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Interactive comment

Interactive comment on "An analytical solution for wind deficit decay behind a wind energy converter using momentum flux conservation validated by UAS data" *by* Moritz Mauz et al.

Anonymous Referee #1

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The authors present an equation to reproduce the wind deficit behind a wind turbine, and compare their results with unmanned aerial system (UAS) observations.

I appreciate the general strategy of trying to establish a wake thickness description with a model based on momentum equation and checked against UAS observations. However I do not find the paper really convincing neither in the analytical modelling nor in the observational section. The observations presented in the paper are restricted to a few values of the mean wind measured by the UAS at several distances from the rotor. Furthermore, some key parameters, such as the friction velocity used by the authors in their parameterization, are not computed from observations (or retrieved from simulations with a weather model), but arbitrarily fixed to a value of 0.3 m/s, supposed representative of a wide range of meteorological conditions. The mathematical developments are confusing, and, in my opinion, wrong in some parts. I find it hard to believe that all of the coauthors have carefully examined this manuscript before its submission to WES.

There is abundant literature on turbine wake observation and modeling (the authors mention the comprehensive review paper by Porté-Agel et al., which appeared this year in Boundary-Layer Meteorology). It is therefore crucial that any new article clearly explains what is brought with respect to the existing knowledge. To summarize, the present manuscript needs a lot of work, on both form and content, before becoming acceptable in the Journal.

Major comments:

1. I do not find any interest in the Euler method to solve the model equation. If there exist an analytical solution, why playing with approximate, numerical solving? This adds confusion.

2. A lot of analytical models describing the wind deficit behind a rotor are already available in the literature. The authors do not explain why there is a necessity for a new one, what is the improvement brought by their model, how it compares with the existing ones, etc.

3. The hypotheses used in the equations lead to a mathematical impasse: it is assumed that the transversal and vertical wind components are zero (<v>=<w>=0; I use brackets here instead of overbars for easy writing). We thus have d<v>/dy= d<w>/dz=0, and to satisfy incompressibility in mass conservation equation, we therefore get d<u>/dx=0!. Furthermore, the authors come to the relation <u>d<u>/dx=d<u2>/dx (equations (2) and (4)), in contradiction to the mathematical relation d<u2>/dx = 2<u>d<u>/dx.

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4. The authors mix partial derivative equations and bulk approximations with finite differences (e.g. equation (5)). If an analytical solution is to be found, then the mathematical developments have to be conducted with the derivative forms (i.e. not approximate) of the equations.

5. The manuscript reveals weaknesses in the knowledge of boundary-layer meteorology. It is mentioned that the model is applied above the surface layer (d<u>/dz=0 from the hub height), but the equation used to compute the eddy-diffusivity (eq. 6) is valid in the surface layer (and for neutral conditions). A sentence such as “Regarding the temperature profile, wind conditions and turbulence intensity (s.a Sec. 3.3), a stability parameter ÏŻ = z/L of approximately 0 to 1 can be concluded, using Businger et al. (1971).” is really annoying, because estimating the stability requires the knowledge of friction velocity and buoyancy flux, and none of these two parameters was measured during the observation periods.

6. The results presented in Figs. 7 to 10 should be grouped in a single graph (note that the observations presented in these 4 figures are the same). The curves relative to Euler’s solutions should be omitted, as well as the model curves which are not relevant (α=constant for distances to the turbine larger than two diameters).

Specific comments and technical errors:

1. There is no mention of the turbine parameters (e.g. the thrust coefficient), though some wake models involve such parameters in their equations. This should be commented.

2. P. 2, L. 19-21: Is there any specific interest to mention this study in regard to numerous other observations done in the wake of wind turbines?

3. P. 3, eq. (2): in the rhs, uj and ui should be overlined separately (i.e., with bracket notation, <uj><ui> instead of <ujui>).

4. P. 3, L. 10-12: If there is no pressure gradient, then there is no geostrophic wind,

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and in non-perturbed conditions the wind comes to zero. A geostrophic balance (compensation between pressure and Coriolis forces) should instead be invoked here.

5. P. 3, lines 19-20: It should be explained why subsidence might be neglected in unstable and neutral conditions (implying a different behaviour in stable conditions?).

6. Fig. 1: The wind profile represents the conditions ahead of the wind turbine. A second profile representative of the wake conditions should be added.

7. To avoid confusion, I suggest to replace z (hub height) and Δz with, e.g. h and Δh.

8. P. 4, L. 11: Typo "von Karman".

9. P. 4, last line: "Joffre et al. (2001) studied the variability of the stable and unstable atmospheric boundary layer height and documented a dependence of the shear stress velocity u* on the stability parameter ÏŻ." The main driver of u* is the wind. Furthermore, u* is one of the two parameters used to define the stability (and not the other way).

10. P. 5, L. 2: "Slight differences in u* solely shift the solution along the y axis". Please explain. What is "the y-axis"?

11. P. 5, L. 3: Unclear for me.

12. P. 5, L. 4: Typo "reduced".

13. P. 5, eq. 7: Please define what αrepresents.

14. P. 5: "Equation 7 is a non-homogeneous non-linear differential equation (DE) of first order". As it stands, Eq. 7 is not a differential equation.

15. P. 5, eq. 8.2: Please define what the superscript hom represents.

16. P. 5: The paragraph "A short assessment ... convenient to solve." Is unclear. Please rephrase.

17. P. 6, eq. 9 and 10: It is not useful to write two equations here.

18. P. 6, L. 24: “the frequency α“. Why is α called a “frequency”?

19. P. 6, eq. 11: Is there any justification (e.g. a reference) for this equation?

20. P. 6, eq. 11: There is no need to introduce a new symbol (R), since D=2R. Please rewrite as a function of D.

21. P. 7, L. 10: typo “... the this method”.

22. Fig. 3: Please define clearly which parameter is represented here.

23. P. 10, L. 3: UAS should be defined at its first appearance (p. 2).

24. P. 10, L. 5: What are the heights of the legs?

25. P. 10, L. 11: Please explain how the turbulence intensity is computed (turbulence observations are not mentioned in the manuscript).

26. P. 10, L. 19: Typo “parameters”.

27. Fig. 5: Please add a scale and indicate the geographical orientation. We can observe close to the right border of this image the shadow of a second wind converter. Is there a potential impact of this 2nd converter on the wake of the 1st one?

28. Fig. 6: There are negative altitudes. Please explain.

29. Fig. 6: Please explain why temperature data are discarded during UAS turns. Is that because the measures are biased, or because turns are too far away from the profile location?

30. Fig. 7, caption: replace “At about 2.5 D...” with “From about 2.5 D...”

31. Section 4.3: There are no observations in the far wake area. So, the model performance cannot be evaluated. Why do not try to test the model against another data set?

32. Section 4.3: The sentence “While the constant-α model underestimates the wake

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behaviour the dynamic-α approach follows the measured data up to 5 D and paints a reasonable picture of the wind deficit decay." is not relevant for this section.

33. Figs. 7 to 10: The parameter represented is not the "wind deficit".

34. P. 14, L. 3-5: I do not understand what is meant here. Please rephrase.

35. P. 15, L. 4: "0.45 m/s".

36. P. 15, L. 1 to 5: This is surprising: it is known that the greater the turbulence level, the shorter the wind recovery distance in the wake. Furthermore, if u* is a key parameter in the eddy-diffusivity value, then enhancing or lowering it by 50% should significantly modify the wake characteristics.

37. Fig. 10: The curve corresponding to the analytical model here is not identical to that presented in Fig. 8. For example, at a distance of a little less than 5D, the model crosses the blue disk of the observations in Fig. 10, whereas it remains well below in Fig. 8. Please explain.

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