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Interactive comment on "Periodic dynamic induction control of wind farms: proving the potential in simulations and wind tunnel experiments" *by* Joeri Frederik et al.

Anonymous Referee #1

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The paper is well presented and well argued, and adds valuable contributions to the literature, including a first analysis of loads in dynamic induction control as well as a wind tunnel study validating the approach. The figures and descriptions are good, and the paper is very direct to understand.

Mostly minor comments follow below. Main over-arching comment is really a question to propose be considered in the next version of the paper. Fig 9 shows very small effect on turbine 3. Is this to be expected? In completing this review, I re-read "Towards practical dynamic induction control of wind farms: analysis of optimally controlled wind-farm boundary layers and sinusoidal induction control of first-row turbines" and found

this passage:

—— Figure 8 illustrates that the first-row optimized thrust coefficient also results in a significant power increase in the third row, which is not observed using the sinusoidal thrust strategy. Furthermore, the analysis of the modified control cases in Fig. 11 proves that the first-row controls are also partially synchronized with the flow. This shows that other mechanisms, dependent on specific flow events for increasing wind-farm power, are at play as well. Even though the application of regression algorithms in an attempt to link turbine actions to low-dimensional flow measurements (e.g., local velocity, shear and kinetic energy) has been unsuccessful thus far, similar analysis based upon more complex flow features (e.g., vorticity structures, high-speed turbulent streaks, or downdrafts) might be more promising. This requires further optimal control simulations over an extended time, as the total control time horizon of 30 min in the current dataset is insufficient for robust statistics in this kind of analysis. This is an important remaining challenge to be addressed in future research. ——

As well as this from the conclusion of the same paper:

—— Although the first-row sinusoidal control led to a robust increase in total power for a reduced-size 4×4 wind farm, a full-scale test indicated that downstream turbine activity is required to obtain increased power at larger farm scales. It was also shown that the simple sinusoidal strategy does not lead to increased power extraction when applied to downstream intermediate turbines. Identifying the mechanisms for power increase in these turbines hence remains an important open research question. ——

My reading is that yes, these results do confirm this, the third turbine is not expected to increase in power unless (if I understand correctly) 1. The first turbine pursues a non-sinusoidal DIC or 2. The second turbine performs DIC additionally

Do you agree? Are there plans to try any DIC on the second turbine etc?

Small Comments:

Fig 1 could use a more descriptive labeling/caption, it's not clear what each of the lines represent

DTU 5 MW turbine (Jonkman et al., 2009) âĂŤ shouldn't that be NREL? 5MW (based on reference provided)

Table 1, for experiments the control input is Beta, but amplitude is specified in Ct? (Now I see this is explained later in the text, but might be good to ensure the explanation is indicated in the table or indicate to the reader explanation is coming?)

Figure 6: This is a really useful view into the loading impacts

Is there a reference for Weibull-weighted DELs? A nice idea, are they used often?

Fig 7-8, why do the effects persist above 15 m/s? I believe this addressed in text, but could be useful to re-iterate in caption, maybe also indicate with a vertical line where the DIC would be actually shut off?

Fig 8: seems to have an error in caption

Section 6.2 Do you use the FLORIS model of Gebraad 2016, or the newer gaussian model of Bastankah within FLORIS? Maybe provide FLORIS version number?

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