Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2018-15-RC2, 2018

eawe european academy of wind energy
Open Access 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 #2

Received and published: 16 April 2018

The authors submitted a well-written paper, which focuses on one of the hottest topics within wind energy applications: wind farm control. The possible power gain with induction based control strategies via further enhancing the turbulent mixing is investigated. The presented analysis of the dynamic induction control starts with further assessment of the previous study, yet the necessary background work is included properly so that the paper still reads stand-alone.

Distributing CT set-points dynamically to use the turbines as flow actuators is a novel and a very interesting idea. The control framework and the considered wind farm scale

Printer-friendly version

Discussion paper

LES setup is described/illustrated properly - except of the characteristics of the turbines/actuator disks considered. Would be nice to have the size of the disks explicitly noted (as they are somewhat hidden in Figure 2) to have a more clear scale of the considered wind farm. From here on, the comments are more detailed and listed following the structure of the manuscript.

- While describing the case setup in Section 2.2, the "flow advancement time", TA (also referred in Figure 1) is considered as half of the prediction horizon T. Would TA (and therefore T) be inflow dependent as the time delay (the time it takes for particles to move from the upstream to downstream turbine(s))? Have you investigated if changing T (and/or TA) has any effects on the resulting optimum CT set-points and on the power gain?

- As clearly seen in Figure 4c and 4d, there is a significant increase in turbulence further downstream. In addition to the TKE and the transport, would be nice to have the turbulence intensity TI values (as listed later on page 20, 10% for the baseline case), both for the baseline case and the maximum added TI reported - possibly somewhere around Figure 4. That again would give an indication on the applicability compared to the field values observed. Also note the typo in the caption of Figure 4: after c) all the subplots are marked to be continuously c).

- On page 10, around line 10, the argument of "upstream actions do not require a specific downstream response in order to increase power in that downstream row", which is also paraphrased in the conclusions, needs to be elaborated. This rather broad conclusion seem to oversee the probability of the curtailment of the downstream turbine where down-regulation might be inevitable for certain CT set-points assigned to downstream turbine(s) in the resulting optimization. Could be partially true for the investigated C3t5 case since there observed very limited curtailment even at the most upstream turbine (as in Figure 3b). However, also seen in Figure 8b (except of the very last row as the authors indicated), there seem to be still a difference between on the power gain at turbine R11 for the scenarios of R1-R10 and R1-R11. Narrowing

Printer-friendly version

Discussion paper

the argument to the considered case or very little to no downstream curtailment CT distributions is suggested.

- On page 13, line 13, "the presence of the flow invariant features of the control signals" needs further justification as Figure 11 would also depend on how variant the flow features are in the simulations. That should include both the spatial and temporal variance within the 30-min window. As far as the field measurements are concerned, high spatial and temporal correlations are observed. For the former, Figure 2 gives a brief idea about the wind speed range between the columns, that can be referred here. For the latter, time series or relevant temporal statistics can be presented to assess the randomness and strengthen the hypothesis.

- On page 17, around line 5, a very nice example on how to implement the optimized sinusoidal CT is presented. The practical examples can be further improved by a short discussion on the expected response time of such increases in tip speed ratio on a machine with high inertia. That would put the estimated sine wave period into perspective as well.

- For Section 4.2.3, the header "Full-scale wind farm test" is a bit misleading... Suggest to change to "Full-scale wind farm simulations (in LES)" instead.

- On Figure 19, why would the power decrease after Row #5 for the sinusoidal case?

- Page 22 around line 5, the (inevitable) discussions on loads are included. In addition to the loads on the controlled upstream turbine, Figure 20(b) indicates partial wakes on the further downstream rows of turbines. Therefore, the section should be improved by highlighting the probable increase in fatigue loading for not just turbine(s) R1 but for the downstream rows as well, possibly starting as early as R3.

On the grammatical note, the manuscript is clear and easy to follow. The only comment might be on the use of Sect. or Section; Fig. or Figure references.

Very interesting and innovative work!

Interactive comment on Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2018-15, 2018.

Printer-friendly version

Discussion paper