} + V\frac{\partial U}{\partial y} + W\frac{\partial U}{\partial y} = f_1 - \frac{1}{\rho}\frac{\partial P}{\partial x} + \frac{\partial \overline{u'u'}}{\partial x} + \frac{\partial \overline{u'v'}}{\partial y} + \frac{\partial \overline{u'w'}}{\partial z},$$

$$U\frac{\partial V}{\partial x} + V\frac{\partial V}{\partial y} + W\frac{\partial V}{\partial z} = f_2 - \frac{1}{\rho}\frac{\partial P}{\partial y} + \frac{\partial \overline{u'v'}}{\partial x} + \frac{\partial \overline{v'v'}}{\partial y} + \frac{\partial \overline{v'w'}}{\partial z},$$

$$U\frac{\partial W}{\partial x} + V\frac{\partial W}{\partial y} + W\frac{\partial W}{\partial z} = f_3 - \frac{1}{\rho}\frac{\partial P}{\partial z} + \frac{\partial \overline{u'w'}}{\partial x} + \frac{\partial \overline{v'w'}}{\partial y} + \frac{\partial \overline{w'w'}}{\partial z}$$

In order to arrive at eq. (3) of the article, the following additional assumptions are necessary:

(a) The momentum equations for V and W are ignored.

(b) The streamwise pressure gradient is zero: $\frac{\partial P}{\partial x} = 0$.

(c) The streamwise external force is zero: $f_1 = 0$.

(d) The gradient of the normal Reynolds-stress in the streamwise direction is zero: $\frac{\partial \overline{u'u'}}{\partial x} = 0$.

and then we get eq. (3) of the article:

$$U\frac{\partial U}{\partial x} + V\frac{\partial U}{\partial y} + W\frac{\partial U}{\partial y} = \frac{\partial \overline{u'v'}}{\partial y} + \frac{\partial \overline{u'w'}}{\partial z} \tag{3}$$

Applying the Boussinesq approximation for the remaining Reynolds-stresses we obtain:

$$\overline{u'v'} = \nu_T\left(\frac{\partial U}{\partial y} + \frac{\partial V}{\partial x}\right), \qquad \overline{u'w'} = \nu_T\left(\frac{\partial U}{\partial z} + \frac{\partial W}{\partial x}\right) \tag{4}$$

In order to arrive at eq. (5) of the article we need to assume that $\partial V/\partial x$ and $\partial W/\partial x$ are zero, which is not mentioned in the article. The assumptions (a) and (c) are neither mentioned. In addition, if you assume that the eddy-viscosity is a constant, then you are basically solving for a laminar flow because one could replace the eddy-viscosity with a molecular viscosity. In other words, one could ignore the Reynolds-stresses and include the viscous terms in the U-momentum equation in order to arrive at the same equation. The work could be approved a lot if you can quantify the errors made by each assumption or quantify the contribution to the wake of each ignored term, which could be based on an elliptic RANS model, as performed by Iungo et al. (2018).

7. Section 2.3.1: You mention that the near wake stream-wise velocity profile is prescribed for each wind turbine. How do you determine the end of the near wake and how does it vary with the wind turbine thrust coefficient and atmospheric conditions as turbulence intensity and stability? Do I understand correctly that eq. (7) is used to determine the initial magnitude of the centerline wake deficit at a defined downstream location? In addition, eq. (7) is derived from wind tunnel measurements where the turbulence length scales and Reynolds-number are very different from utility scale wind

turbines, so would that mean eq. (7) needs to be recalibrated? Finally, I was wondering why you are not modeling the wind turbine thrust force as an external force in the streamwise momentum equation instead of prescribing a velocity deficit profile in the near wake.

8. Section 2.4: The eddy-viscosity is no longer a constant, as assumed in eq. (5) of the article, which is inconsistent. Please motivate and clarify.

9. There are a number of undefined parameters and constants. What are the values of k (Page 6, line 158), ℓ (eq. 13), ϕ (eq. 14), $\lambda_{\max}$ (eq. 14)?

10. Should k be κ in eq. (14)?

11. Section 2.4.1. I suspect that the eddy-viscosity lag model could be replaced or simplified using a length scale limiter in the form of an f_P function ($\nu_T^* = \nu_T f_P$) that only has one constant to calibrate, see for example van der Laan et al. (2015b) or van der Laan and Andersen (2018).

12. Section 3.1: Please motivate the chosen grid resolution of $D/10$, where D represent the rotor diameter, using a grid refinement study. In addition, a domain height of $3D$ seems very low to me, please show that this domain height does not affect the solution. I normally use $25D$ for 3D elliptic RANS simulations of wind farms. What are the other dimensions of the 3D flow domain? Do you use stretching of cells in order to reduce the total number of cells or is the domain discretized uniformly?

13. Section 3.1: You mention that a flow case of Horns Rev I takes 5 s. (Please briefly introduce the Horns Rev I wind farm here). That would mean an annual energy production calculation of 22 wind speeds and 360 wind directions would take 11 hours on a single CPU. This can be made parallel as you mention (using a few hundred cores). However, WakeBlaster should provide more accurate results compared to an engineering wake model that can calculate the AEP in about 1 s on a single core in order to make sense to run. Therefore, I would suggest to both validate WakeBlaster with wind farm measurements and compare the performance with one or two engineering wake models in the present work.

14. Section 3.2: You could use this wind farm as both a verification (grid study) and validation case. Presenting a show case is not enough for a scientific document.

15. Section 4: You could add that only flat terrain is considered. In addition, I do not agree that meandering of the ambient wind direction is a limitation of the model because its effect on wake mixing can be modeled by either changing the eddy-viscosity or by running several wind direction cases and average them using a Gaussian filter, see for example van der Laan et al. (2015a), wich is based on the work of Gaumond et al. (2014).

16. The conclusions are not based on the results of the present article:

    - *The characteristics of this model show the desired performance balance between speed and realistically achievable accuracy.* The accuracy of the model is not shown because you lack a validation.
    - *The model has been validated against performance data from offshore and onshore wind farms.* This is not performed in the present article.

Minor comments

1. Page 2, Line 45. You write here: *Models of this group are, in principle, also capable of solving the upstream effects of wind turbines.* You are right about the upstream effects, however, it is also the interaction of the wind turbine wakes and wind turbine induction zones, which represents the elliptic nature of these models.

2. My last name is miss-spelled in the corresponding reference (Laan should be van der Laan).

3. Page 1, Line 25: There is a typo in a citation: citetSchlez2009.

4. Section 1: You mention parabolic and elliptic solvers. While I am aware of the meaning of these terms, it would be useful to explain them in order to reach a broader audience. For example, you could mention that a parabolic solver does not need to iterate numerically and information of the flow is only transported with the flow direction, while elliptic solvers have to iterate to solve the equations and information is transported in all directions.

5. Eq. (1): There is an additional plus sign that can be removed.

References

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