# Author's response to Referee #1

June 4, 2021

Thank you for the detailed review of the manuscript. In the following we will comment on each point. The referee's comments will be repeated in blue italic before the answer. We will carry over and enumerate the referee's comments from the supplement pdf-file.

## Summary and general comment

*The paper sets out to develop an analytical far-wake model and compares model results to UAS obtained wake measurements. The topic is interesting and so is the measurement technique employed, unfortunately though, the paper should be rejected for a number of reasons. Firstly, there are issues with the most important part of the paper, namely the wake model description and development:*

- *Inconsistent and incomplete description of the analytical wake model*
- *Underlying model assumptions and their significance are not explained*
- *The reasoning for certain model choices remain unanswered and cannot be followed*
- *None of the model subcomponents are verified against existing datasets of which there exist many*
- *The model is only valid below rated wind speed*

*Unfortunately, the described wake model is not thoroughly derived nor is its derivation sufficiently innovative with respect to existing models to justify the flaws in the model development and description to be overlooked or fixed in a major revision. An underlying issue is that the need for such a model remains unclear, as it is not an improvement over the status quo. Important recent developments are not mentioned in the introduction and have been missed. Secondly the comparison with measurement data is flawed.*

- *The measurement campaign is not sufficiently described, the only information consists of the three flight paths taken by the UAS and its sensory equipment. The measurement period and number of data points at each position in the wake is not given. Were there really just three flights?*
- *As there were only 3 flights, the dataset is not statistically significant. The authors do not comment on this issue and s it also seems bizarre to compare instantaneous data (slightly time-space filtered as they employ a time filter but the plane is in-stationary) to analytical models that are derived under steady-state assumptions, including their own. The authors miss that the wake spreading coefficients of the other analytical models (including the one they use for their model) are only valid for time-averaged solutions. One can use these models anyhow and retune the wake spreading coefficient for instantaneous wake computations as well, however it did not seem as this was the ambition of the authors nor do they seem to be aware of this issue.*

*The authors are advised to restructure and rewrite the model development and use some of the existing, already published wake measurement datasets and existing models available to verify and validate their model. However, this would entail writing a paper from scratch. This paper should be rejected. Attached are some more detailed comments to the authors on some essential issues.*

Thank you for sharing your general thoughts. We want to use the possibility and answer to your general comments quickly. Down below, your specific comments are listed individually and we answer to all the raised issues in detail.

We took a lot of effort in describing every step of the derivation of the analytical model and the underlying

assumptions. The reader may also just estimate each term, order of magnitudes etc. for him- or herself by all the equations provided. Also, we are not aware of in-situ wake measurement datasets available. We also reason that wind tunnel measurements do not represent real world scenario sufficiently. Especially in the near wake.
The point that the model may only be valid below rated conditions is simply not true. The E-112 WEC (wind energy converter) has a rated wind speed of $\approx 13$ m s$^{-1}$. These wind conditions were met at the measurement site (SCADA data). Also the pitch angle of 1° indicates that the WEC was already meeting rated conditions and reacted by changing its pitch angle.

## Specific comments and technical errors

1. *p. 2 l. 21: This is true, but from the same group a lot of modifications were added since 2017 that account for the influence of TI. k is then dependent on TI and added TI can be calculated with an added TI model. Refer to Niayifar 2016 for instance.*
   We will look into it and add this reference.

2. *p. 3 l. 18: velocity in the wake is a more descriptive name. just to avoid confusion it would be good to add $u_r = u_0 - du$ where du is the deficit*
   Thank you. Good point. We will add it to the text.

3. *p. 3 l. 27: The exponential decay in the lateral directions is missing here. Either state that this is the deficit at the wake centre for the Bastankhah model or add the missing term. After all the exponential decay term was the novelty in their work.*
   We are aware of the lateral exponential term of the model. For the wake centre line this part is irrelevant. We will mention it in the new manuscript.

4. *p. 4 l. 14f: The pressure term is non-zero in the near-wake. However, it needs to be mentioned why in your case this does not matter (i.e. far-wake pressure recovery ...). A discussion/argument about the steady-state assumption would also be welcome.*
   Indeed, it does not matter in our case since we want to regard the whole wake and therefore, neglect the decline in pressure along $1 - 3$ D. This might add a minor error in the first 3 D but simplifies the equations a lot.

5. *p. 5 l. 6: You are implicitly assuming a coordinate system with x along the wake centre line. Already in section 2.1 you are assuming a coordinate system that is unknown to the reader. State your coordinate system at the start of section 2 in some form.*
   Thank you. This information got lost in the rework probably. We will add it immediately.

6. *p. 5 l. 24: Please give a reference and/or explain why this is the case. Just because something is "commonly done" it does not necessarily need to be correct. An underlying assumption is also that the radial turbulent fluxes dominate. Is this true?*
   Thank you. As you have also mentioned, the radial turbulent fluxes are much higher than the longitudinal one. This explanation can also be used to neglect term D in Eq. 15.

7. *p. 6 l. 6: Following your argument it should be the radial momentum flux, as you assume vertical and lateral fluxes to be the same.*
   True. But as shown by Emeis (2010) and others, the vertical momentum in-flux is mainly responsible for the wind deficit decay. One can also assume a simplified squared wake instead of a circular one to see this point.

8. *p. 6 l. 14: Please explain this step (introduction of $K_{\mathrm{m}}$) in more detail and its validity in this context. After all it is a crucial step in the development of your model. You need to argue for why this assumption is justified.*
   True. We also have a whole sub-section (2.3)dedicated to the momentum transfer coefficient. We will extend this sub-section or might also re-introduce it earlier, as $K_{\mathrm{m}}$ appears. In lines 8 - 10 on the same page we justified

9. *p. 7 l. 8: Probably not the correct word here and I would doubt that you can infer "realistic" behaviour simply by referring to similar results that also used extreme simplifications. It is consistent*

*with those results, but this does not directly lead to a "realistic solution", after all your model development started from RANS equations. Are those realistic for wakes or all of the assumptions you make?*
We can rephrase it to 'be consistent with other results', as you mention. Also the RANS equations can describe the mean flow of anything. Therefore, why not a WEC wake?

10. *p. 7 l. 16f: So the error depends on the size of the turbine? This is unfortunate.*
Not really. The order of magnitude that should be considered would also change, if the size of the WEC changes. For example in a wind tunnel scenario this velocity gradient would also be neglected when compared to the shear stresses and turbulent momentum influxes. It is the ratio of expected wake length and rotor diameter D that is important.

11. *p. 7 l. 19: "will later be shown to be small". It is hard to follow this argument if there is no justification of it yet.*
Okay. The reader can have a look at the results real quick, or believe the authors and that they do not intend to mislead the reader at this point with something very obvious to dismantle, if not true.

12. *p. 8 l. 2: It is easily guessed where the origin of your coordinate system is, but it should not be down to guessing.*
Good point. We add a quick mention of the origin of our coordinate system.

13. *This is wrong, I am sure you meant $u_r = u_0 \cdot (1 - a)$ and be careful here as "a" (the induction factor) is a function of $C_T$ and not a constant. Make it a function of $C_T$ otherwise your model is simply incorrect for almost all turbines, as they operate below $C_T = 8/9$ below rated $C_T$ and with it a quickly drops. In case of $C_T = 8/9$, $a = 1/3$ at the rotor ($x = 0$), only in the far-field will it become $2a$ and your statement is correct. Is this your assumption? Still turbines mostly do not operate below rated and thus you cannot simply make this statement!!!*
Okay.Yet, we used a measured value from the data available at 0.5 D. If anyone will ever use a theoretical value for the initial condition he or she should do as you mention. Also we will change the citation to Betz (1920).

14. *p. 8 l. 6: I am sure you can find a more appropriate reference here, otherwise omit it.*
We will have a look into it.

15. *p. 8 l. 7: Often this part is already called far-wake. Why then give a fixed number for the end of the intermediate wake?*
True. We were using the boundaries used by Frandsen. We can omit rigid numbers for wake distances. This is true. The intention was to give the reader also a vague impression of the distances.

16. *p. 8 l. 13: which is assumed to equally act in the lateral direction!*
Yes, which is mentioned in Section 2. This is why the parameter $C$ is introduced and the lateral and the vertical flux are combined to twice the vertical flux. And therefore, all momentum influx is expressed in the vertical momentum flux.

17. *p. 8 l. 17: ?*
This is the term how Frandsen describes and distinguishes the turbulence from ambient turbulence. And it is quite fitting.

18. *p. 8 l. 19: This is an interesting assumption, however TI does influence the breakdown of the wake, i.e. the near-wake region gets smaller depending on TI. There is entrainment from the sides as well as you mention but ambient TI also influences this core you mention. Please comment on this assumption.*
This is exactly what we mention in the previous sentence (l. 15f) that ambient turbulence aka TI is acting on the wake turbulence aka influencing the wake turbulence and all its dynamics.

19. *p. 8 l. 19f: Why? This is different from other commonly employed wake models, which have shown some promising results. There they are either linear (as the wake growth is assumed linear) or one could argue that the expansion growth rate should be locally proportional to TI, but it is hard to see*

*why the decay should increase and not decrease. After all the mixing reduces as TI decreases with x.*

Well, $\alpha$ needs to increase, since the auxiliary variable $d$ the decreases. One could implement a linear $\alpha$ but then one has to know the wake length, which is the purpose of a model to determine.

20. *p. 8 l. 24: There is a lot of published LES, RANS and measured wake data out there, you do not need to make a "guess". You need to justify your choice.*

    In the presented HeliOW project we also work with LES modellers. State of the art LES models can not provide a decent calculation of blade-tip core radii. Their radius depends on the grid size for example. Marion Cormier has a publication pending on this topic. Therefore, we do not want to rely on LES calculations. Actually the other way around is the desired pathway. We want in-situ measurements and see if LES calculations are correct or comparable. We also are not aware of any high-quality measured in-situ wake data. We will, however, increase the effort in justify our choice here.

21. *p. 8 l. 25: What is R?*

    Half the rotor diameter. We will change it.

22. *p. 8 l. 26: Why?*

    As can be seen in Fig. 1 the thickness of the rotational symmetric volume is $d$. We will make it more clear to the reader.

23. *p. 8 l. 28: Again why? It seems that you make this choice as your model is really a far-wake model, so you essentially assume that the near-wake is fixed at 2D. This is not the case, but a assumption which is fine, but you need to argue for it.*

    No. We make this choice after having a look at our data, this is why we state 'with the available data'. And as aforementioned in the script, this distance is not fix and depends for example on the thermal stratification of the lower atmosphere. We will make this point more clear.

24. *p. 9 l. 4: You previously argue why you think that d(x) should not be constant but for some reason you still consider this case nevertheless. What is the reason for this? It for sure seems weird that d(x) should be constant, as this seems extremely unphysical following your definition of d(x) as the turbulence thickness. Is there any physical argument to assume that it is constant?*

    Several reasons. It is easy to calculate and then to show the difference between the two solutions. Also there is a model (EFFWAKE) out there using constant wake deficit decay, although it is a wind park wake model. We will add a short explanation why we calculate this solution, too.

25. *p. 9 l. 6: A little more detail would be appreciated. It is an important factor in your model. Where does it come from and what is its significance and physical meaning?*

    This sub-section should probably be merged in when the gradient method for the Reynolds shear stress is introduced. We can add a description of the physical meaning.

26. *p. 10 l. 1 (Fig. 2): It should be stated that this is the way the authors assume the wake behaviour to be.*

    We will add this statement.

27. *p. 11 l. 2ff: This description is irrelevant in the context of this paper.*

    You are partly correct. In the context of this paper it may not be too relevant. Yet, the authors are part of the HeliOW project funded by the BMWi (essentially tax payer money). Therefore, we are held to introduce the project we are working for and what the data are used for.