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# Comment on wes-2021-34

Anonymous Referee #1

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Referee comment on "Dynamic inflow model for a floating horizontal axis wind turbine in surge motion" by Carlos Ferreira et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-34-RC1, 2021

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

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.

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_act , u_str etc, Eqs. 17 & 18 come from? There seems to be no connection with any actual physics or principles being invoked.

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 du/dt = ... 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.

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.

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.

Some additional comments for minor revisions, if useful:

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.

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.

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.

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.