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Interactive comment on "Actuator Cylinder Theory for Multiple Vertical Axis Wind Turbines" *by* A. Ning

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Thank you for your feedback.

> Due to the model limitations in its present form as mentioned by the authors (a corrected linear version of the model) future work on computing the flow field for a full nonlinear AC model e.g. by CFD would be of big value.

Agreed.

> P5 l23 and P6 l7: It is mentioned both places that the wake terms could be omitted and instead one could apply a momentum deficit factor from some other VAWT wake model. This should be elaborated a little bit more. For example if such wake model can be integrated to yield a continuous transition from the wake flow and to flow outside the

wake as computed here with the AC model ?

Yes, I will elaborate more in the paper. All that is needed is a wake model that predicts the velocity deficit at any location. Then those velocities are evaluated at each control point rather than using the full freestream velocity. The goal wouldn't be to evaluate a velocity field, although one could do that, just to compute loads as is focused on here.

> P12 l7: It is mentioned that the notation corresponds to assume that blade sweep is accomplished through shearing rather than rotation of the blade section. As rotation of the blade sections might be the way that sweep is introduced the force equation for that case could also be shown.

Either approach can be used to define the same geometry. It does not mean that one can't rotate the blade, it just depends on whether you define airfoils streamwise or normal to the leading edge. I've taken the convention used in the aircraft industry, and the one we decided was most convenient when developing the latest version of AeroDyn. I don't think it is worth adding a second convention when it's not the focus on this paper.

> P13 l4: It is mentioned that "Madsen notes that this linear solution produces good trends for the induced velocities, but is off in magnitude". This should be modified and expanded to say that the linear solution is off in magnitude for high loading but fits well for low loading measured by the thrust coefficient.

Great point. I'll add that.

> P18 l2: Please explain what "Julia" is ?

Julia is a relatively new programming language for technical computing: http://julialang.org. I'll add a concise description to the paper.

> P18 l5: The investigation has been carried out for spacing down to 1R. However, it could be interesting to go to even closer spacing as this is relevant for at least one floating VAWT concept which has two rotors on the same floater and where the spac-

ing seems to be less than 1R. http://www.nenuphar-wind.com/en/15-the-nenuphar- solution.html

Great suggestion. I can go down to even smaller spacing.

> Page 19: For the interpretation of the contour results in Fig. 15 it would be very helpful to show the rotation direction on the plots by an arrow.

Great. I will do that.

> P23, l15: The presented data from The Caltech Field Laboratory for Optimized Wind Energy show that the Counter Down configuration shows a benefit compared with the isolated turbine and thus opposite to the present results. As it can be seen that these turbines have a much lower Cp it could have been interesting to see what the present modelling would show for a turbine configuration matching the low Cp shown in Figure 22. This could be done by adjusting mainly the profile drag coefficient. It might be that a considerable higher airfoil drag coefficient would have a major influence on what is the most optimal configuration. However, as the paper already has a considerable length it is probably not good to propose any extensions.

Good points. I chose to focus on both turbines operating near peak cp, and you're right the Caltech turbines also have much lower Cp. I will do some exploration, but I agree, there are many cases one could look at and exploring them would make this much longer and is not the focus of this paper. We are interested in a subsequent paper to better understanding the effects of close interacting VAWTs across a range of models and fidelities.

> Page 24: In the conclusions it seems that in particular the mid part from line 16 to line 27 is not really conclusions from already presented material in the paper but new discussions of other research results from literature. This should be moved to the main body of the paper.

Ok. I can fix that.

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