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Comment on wes-2021-34

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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-RC2, 2021

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.

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.
- 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.
- 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.
  - What are the limitation of the current model towards the tip? How could these be lifted?
  - 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).

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

Please also note the supplement to this comment:
https://wes.copernicus.org/preprints/wes-2021-34/wes-2021-34-RC2-supplement.pdf