Peer review: wes-2021-21

General comments:

The manuscript presents a novel analytical wake model that is compared to aerial measurement of a utility-scale wind turbine wake. The reviewer is firmly convinced that such work must be rejected due to the poor scientific content, negligible relevance and confusing presentation. The analytical treatment and the supporting arguments are so rife with flaws that even major reviews would unlikely bring the quality of this study up to the standard of the Journal. Since this paper is already a re-submission of a work done a year before and rejected, a further re-submission is strongly discouraged.

Firstly, the introduction focuses mainly on Bastankhah and Frandsen model and claims that no efforts have been made to develop expression to relate the atmospheric turbulence to wake characteristics, which neglects all the scientific efforts in this sense done by several researchers in the past 5 years (see references).

The derivation of the simplified momentum equation (Equation (16)), which is simply the streamwise momentum equation at the wake center and for an axisymmetric flow, is unnecessarily convoluted. It switches between conservative and non-conservative form of the Navier-Stokes and is not rigorous. The turbulence closure (Equation (17)) is resemblant to a constant eddy viscosity model, but this is not mentioned. The "approximate" solution in page 7 is evidently wrong, as it treats the residual velocity sometimes as a constant (a behavior wrongfully ascribed to Taylor-frozen hypothesis) and sometimes as a variable when it is solved as a 2nd order polynomial.

The experimental part is very concise and the characterization (especially stability) of the inflow not thorough. The several ad-hoc adjustments done to C and α make unclear the generality of the results. The calibration of Bastankhah model is also not explained sufficiently.

The following specific comments are given just for the records, as the reviewer is not willing to further review the present work.

Specific comments:

Page 1, Line 18 (and throughout the paper): “wind deficit” should be changed to “velocity deficit”.

Page 2, Line 6: Works such as Carbajo Fuertes et al., 2018 and Zhan et al., 2019 should be added mentioning the influence of ABL stability and turbulence on the wakes. Also, avoid repeating the same citations at Page 2, Line 16.

Page 2, Line 9: Please clarify “A key parameter is to resolve the wind deficit as a main driver of shear stress in the wake.”

Page 2, Line 14: Remove “additional”, since also Fransden model uses a wake expansion coefficient.

Page 2: Line 21: Although it is true that the original Bastankhah model did not explicitly relate the wake expansion coefficient to the inflow, several later studies provide empirical expressions for k as function of turbulence intensity or stability (e.g. Niayifar and Portè-Agel, 2018; Carbajo Fuertes et al., 2018; Cheng et al., 2019; Zhan et al., 2020). This point must be mentioned and the additional improvements of the presented approach compared to the cited works discussed in the introduction.

Page 2, Line 29: Better clarify or remove “based on some assumptions”.

Page 2, Line 31: The expression “atmosphere’s point of view” is not clear.

Page 2, Lines 31-33: As already mentioned, there are several examples of wake model that take into account turbulence intensity and atmospheric stability through empirical correlations. Remove the statement and discuss the impact of this work more thoroughly and expand the literature review.

Page 3, Lines 9-10: Clarify “in-situ measurements have the advantage that the measured wind is already a superimposition of turbulence created by the WEC and the free-stream turbulence”.

Page 3, Line 17: Please define “residual velocity”.

Page 3, Lines-21: Equation (2) could not be found directly in the Frandsen et al., 2006, which just states the K is $\sim 10k_{Jensen}$. Also, such value is valid for small Ct whereas here the optimal Ct is assumed.

Page 3, Line 13: The assumption of one-dimensional flow is not clear, since the cross stream component are retained in the following.

Page 3, Line 16: the pressure close to a turbine varies not only due to meso-scale gradients but also because of the thrust. Neglecting pressure terms in the near wake does not ensure continuity. The equations become parabolic if the pressure is removed, which is not mentioned.

Pages 3-4: Equations (8) to (11) can be simply omitted by stating that the conservative form of the momentum equation is used.

Page 5, Line 6: The partial differential equation for u is here "localized" by applying it to the wake centerline. This is confusing, since if u_r is a function of but the derivative in y and z are still present.

Page 5, Line 22: The sentence "Coming now.." is incomplete.

Pages 5-6: The streamwise diffusion of momentum is more correctly neglected in comparison with the transversal terms in the so-called boundary layer approximation (e.g. Iungo et al. 2017).

Page 6, Equation (18): This turbulent closure is simply a discretized form of a constant eddy viscosity applied to the Reynolds stresses at the wake centerline and it must be stated.

Page 7, Equation (19): This integration appears wrong in this point of the manuscript, the assumption of $u_r = const.$ must be introduced first. However, having a constant flux of momentum $\alpha(u_0 - u_r)$ as a function of x is unrealistic and lead to velocities higher than the freestream

Page 7, Equation (20): The homogeneous equation would be the one without the constant term on the RHS of Equation (19), not this one. Also, the necessity of such distinction is unclear.

Page 7, after Line 5 (the line numbering is wrong): The Taylor frozen hypothesis deals with the velocity fluctuation, not the mean flow which is here analyzed.

Page 8, Line 4: Betz equation in optimal Cp conditions is $u_r = \frac{1}{3}u_0$ and it applies after the pressure recovery region, not in $x = 0$.

Page 7, Equation (20.3): This solution is mathematically outrageous since it is obtained from 20.1 which assumed u_r as constant but then uses the solution of a 2^{nd} order polynomial where u_r is now a variable.

Page 8, Line 8: "drain turbulence" is not a scientific definition.

Page 8, Lines 15-20: This description of the turbulent flow of a turbine wake is very poor. The turbulent region in the wake should expand due to diffusion and not shrink as hypothesized.

Page 12, Line 7: the "resemblance" with stably stratified profile is not proven.

Page 12, Lines 13-15: the extremely low roughness cannot be justified just by the proximity with the sea which is 2 Km away. If an internal boundary layer is developing it should be clearly stated and its growth rate estimated.

Page 14, Equation (29): this estimation method for the growth rate is not clear.

Page 15, Lines 9-12: the arbitrary reduction of C is very questionable.

Page 16, Lines 5-9: this justification of the failure of the model outside of the near wake is not supported by enough evidence.

References

Carbajo Fuertes, F., Markfort, C.D. and Porté-Agel, F., 2018. Wind turbine wake characterization with nacelle-mounted wind lidars for analytical wake model validation. *Remote sensing*, 10(5), p.668.

Cheng, Y., Zhang, M., Zhang, Z. and Xu, J., 2019. A new analytical model for wind turbine wakes based on Monin-Obukhov similarity theory. *Applied Energy*, 239, pp.96-106.

Iungo, G.V., Santhanagopalan, V., Ciri, U., Viola, F., Zhan, L., Rotea, M.A. and Leonardi, S., 2018. Parabolic RANS solver for low-computational-cost simulations of wind turbine wakes. *Wind Energy*, 21(3), pp.184-197.

Niayifar, A. and Porté-Agel, F., 2016. Analytical modeling of wind farms: A new approach for power prediction. *Energies*, 9(9), p.741.

Zhan, L., Letizia, S. and Valerio Iungo, G., 2020. LiDAR measurements for an onshore wind farm: Wake variability for different incoming wind speeds and atmospheric stability regimes. *Wind Energy*, 23(3), pp.501-527.