Emmanuel Branlard (Referee)

Referee's comment *In this paper the authors present a dynamic inflow model suitable for FOWT, and verify the results against high and mid fidelity simulations. This is a nicely written paper, with interesting methods and conclusions. I have some general comments that I hope can improve the revision of the paper.*

Answer: Thank you for the kind comment. You comments have been very useful towards improving the work. Thank you.

Referee's comment

- I believe the paper would benefit from adding more justifications for each of the important equations of the model. You'll find several specific comments in the pdf regarding this.*My general comments are the following:*

- I believe the paper would benefit from adding more justifications for each of the important equations of the model. You'll find several specific comments in the pdf regarding this.

Answer: *The text ws modified to address this, including the comments in the pdf, which are listed below.*

Referee's comment *- Some results for various radial positions would probably be needed to support the conclusion that the model compare well with the ring model for up to r/R=0.8.*

Answer: *Section 3.4 was added, where the model is compared with CFD simualtions and semi-free wake vortex model simulations, includign results at different radial positions. These results are used to support the discussion and conclusion, which are modified.*

Referee's comment

- Comparison with similar models: How does the model compare with the model of Oye, and Hawc2? All models use two time constants. Oye's model has the advantage of being continous. - I would suggest adding a discussion section to address the following points:

- Comparison with similar models: How does the model compare with the model of Oye, and Hawc2? All models use two time constants. Oye's model has the advantage of being continous.

Answer: *Section 3.3 was added, where the proposed dynamic inflow model is compared with several other dynamic inflow models, namely the one by Pitt and Peters (1981) as described by Yu (2018), by Øye (1986) as described by Yu (2018), the model by Larsen and Madsen (2013), the model by Yu (2018) (also described by Yu et al. (2019b)) and the model presented in the work of Madsen et al. (2020).*

Referee's comment *- What are the limitation of the current model towards the tip? How could these be lifted?*

Answer: *Once again, we refer to the new Section 3.4 .*

Referee's comment *- Vortex ring state: The paper mention that vortex ring states do not occur as commonly thought, but I think this might need further justifications. The paper demonstrates that at high frequencies, the variation of inductions are limited, but variations are expected for lower frequencies. The cases studied in this paper were reasonably far from "high thrust" conditions. I think it would be worth investigating the variation of amplitudes of "a", for various "k" and "CT", and try to reach the vortex ring state. There has to be a point where the vortex ring state will be reached. (Obviously, this will likely go* *beyond the region of validity of the model and the vortex-ring-based models, so it will have to be treated with care – I do not expect the vortex-ring based model to accurately capture the vortex-ring state which will be highly turbulent and diffusive.). The question that could be answered and would be really interesting would be whether the vortex ring state model occurs "sooner" (for some low frequencies maybe) than one would expect from the steady conditions (zero frequency), or "later", or simply "at the same time". I think such an investigation will really add to the paper (again, keeping the limitations of both models in mind). At least a small moderation on the fact that the vortex ring state was not really "tested" would be great (I understand that the study still makes a point that it was not reached for "moderately loaded" rotors).*

Answer: *Previous authors claimed that high thrust coefficients occurred because the perceived velocity in the reference frame of turbine becomes very low or negative, and that this represented a vortex ring state . That interpretation is incorrect. However, regardless of the motion, the streamtube can enter vortex ring state if a large loading is applied for a long enough time. So, the work does not mean that vortex ring state cannot occur, only that the interpretation of the velocity perceived in the reference frame of the wind turbine does not represent vortex ring state. The text is modified to further clarify this.*

Referee's comment

Congratulation for your work, I'll be looking forward to review a revised version of this paper.

Emmanuel I enclose some specific comments (along the lines of my general comments) in the pdf enclosed.

Congratulation for your work, I'll be looking forward to review a revised version of this paper.

Emmanuel

Answer: *Thank you very much for the additional annotations and the overall appreciation. The answers to the comments in the pdf can be found below.*

Annotations by second reviewer

Referee's comment *suggest stressing again here that vact is constant (time invariant). (note on p.2)*

Answer: *Thank you for the very good suggestion. The text has been added explaining Equation 2 is only valid when* v_{act} *is constant.*

Referee's comment *suggest: arbitrary or periodic (note on p.3)*

Answer: *Thank you for the very good suggestion. The text was modified.*

Referee's comment *I would suggest using small omega to avoid confusion with Omega typically used for rotor speed. The context is yet clear in this paper. (note on p.5)*

Answer: Ω *was replaced to* ω

Referee's comment *How realistic is it to assume a uniform and sinusoidal CT distribution? I'm guessing you have found this to be true using higher fidelity/vortex method. Could you discuss/mention this a bit here? (note on p.5)*

Answer: *Thank you for this observation. It also connects with the next observation. The following text was added: The sinusoidal loading approximates the load oscillations observed by other authors, as described in Section... . The load change is a first-order result of the sinusoidal change in the non-entry boundary condition on the blades/actuator surface caused by the sinusoidal motion (this is further expanded in Section...).*

Referee's comment *You can maybe add here the formula that supports this sentence (I'm a formula person..) (note on p.5)*

Answer: *Thank you for the suggestion. Equation7 was added and the text was extended to explain it.*

Referee's comment *It took me a bit of time to understand this figure. Could it maybe be made clearer in the text that this figure simply shows what are the "operating conditions" tested in the literature. (note on p.6)*

Answer: *The caption of the figure was changed to indicate this.*

Referee's comment *I believe the model of Oye (found also in the book of Martin Hansen) also uses two time scales, and predates these references. (note on p.7)*

Answer: *The reviewer is absolutely correct. A reference to the earlier work by Øye has been added. The Øye model is also used in Section 3.3.*

Referee's comment *Is this model not also inspired by the one from de Vaal? (note on p.7)*

Answer: *The simulations by de Vaal were in Fluent. Or is the reviewer suggesting another reference? The text is not changed.*

Referee's comment *Potentially use vact in this formula (note on p.8)*

Answer: *The formula is correct according to the derivation. It is not a typo. The formula was not changed.*

Referee's comment *Can you mention how this formula was obtained as a quasi-steady solution? (note on p.8)*

Answer: *The text has been modified to explain the formula more clearly. The formula is an adaptation of the 1D actuator disc thrust equation, where the term of mass flow rate is changed to the weighted term.*

Referee's comment *Could you justify the use of this formula? For an actuator disk moving against the wind, I would think the convection velocity would be Uinf - uact - ustr/2, no? Maybe this could be mentioned/discussed in the text. (note on p.8) Coming back up here, I noticed that you have both the notion of uact and vact. It was not clear to me that there was a distinction between the two. What is meant by the induction velocity of the actuator disk? (Similarly, the other terms in this equation might need to be clearly introduced and defined to avoid confusion). (note on p.8)*

Answer: *The text has been expanded to include a more detailed explanation.* u_{act} *is the induction at the location of the actuator in the reference frame of the reference wind speed. the velocity of motion of the actuator is defined as* v_{act}.

Referee's comment *It was not clear to me that this was not already the case. Could you stress above (or using subsections) that the first developments are for a constant vact? (note on p.9)*

Answer: *The derivation of the model was for the case of a oscillatory motion (average displacement is zero). The additional equation allows to consider a reference frame of unperturbed wind speed and an actuator motion which as a non-zero average displacement (e.g. forward motion plus oscillatory motion). The text was modified to make this clearer.*

Referee's comment

I believe Oye uses u_int *for instance. (note on p.9) It seems that uact is actually an intermediate induced velocity. Can you give a physical meaning to this velocity? I would suggest another notation, because uact has been confusing me above, it can easily be confused wih vact.*

I believe Oye uses u_int *for instance. (note on p.9)*

Answer: u_{act} is not an intermediate velocity, it is the induction at the actuator. the definition of u_{act} was edited to be made clearer.

Referee's comment Could these equations be written in continuous form? (like Oye) (note on p.9)

Answer: Yes. But this formulation has an higher order of numerical integration.

Referee's comment It might be worth (somewhere in the text) to mention how this formulation differs from Oye's formulation, and Hawc2 formulation. My first impression is that they are very similar, modulo some scaling and definitions of time constants. (note on p.9)

Answer: The comment is correct. A text referencing this was added.

Referee's comment More justifications would be needed here the choices do not appear straightforward to me. Could you discuss/justify them? Could you mention why were the induced velocity are not used in the time constants for instance? (note on p.9)

Answer: Text was added to justify the lengths scales as relations to the scales of wake expansion and vorticity-velocity solution system. The second question of the reviewer is not clear, as the induction velocity is used to determine the time scales.

Referee's comment Potentially mention in parenthesis the sign of vact. (note on p.9) **Answer:** Added to the text.

Referee's comment Could you precise in the text which reference velocity is used to define a and CT? (note on p.10) **Answer:** All values are defined in relation to U_∞. Text was added to this effect.

Referee's comment Could the results of the dynamic inflow model be plotted at different radial position too? (note on p.10)

Answer: The formulation of the dynamic inflow model is 1D. The radial variation, which is modelled in other dynamic inflow models, has not been translated to this new model. That topic is left for future research.

Referee's comment Is there a reason for this choice? How does the model perform at other radial stations? Stronger induction effects might be found at larger radial position (closer to the wake). Could you show a small study for different radial position? (note on p.11) **Answer:** As in the previous comment, the dynamic inflow model is 1D. In the text, the reference to "center of the actuator" was removed.

Referee's comment It might be worth stressing in the figure which velocity is used to define a and CT. (note on p.12)

Answer: Text was added to the effect.

Referee's comment It might be worth discussing what's "wrong" with the model towards the tip. (note on p.17) **Answer:** This was addressed in a previous comment. The text has been modified to address this.

Referee's comment I don't think this was presented in the paper, or I might have missed it. Presenting some results for this would be great. (note on p.17) **Answer:** This was presented in Section 3.1. However, the text was modified for clarity.