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

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Very interesting work, which I strongly recommend for publication. I have a number of smaller comments, that should be relatively easy to incorporate in a revision.

1. abstract: "In this paper, only periodic variation, " –> variations

2. Figure 1: please improve. In 1a (bottom) – for clarity, please indicate levels of C_T associated with different velocity profiles. In 1b, not clear what the order is of the velocity profiles (in time or phase of the sinusoidal forcing). Also not clear what the order is of the velocity profiles (in time or phase of the sinusoidal forcing). Also not 100% convinced that this will be the effective response – is this an 'artists' impression, or is this based

on some model? Please clarify in the fig caption and text.

3. In the paper, it is suggested a couple of times that CFD is performed:

- Page 2: "Simulations will be executed using the high-fidelity Computational Fluid Dynamics (CFD) environment SOWFA"

- Page 7: "Once the optimal DIC parameters in terms of wake mixing have been evaluated using CFD, ..." However, apart from these, CFD seems not to be really discussed... Please clarify. If you use CFD in some way, it would merit a much lengthier description (computational domain, mesh, boundary conditions, models used, some results, ...)

4. Figure 2: how was this figure constructed (please make caption more self-contained). Did you use the procedure described on top of page 4? Or did you use BEM, or the Cp-Lambda model, ...

5. page 4: "A region I-1/2 with constant rotor speed equal to 6 rpm extends from the cut-in speed of 4 m/s to 7 m/s." I'm a bit surprised by this – please double check. As far as I remember, in region 1.5 the rotor speed is increasing, and not constant .

6. Table 2: for completeness, please add values for average pitch angle and amplitude of pitch oscillation

7. Following up on previous point, for sake of reproducibility, it would make sense to add a detailed figure with the C_T & C_T' signal together with the pitch signal and the rotational speed signal

8. page 7,line 15: "Once the optimal DIC parameters in terms of wake mixing have been evaluated using CFD, ..." not sure CFD is used... - cf point 3 above? How did you determine optimal DIC parameters?

9. page 8, line 9: please refer again to Turbsim, and IEC when you reference to NTM

10. Figure 8, check caption

11. page 11, start of section 6.1: five different cases are mentioned, but later on, results of only three experiments seem to be reported (the ones with different amplitudes). What about results for block signal, and results for phase difference between turbines?

12. Figure 9: I'm a bit confused: in the caption you mention different amplitudes, but in the legend (bottom-left panel) you seem to show averaged values for C_T (1, 1.5, 2). First of all – are these averaged values of C_T' (see table 2)? Therefore, do you mean different average & amplitude. Please clarify and improve caption/legend

13. page 15, line 4: "It can therefore be concluded . . .". In the work of Munters, Sinusoidal DIC was shown to work for the first turbine, with a positive effect on the second, but not on the third. Sinusoidal DIC applied to the second (or later) turbines did not work. The results in the current paper seem to confirm this. Therefore, this conclusion should probably be adapted/tuned down a bit + maybe additional discussion on future work in the conclusions section.

14. Continuing on the previous point: what about the results of the out-of-phase experiment with the first & second turbine (cf. comment 11 above) – was this intended to improve turbine 3 performance – if so, what were the results. Did you do in-phase as well? Reading the text, I'm presuming that most experiments were only using sinusoidal DIC on the first turbine? Is that correct? Should maybe be emphasized/discussed a bit more throughout.

15. page 15, line 15: to be fair, you should compare weighted DEL against weighted power gain (which will also be much lower when averaged over a Weibull distribution)

16. page 16, line 1: significant differences between simulations and experiments. What do you mean by that? please clarify. . .

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