Referee's comments to wes-2022-50

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General comments

This paper presents a thorough assessment of a non-linear eddy viscosity model for RANS wind farm simulations. The scope is well-defined and very relevant in the context of CFD models for wind farm flows since there are significant flaws in traditional two-equation turbulence closures with linear eddy viscosity to model wakes.

The literature is fairly reviewed and adequate credit is given to previous research work done on this topic.

The model's equations are described with a great level of detail and seem accurate. A possible source of confusion for the reader may be the terminology used to describe the 2D and 3D EARSM which seems to overlap with the 1D and 3D flow simulations. It should be clarified in Section 2 that both 2D and 3D turbulent closures can be applied to three-dimensional turbulence.

The validation through the three idealized flow cases is quite interesting and the reviewer appreciated the effort devoted to this section, which is useful to strengthen the arguments of the paper.

The results are discussed thoroughly and with commendable intellectual honesty even in the cases where there is not a clear improvement provided by the new model.

The paper shall be accepted with minor revisions according to the reviewer, as indicated in the main and specific comments provided below.

Main comments:

- Explicitly state the additional computational time compared to the other models, even just for the single wake case.
- Provide the boundary conditions used for the wind turbine row simulations in Section 5. This is a tricky setup since the boundaries of the domain are very close to the rotors and no symmetry seems to apply at the upper boundary due to the staggered layout.
- Remove Appendix B; although it may be true that the LES may be able to capture better the wake meandering induced by the larger incoming eddies, the Gaussian convolution method seems weak since it includes all the lateral turbulent fluctuations as wind direction variations. A well-known rule of thumb states that just eddies larger than 2 rotor diameters induce wake meandering, so using σ_v^2 as the total "meandering energy" seems an overestimation. Furthermore, the wake is moved as a rigid body according to equation C3, which would imply a perfectly frozen turbulence, which is not realistic since smaller eddies decay way faster than the full wake length. Considering the limited improvement seen in Fig. C3 and the fundamental questions that this appendix may arise, it is suggested to remove it and mention the possible lack of wake meandering as a cause of the top-hat profiles.

Specific comments

Line 4: please use "alleviate" instead of "aid" when referring to deficiencies.

Lines 29-40, “Linear…applications”: please add a reference to support this statement.

Line 56: Please consider “is chosen in this work” instead of “preferred” to limit the generality of this statement.

Lines 86-87, “This paper…flows”: please add a reference to support this statement.

Lines 97-00, “The more…Ω_{ij}”: this statement is not clear since also the standard model solved the transport equations of κ and ϵ.

Line 124: Please define “RDT”.

Line 200: was “abandoned” intended instead of “abounded”?

Line 201: the formulation of the production cannot be derived readily, please provide derivation (in appendix) or reference.

Lines 246-249, “In principle…code”: this statement is unclear and unnecessary. Please clarify or remove.

Line 300: The fact that the log law profiles hold for $z < 0.3L_z$ agrees with [Pope S., Turbulent Flows, 2000], which could be cited.

Lines 361-376: At the end of this discussion, it is not clear which values are utilized for the simulations. Please state the value of the turning constants clearly.

Line 504: the Boussinesq’s closure leads to a Reynolds stress tensor that is inherently isotropic only for the diagonal terms $\overline{u_i u_i} = \frac{2}{3}k$, but not for the off-diagonal (shear stress) terms, which can produce different eigenvalues. It is recommended to remove “by definition of the Boussinesq’s closure”.

Line 554: Please clarify “behave sensible”.

Appendix seems not to be cited in the manuscript.

Line 591: Does the expression “rolling average” mean that the RASN profiles are “convoluted” using the pdf of the wake centers in Fig. C2?