Anonymous Referee # 1

Referee review of "Dynamic inflow model for a Floating Horizontal Axis Wind Turbine in surge motion" by Ferreira et al.

Referee's comment *The paper deals with a timely and interesting set of questions, related to the state of actuator disk/momentum theory for the case of oscilatory disk motions. Clearly this area is of interest for wind turbines placed on off-shore platforms that will oscillate back and forth and will change the inflow velocity being seen by the system. The overall conclusions, which this referee finds reasonable and interesting, is that if properly formulated, standard actuator disk approach still works, as long as the correctly chosen U-infinity(t) is used. The introduction and motivation are well described and the survey of prior work (in particular Fig 1) is very good. The introduction also gives the impression that a more fundamentals oriented rational method will be proposed to deal with non-inertial to inertial reference frames etc etc. that has caused confusions in the past. So that all seemed very promising.*

Answer: Thank you for this positive starting comment. The objective of the work is to derive a model based on correct physics.

Referee's comment *However, once the "meat" of the contribution starts being described, the material is suddently presented as an "algorithm" to be implemented in python, etc. and there seems to be no connection whatsoever with any actual physics or principles being invoked. That is to say, where did Eqs. 20 and associated Eqs. for $u_a ct, u_s tr$ etc, Eqs. 17 & 18 come from? There seems to be no connection with any actual physics or principles being invoked.*

Answer: Thank you for this helpful comment. The expression "algorithm" has been replaced by model. More importantly, text has been added/modified to explain the derivation of the equations.

Referee's comment *More specifically, in line 195 authors claim to be computing "new solutons for the streamwise induction velocity at actuator". What equation is being solved exactly and how is the solution obtained? Up to this point in the paper there is not a single dynamical evolution equation being presented. One would expect some equation of the form $\mathrm{d}u/\mathrm{d}t = ...$ and then the solution is Eqs 17,18 etc. Instead, what the authors seem to be doing is simply a-priori assuming that a time filtering will have benefits of some sort to be used as inflow for the model implementation to come later, but it does not look like Eqs. 17 and 18 are "solutions" to anything in particular. Only in point 6 of the introductory sentences there is a reference to a time-filtering method (Larsen-Madsen model). In that paper the time-filtering was motivated simply by saying something along the lines of "engineering model for response functions" including inertia of structures etc. How is that approach really justified in light of the very fundemantal sounding comments made in the introduction of the paper? This paper should provide a clear discussion of these aspects.*

Answer: Several points are mentioned in this comment. We will aim to address all. The model aims to present an equivalent solution of the vorticity-velocity problem, in the perspective vorticity is shed at each time step and previously shed vorticity is convected away from the actuator at each time step. This can be approximated by a convolution of the current solution and a new steady state solution. This approach is on the basis of dynamic inflow models such as the one by Øye (1986), Larsen and Madsen (2013), Yu (2018) and Madsen et al. (2020). These models often referred to a filtering approach of the near and far wake, which is a reasonable description; we opted for the same description, but the language is not totally

correct. The text has been modified to avoid the word "filter" and instead present the evolution from one vorticity system to a new vorticity system.

Regarding the point of the need of an explicity $du/dt = ...$ formulation, here one politely disagrees with the referee. The dynamic inflow model by Pitt and Peters (1981) (and also the ECN model) has an explicit time integration of du/dt because it models a linearized form of the unsteady momentum equation. The model of Øye (1986), although it presents a $du/dt = ...$ formulation, is in fact solving the same convolution problem as the models by Larsen and Madsen (2013), Yu (2018) and Madsen et al. (2020), just with a different numerical integration procedure. The formulation of solutions that decay with time through an exponential of time (as these last cited models and the model proposed in this work) provides an implicit form of time integration and, a clearer interpretation of the phenomena. However, the equation presented in this work can be converted to a $du/dt = ...$ formulation as Øye (1986). We have added/modified the text to make this clearer.

Referee's comment *Presentation of results (Figs. 3-5) show one cycle of resulting induction factor for various conditions and good results compared with the semi-free wake vortex ring model are shown. Was the inflow velocity time-filtering approach simply proposed by noting empirically from such plots that time-filtering the input would yield desired results? And parameters obtained by fitting the observed behaviors? That may be a fine approach for very applied settings, but unless better justified by analysis of governing equations, it it does not seem to rise to the level of a scientific contribution since it does not seem convincing that it can be generalized in any way to other conditions.*

Answer: Choice of the formulation of the model was not based on what works. Once it was defined that the model needs to evaluate the solution in the inertial reference frame and accounts for the motion of the actuator, it was necessary to have a formulation that was invariant with the reference frame, and that is the vorticity-velocity formulation. The model needs to account for the change of the vorticity system, as new wake is shed and old wake is convected, and the relative position of the actuator in relation of the vorticity system. The text was modified to better explain this.

Referee's comment *In view of the above comments, it is recommended that the authors aim to justify and derive the "time-filtering" approach somehow, if that is possible. If not possible, publication in WES is perhaps not fully justified and also, then the characterization of prior work (references to past "confusions") should be reworded to avoid raising the readers' hopes that the present paper will clarify these things.*

Answer: The changes to the text should clarify the physics behind the derivation of the model.

Some additional comments for minor revisions, if useful:

Referee's comment *Abstract, first sentence: the statement " ..surge motions .. when faster than the local wind speed, cause rotor-wake interaction." Do the authors mean to imply that only if surge motion is larger than, say, 8 m/s (local wind speed), there will be rotor-wake interactions? One would expect "interactions" even at much lower surge motion speeds.. Needs more precise wording. It seems when authors say "interactions" they have something very specific in mind but at this stage of the paper readers will have more general interpretations of "interactions" in mind.*

Answer: The text is modified to specify *blade-vortex interaction*. The abstract is also revised.

Referee's comment *Line 24: do the authors mean to say "a turbulent wake with the wake in front of the turbine?" since the normal state of turbine wakes is a turbulent wake state in the first place.*

Answer: The precise sentence written above was not part of the text. For clarification, "turbulent wake state" describes the streamtube loading condition and wake-breakdown/flow reversal downwidn of the rotor. That is not the normal state of a wind turbine wake (which is turbulent, but not in "turbulent wake state").

Referee's comment *Sentences are often unclear referring to undefined properties that are perhaps coming later? Text needs careful proof-reading for such things. For instance, line 171, there is talk about "to be used later as a forcing function for the filter functions". At this stage of the paper, it is unclear what filtering functions this refers to. Again, wordings need to be critically reviewed throughout.*

Answer: The text, and in particular the section mentioned, has been changed and reviewed.

Referee's comment *I found the set of 9 "hypothesis" (lines 85-110) a bit tedious to go through, some read like the conclusions of which one is not yet convinced without reading the rest of the paper, others read like additional assumptions, etc. They really read like sentences in a research proposal and seem suboptimal at this place in the paper. I would recommend restructuring/shorten/or even delete 85-110.*

Answer: The text was modified.