Author's response to Referee #1

September 8, 2020

Thank you for the detailed review of the manuscript. In the following I will comment on each point. The referee's comments will be repeated in blue italic before the answer. We will adopt the enumeration format from the original referee's comment list.

Summary and general comment

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 parametrisation, 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 co-authors have carefully examined this manuscript before its submission to WES.
There is abundant literature on turbine wake observation and modelling (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.
Thank you for sharing your general thoughts. Below we answer to all the raised issues.

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.*
   In this manuscript the Euler method is intended as validation tool for the simplification done in Eq. 7 (or from Eq. 8.1 to 8.3). The differential equation (or to be more precise: difference equation) is a non-homogeneous DE. When integrating Eq. 8.1 we treat u_r independent of x which is not really correct. But it makes the solution much easier treating u_r constant over (an infinitesimal increment) dx . The quadratic character of the solution is kept, as can be seen in the resulting Eq. 8.3. Also, presenting a rather simple numerical method that solved the equation that can describe a wind turbine wake, can be motivation for an implementation in any numerical solver.

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.*
   Thank you. This is fair point that we will address in the new version of the manuscript.
   The authors of the manuscript are all affiliated with atmospheric (in-situ) measurements/science. In-situ measurements of the near wake are extremely rare in the scientific community and often overlooked (e.g. the comprehensive paper of Porté-Agel et al. (2020)). In the earliest evaluation period of the data, the in-situ measurements were plotted against models from Bastankhah and

Porté-Agel (2014, 2017) and Emeis (2010). Given that the Emeis model (E10) is intended for wind parks the idea was to modify the approach to fit a single wind turbine deficit. The analytical solution based on the model from Bastankhah and Porté-Agel (2014, 2017) did not fit to the measurements. The data could fit in the near wake or in the mid wake. But never over the whole measured wake region (0.5 - 5 D). We concluded that the simplifications made by Bastankhah and the origin (thrust based derivation) did not allow for a fit to real world data, e.g. neglecting tip vortex helix structures. Yet, we do not want to take anything away from thrust based models.
We will adjust the introduction to tell the reader which concerns lead to the alternative approach of a new analytical model.

3. *The hypotheses used in the equations lead to a mathematical impasse: it is assumed that the transversal and vertical wind components are zero ($\overline{v} = \overline{w} = 0$. We thus have $d\overline{v}/dy = d\overline{w}/dz = 0$, and to satisfy incompressibility in mass conservation equation, we therefore get $d\overline{u}/dx = 0$!. Furthermore, the authors come to the relation $\overline{u}d\overline{u}/dx = d\overline{u^2}/dx$ (equations (2) and (4)), in contradiction to the mathematical relation $d\overline{u^2}/dx = 2\overline{u}d\overline{u}/dx$.*

   We have reworked this part of the manuscript. The main focus here shall be to condense the Navier-Stokes (N-S) equations to get to the momentum conservation equation in a steady-state incompressible flow (Eq. 1).
   The presented N-S equation is already the divergence-free version. So we are very thankful to point out this error. We have now updated the steps to boil the N-S down to the momentum conservation equation for a steady-state incompressible flow.

   The full derivation can be found in the new iteration of the manuscript. We believe that we have connected the N-S equation to the resulting Eqs. 8,11,12 in the manuscript successfully.

$$\underbrace{\frac{\partial(u_r \cdot u_r)}{\partial x}}_{A} + \underbrace{\frac{\partial(\overline{v} \cdot u_r)}{\partial y}}_{B} + \underbrace{\frac{\partial(\overline{w} \cdot u_r)}{\partial z}}_{C} + \underbrace{\frac{\partial(\overline{u'u'})}{\partial x}}_{D} + \underbrace{\frac{\partial(\overline{u'v'})}{\partial y}}_{E} + \underbrace{\frac{\partial(\overline{u'w'})}{\partial z}}_{F} = 0 \quad (1)$$

4. *The authors mix partial derivative equations and bulk approximations with finite differences (e.g. Eq. 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.*
   We know this is not hundred percent accurate. From our understanding the final analytical solution is derived from the governing NS equation. From thereon we have to do some simplification using real world order of magnitudes (e.g. Δz, or the Gradient approach in Eq. 5). We then stick to differences assuming a continuously differentiable solution/equation. We will mention this issue in the paragraph before continuing with Eq. 7.

5. *The manuscript reveals weaknesses in the knowledge of boundary-layer meteorology. It is mentioned that the model is applied above the surface layer ($d\overline{u}/dz = 0$ 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 $\zeta = z/L$ 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.*
   Equation 6 is indeed valid in the surface layer. From our understanding, up to the inversion height z_i. We (have to) assume the hub height of the wind turbine to be still in the surface layer, in order to be able to use this equation. As in Fig. 1 implied, we assume that $du/dz \approx 0$ not $= 0$. We are aware that we stretch the validity of some of the equation. It would have been really great to have had an EC (eddy covariance) station at the measurement site at the field campaign. But then, measurement campaigns are always more complex as people might assume. We have to work with what we got at the moment. And as mentioned in the discussion, if we would have known the outcome of the data evaluation, we would have installed an EC station or would have implemented a measurement path for the UAS to measure the horizontal Reynolds shear stress on site at a decent altitude. But wind parks (off- or on-shore) are never ideal to measure undisturbed flow. Hence, we use this method to at least somehow assume a u_* value, for a near neutral thermal stratified lower

atmosphere, using the vertical profile of the virtual potential temperature directly in front of the wind turbine.
And please also consider that the mathematics or the validity of the derivation would not change by using a 'true' value for the shear velocity.

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 ($\alpha = constant$ for distances to the turbine larger than two diameters).*
Thank you. We can see, how the four figures really might be unnecessary. This is work flow related. As the manuscript evolved, the far wake behaviour of the model got interesting. The initial intention was to have graphs that depict the near wake behaviour and an overview ranging all the way up to the far wake of the wind turbine (although we have no UAS data). Yet, we wanted to see, if the model depicts a realistic wake length (what it does). But we admit that the near wake behaviour is still reasonably well shown in the far wake graphs. Concerning the suggestion to combine all graphs into one plot, we do not want to do so, since we think that the graphs will look cluttered, especially since we use the Euler method as validation for the analytical derivation of the model and need to keep it in the graphs. We oppose your suggestion to remove the Euler method completely (from the graphs and from the manuscript).
We also do not want to remove the $\alpha = constant$ line for distances larger than 2 D, because this is the essence of the graph. The result is that the $\alpha = constant$ line does not match the measured data. Removing this line would be contrary to the graph's intended statement.

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.*
We have trouble to assign this comment to a specific occasion. We mention the models of Magnusson and Smedman (1999) and Bastankhah and Porté-Agel (2014) and that they use the thrust coefficient C_T. Maybe you mean that we do not mention turbine parameters in our model. We use the diameter of the wind turbine, but this is mentioned throughout the manuscript.
It is a unique selling point to contrast with existing models and should be more stressed in the introduction.

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?*
We aim for an overview of all methods that are used to measure wind turbine wakes and wind deficits. Hence, we want to mention that there are also approaches using remote sensing methods.

3. *P. 3, eq. (2): in the rhs, uj and ui should be overlined separately (i.e., with bracket notation, $\overline{u_j}\ \overline{u_i}$ instead of $\overline{u_j u_i}$).*
It is actually the case, but the 'overline' command is used instead of the 'bar' command. Each with their own problems. We have addressed the issue and added a spacer.

4. *P. 3, L. 10-12: If there is no pressure gradient, then there is no geostrophic wind,and in non-perturbed conditions the wind comes to zero. A geostrophic balance (compensation between pressure and Coriolis forces) should instead be invoked here.*
This might be a misunderstanding. We assume no *changes* in the pressure distribution along the wake. Meaning no change of the pressure gradient. We will make this clear in the next iteration of the manuscript.

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?).*
Buoancy-driven flows need a variation in density (and a gravity field). In neutral conditions the BL can be assumed well mixed. In stable condition a WEC is mixing air over an altitude of up to 200 m, e.g. in modern off-shore wind parks. Here, the artificial and forced mixing of the air

can lead to subsidence. Especially, when the hub height of the wind turbine is somewhere near the inversion height z_i, e.g. during diurnal transition in a MBL.
We will add a short explanation.

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.*
This is a good idea. We have added the additional wind profile.

7. *To avoid confusion, I suggest to replace z (hub height) and Δz with, e.g. h and Δh.*
This is true. We can see the confusion. At the moment something like $z \rightarrow z + \Delta z$ is in place. We will implement a more comprehensive labelling of the heights in the new iteration of the manuscript.

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

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 ζ." 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).*
Fair enough. We could change it up. However, this description is directly a citation of Fig. 2 in Joffre et al. (2001).

10. *10. P. 5, L. 2: "Slight differences in u_* solely shift the solution along the y axis". Please explain. What is "the y-axis"?*
The sentence has been removed. The sensitivity towards a change of the shear velocity u_* will be addressed more comprehensively in the new manuscript.

11. *P. 5, L. 3: Unclear for me.*
Please be more specific, so we can clear out the issue. We assume you might mean the need for the parametrization, so we added a short explanation.

12. *P. 5, L. 4: Typo "reduced".*
Fixed. We have also moved the sentence up directly under Eq. 5 were Δu appears for the first time.

13. *P. 5, eq. 7: Please define what α represents.*
α is the wind deficit decay rate. We will add a mention.

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.*
We changed the sentence to be more exact.

15. *P. 5, eq. 8.2: Please define what the superscript 'hom' represents.*
It stands for 'homogeneous'. The issue has been cleared out.

16. *P. 5: The paragraph "A short assessment ... convenient to solve." Is unclear. Please rephrase.*
The paragraph has been restructured and rephrased.

17. *P. 6, eq. 9 and 10: It is not useful to write two equations here.*
We have omitted the former Eq. 10 and added some explanation.

18. *P. 6, L. 24: "the frequency α". Why is α called a "frequency"?*
Since it sounds a bit odd, we changed 'frequency' to 'wind deficit decay rate'.

19. *P. 6, eq. 11: Is there any justification (e.g. a reference) for this equation?*
In this study we use this hyperbolic function as a first approach. Different functions (quadratic $(1/x^2)$ or any potential law function) were used, and the one in the submitted manuscript worked best. The justification behind it, is the idea that the function should somewhat represent a non-linear decay.

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.*
The equation has been adjusted.

21. *P. 7, L. 10: typo "... the this method".*
Thanks. It has been fixed.

22. *Fig. 3: Please define clearly which parameter is represented here.*
The caption of Fig. 3 has been rephrased.

23. *P. 10, L. 3: UAS should be defined at its first appearance (p. 2).*
You are correct. No argument here.

24. *P. 10, L. 5: What are the heights of the legs?*
All data were captured at hub height. The information has been added. Additionally the information is now also given in the caption of Fig. 5.

25. *P. 10, L. 11: Please explain how the turbulence intensity is computed (turbulence observations are not mentioned in the manuscript).*
This information is from the vertical profile flown in the inflow of the WEC. It is computed by calculating $TI = u'_{hor}/\overline{u_{hor}}$. With u_{hor} the horizontal wind. The averaging window is derived from the computed integral length scale. The information has been added to the manuscript.

26. *P. 10, L. 19: Typo "parameters".*
Thank you. Fixed.

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?*
We will add a new map for Fig. 5. The WEC south of the E-112 converter did not interfere with the measurements. Due to the wind conditions the UAS needed extra space for a wider turning manoeuvre.

28. *There are negative altitudes. Please explain.*
The altitude is actually m a.s.l not a.g.l, and the issue has been addressed.

29. *Fig. 6: Please explain why temperature data are discarded during UAS turns. Is that because the measurements are biased, or because turns are too far away from the profile location?*
Thank you, that is an interesting topic in itself. The data needs to be discarded, because of the internal boundary layer that touches the temperature sensor. Thus, the flow conditions are no longer according to the specifications of the temperature measurements. The flow temperature is then highly impacted by the temperature of the fuselage of the UAS.
We have also added a short explanation in the caption of Fig. 6.

30. *Fig. 7, caption: replace "At about 2.5 D..." with "From about 2.5 D..."*
Fixed.

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?*
As you might have noticed there is a far wake model behaviour analysis later in the manuscript. At the measurement campaign we had a tight schedule, since we have to perform several measurement flights for different wake evaluations, amongst them also measurements for the partners of the HeliOW project.
We are planning to measure at a similar data set behind a E-82 WEC in the south of Germany. We have also applied the model to wake data from off-shore wind parks, where it performs very well.

32. *Section 4.3: The sentence "While the constant-α model underestimates the wake 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.*
You are correct. We will move the evaluating part of the statement to the discussion section.

33. *Figs. 7 to 10: The parameter represented is not the "wind deficit".*
Strictly spoken it is the normalised reduced (or residual) horizontal wind speed in the wake. We choose the remaining wind velocity to represent the wind deficit in the wake. To prevent confusion we will call it as such. It is much more convenient to work and calculate with what is left than what is missing in wind velocity.

34. *P. 14, L. 3-5: I do not understand what is meant here. Please rephrase.*
Fair point. We have stretched the paragraph and explained the matter more thoroughly.

35. *P. 15, L. 4: "0.45 m/s".*
Thanks. It has been fixed. The paragraph has been altered altogether.

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.*
True. The paragraph is outdated. It refers to a first evaluation that only considered up to 5 D, and no wake lengths at all. In an up-to-date evaluation it could be shown that the wake length differs ± 3 D for the assumed variations in u_* of $\pm 50\%$. Thus, this paragraph has been rewritten.

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.*
We have addressed the issue. The wrong plot/pdf was copied into the tex folder. The plot was from an early calculation where the reference height ($z = h + \Delta z$) was wrongly implemented (Δz was missing).

References

Bastankhah, M. and Porté-Agel, F. A new analytical model for wind-turbine wakes. *Renewable Energy*, 70:116 – 123, 2014. ISSN 0960-1481. doi: https://doi.org/10.1016/j.renene.2014.01.002. Special issue on aerodynamics of offshore wind energy systems and wakes.

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