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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, this is all very helpful. Responses below.

> In particular, the predicted power coefficients are much higher than those found in practice

Agreed, but these values are consistent with actuator cylinder theory. I have validated the model against the original actuator cylinder theory using published data from Madsen as well as data provided in a private communication from Madsen. These power coefficients are also about the same as is predicted by double multiple streamtube theory. Keep in mind that this is an idealized turbine (no tower, struts, etc.) and so it is expected to be higher than actual measurements. Existing studies have already been performed to look at the accuracy of actuator cylinder theory. It is of course not

as accurate as LES or unsteady RANS, but is rather in the class of double multiple streamtube theory for speed, but with better accuracy than DMST.

> For example, why is there no data in a streamwise swath of the plots?

That is the wake region. There is data there but it's just a really low normalized power. As seen on the colorbar scale, all plots are adjusted to the same scale, and so values below this scale do not show up. If the colorbar range is extended all the way to zero, the wake would dominate the plot and you wouldn't see any other details besides the wake. I need to note this in the caption.

> 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?

Turbine 2 is what is moved, but not all plots are the performance of turbine 2. The first row is power of turbine 1, the second row is power of turbine 2, last row is combined. So the power of turbine 2 is reduced only when it is downstream of turbine 1.

> And, why is there a thin line of blue color along the edge of the white streamwise swaths.

start of wake region. cut off because of colorbar range as noted above.

> 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.

As noted in the text, the differences are subtle. For the second row, second column figure, you'll notice that the top red region is a a little darker in color and the region is slightly larger. Similarly, the first row, second column has is a little lower (more blue) in the bottom half. I agree, that the differences are hard to see, but that's what contributes to the small differences observed. Perhaps having the first and second row is not that helpful and should be removed?

> Also, a clearer statement of how the performances are 'combined' would be useful

They are just simply added. (P1 + P2)_together / (P1 + P2)_isolated

> The author's primary claim appears to be that the benefit of close turbine spacing is lost when wind direction changes.

This is not a primary claim, just an observation that the turbines will be operating within each others wake for some regions of the wind rose, and so any assessment on power benefits must be carefully examined in this broader context. I am aware of the mentioned paper by Dabiri. It does seem to show no apparent detriment when one turbine is waked by the other, in fact the largest increase ($\sim$30% power increase) appears to occur when one turbine is mostly waked by the other! I have no explanation for that. Perhaps I am misinterpreting the plots. All of the other papers on this site show a more limited wind rose. I have looked at the experimental data provided by Dabri's lab and don't see evidence of benefits while waked. In the provided experimental data, there isn't a 2-turbine case, and limited angles are available, so it's harder to assess across a full wind rose, but as suggested I could take a closer look at the impact of wind direction from the results.

> 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 of those previous models are incorrect and the current model is the right one,

I make no such claim. I simplify extend an existing theoretical approach that has been useful for an isolated turbine (at a conceptual design level) and observe what the consequences are. In fact I mention that both a separate wake model, and a separate induced velocity model would be necessary to improve accuracy. Perhaps I am not strong enough on explaining the implications. I am not at all suggesting that this simple method is more accurate that unsteady CFD or experiments. Just reporting on what

the model predicts and where improvements are needed.

> The authors are especially encouraged to examine (and to cite in their revision) the recent publication by Ahmadi- Baloutakia

I will certainly do so.

> Figure 15 ... appears to be the crux of the paper. ..The author's primary claim appears to be that the benefit of close turbine spacing is lost when wind direction changes.

I think I need to do a better job in the intro/conclusions on clarifying what the focus of the paper is and what the implications are. It is simply a derivation of how one could extend an existing theory to handle multiple turbines, and a (limited) exploration of some of the consequences. The focus of the paper is not on quantifying the interaction between VAWT pairs. That would require an entire paper of its own (and in fact is something we've been working on). That case was simply chosen as an example. A model like this would be useful for conceptual layout and optimization, as it is instantaneous as compared to CFD, but clearly still has deficiencies. There is no claim that this is a more accurate model than CFD or experiments, or that it is even accurate enough to be useful (without supplementary wake and induced velocity models).

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