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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Towards practical dynamic induction control of wind farms: analysis of optimally controlled wind-farm boundary layers and sinusoidal induction control of first-row turbines" *by* Wim Munters and Johan Meyers

Anonymous Referee #1

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This was a good and interesting paper. The results are well presented, and the explanations are clear. The authors use an interesting approach to explore what is possible with induction-based wind farm control and find a method which can be reduced to a simple control law and increases power in LES. The authors use the results to make interesting points which might be fruitfully followed-up in their own research, or can inform similar research in wind farm control.

For example, the result that much of the value of axial control is produced by the first row alone is an interesting, and useful result, as it might imply that the job of imple-

menting wind farm control can be limited to perimeter turbines. Perhaps this result will hold for other types of wind farm control.

The sinusoidal frequency used to encourage wake recovery, its presence in the optimal solution's spectrum, and its resilience to changes in turbine and timing is an additional useful result. Finally, the flow analysis and discussion adds to the intuition of how this, and more generally, how all wind farm controllers finally will gain power. For example the point that the flow over the intermediate turbines is lower energy then what flows over the first turbines, and so can only be less-profitably mixed down is a good point I hadn't seen made before.

The main problem confronting the approach appears to be the probable impact on turbine loads in varying the thrust to such a degree. However, the authors acknowledge this and identify this several times in the paper as a subject of future work. It would be an interesting problem to see, perhaps using a more detailed turbine model, if the required forcing of the flow can be generated in a realizable way with loads that not too severe. With low-enough frequencies this would seem at least possible, but perhaps the fact that the peak thrust is being raised in the Ct3 controllers will unavoidably raise loads.

A second issue might be the gain in power appears to be very small in the latter sections where a full wind farm is simulated (unless I misunderstand). Considering that LES simulations typically simulate one of the more promising wind directions for a wind farm, back of the envelope often suggests a gain in 10% or similar for an overall increase of AEP to be significant when all wind directions are considered. But perhaps this is a misunderstanding of section 4.2.3.

However, the paper contributes insightful analysis and presents an interesting study of how axial-based wind farm control can successfully raise power in LES, which had seen to be a challenge using static points. Further, the insights of the paper I believe are generally useful for understanding the underlying challenge of wind farm control.

Very interesting.

Specific comments:

One comparison that came to mind reading the paper, is that the results show very little value in optimizing the most-downstream turbines, and in general, improvement in power comes from modifications upstream, and that self-optimization is not possible. This stands in contrast to a result such as:

Ciri, Umberto, Mario Rotea, Christian Santoni, and Stefano Leonardi. “Large Eddy Simulation for an Array of Turbines with Extremum Seeking Control.” In American Control Conference. Boston, MA, 2016.

Where the TSR of downstream turbines is re-optimized for wake conditions (and the upstream turbine is left as is at the end of the optimization). It would seem the difference in modeling methods and/or how turbine control is implemented yields the different results, but I believe it would be worth discussing the difference, for example around the paragraph beginning with "The figure shows that the first row (R1)..." on page 12.

Figure 3/related text: Would another way to describe Ct2 vs Ct3 be that Ct2 can only lower the thrust, while Ct3 is allowed to raise it?

Page 17: "...NREL 5MW rotor with a 50% increase in chord length..." does this imply the method is currently assuming the chord length is variable? Could this not be achieved by change in pitch angle?

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