

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

**Anonymous Referee #1**

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The author develops an extended version of actuator cylinder theory to predict the performance of multiple VAWTs simultaneously. The resulting model is used to predict the performance of pairs of co- and counter-rotating VAWTs. It is concluded that co-rotating turbines experience no net benefit, whereas counter-rotating turbines experience a small benefit. However, the author claims that the benefit of the counter-rotating pairs only persists if the wind direction has a narrow distribution.

This paper could be a useful contribution to the literature on VAWTs if the author can validate his model. At present, the reader is expected to accept the efficacy of the model on faith. In particular, the predicted power coefficients are much higher than those found in practice, with a maximum  $C_p$  of over 47 percent predicted at a tip-speed ratio of 3.46. The large discrepancy between this value and actual measurements leads to skepticism of the accuracy of the author's model. Moreover, the abnormally

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high  $C_p$  likely impacts the corresponding thrust coefficient and wake predictions. Given the importance of properly capturing the wake in order to predict inter-turbine interactions, the conclusions of the manuscript are brought into question by the aforementioned issues.

Figure 15 should be significantly improved to clarify the results being presented, as this appears to be the crux of the paper. For example, why is there no data in a streamwise swath of the plots? Also, if the plots show the performance when turbine 2 is at a given location, then why is turbine 2 power reduced when it is further upstream than turbine 1? And, why is there a thin line of blue color along the edge of the white streamwise swaths. These seem to be numerical artifacts. If not, the author should explain them. If so, they also cause skepticism of the fidelity of the model. Lastly, I have stared at this figure for quite some time, and I can discern no difference between the two top panels (i.e. for co- and counter-rotating), or between the two middle panels. Yet, the 'combined' panels are significantly different. This requires better explanation/presentation. If some panels are intentionally redundant, this should be stated explicitly (or better, the redundant panels should be removed). Also, a clearer statement of how the performances are 'combined' would be useful.

The author's primary claim appears to be that the benefit of close turbine spacing is lost when wind direction changes. This conclusion does not appear to be consistent with available measurements, e.g. the Caltech dataset mentioned in the manuscript or earlier work by Dabiri (J. Renewable and Sustainable Energy 3: 043104). Since the author used measurements from a real site to produce figure 22, presumably the effect of wind direction could also be evaluated and compared with the current computational model.

Perhaps more concerning is that even in direct comparison with previous computational studies for narrow wind direction distributions (e.g. Bremseth and Duraisamy, 2016; Korobenko et al., 2013; Araya, 2014), the predictions of the current model appear to be an outlier in terms of the limited interaction between the turbines. It is possible that all

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of those previous models are incorrect and the current model is the right one, but the author needs to do additional work to demonstrate that claim via a more rigorous set of validations using measurements of interacting turbines. The authors are especially encouraged to examine (and to cite in their revision) the recent publication by Ahmadi-Baloutakia, Carriveaub, and Ting: A wind tunnel study on the aerodynamic interaction of vertical axis wind turbines in array configurations, Renewable Energy, Volume 96, Part A, 904–913.

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