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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Wake redirection at higher axial induction" *by* Carlo Cossu

Wim Munters (Referee)

wim.munters@gmail.com

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In the manuscript "Wake redirection at higher induction", the author describes a study into combining wake redirection techniques from yaw and tilt control with increased turbine induction. The study is well-described and the structure and elaboration of the manuscript is very clear and easy to read. The overall contribution to the field is rather limited and incremental, i.e. tilt control at higher induction has been shown in an earlier study of the same author (albeit using a different turbine model); and combined yaw and induction control has been shown in earlier studies by Park Law and Munters Meyers (albeit using different ways of generating control strategies). That being said, the current work is still highly relevant to the general community and I believe the topic is suitable for publication in Wind Energy Science. However, I feel there are several points for imprfovement of the quality and novelty of the considered work, as detailed below in my comments.

Major comments

- The added contribution of the current paper is relevant but incremental: overinduction has already been shown to work for yaw and tilt control in earlier LES-based studies (Cossu 2020b and Munters and Meyers 2018 respectively. The author shows that this strategy also works in his current setup (with a slightly different turbine model for tilt, and a static vs dynamic control strategy for yaw). The added value of the current paper over existing literature would benefit from a more detailed flow analysis of the current LES results. For example, it would be interesting to see expand Figure 1 with additional flow field sections and compare to results from Cossu 2020b, which would allow to show effects of wake rotation on tilt-based redirection. Further, a flow-based comparison between the differences for yaw and tilt control would be very interesting. For example, the author mentions in line 160 that the shift of larger optimal angles after including pitch is present for tilt but not for yaw, and that this can 'probably' be explained by observing that vertical shear is not exploited by yaw. The author has the data to show this quantitatively, and I feel this could be an important addition to the current work.
- The first goal of the study is to assess whether additional gains in overinductive tilt control still hold up when considering realistic turbine models closer to reality. However, I believe that this goal is only partially achieved and the step forwards from the Cossu 2020b study is relatively small. A significant step forward would have been made using an actuator line model instead of an actuator disk model. The limitations of an actuator disk model should be mentioned earlier in the study (currently they are left to the conclusions). Some comments related to this:
  - In the conclusion, the author mentions that 'the absolute level of power gains is larger in Cossu 2020a, b'. Unless I'm mistaken, this is not mentioned in the main text. The author should attempt to explain this. Could this be due to

the different turbine model (e.g. accounting for wake rotation), or a different wind-farm setup (i.e. 2 rows vs 3 rows)?

  - The increase of C_T' shown in Figure 3b requires some further explanation, is this caused by a change in the effective angle of attack of the blades?
  - The author shows the dependency of C_T' on different control parameters, but there is no mention of how e.g. the pitch angle affects the power coefficient C_P (or C_P' if you will). This should be clearly mentioned

- The author frequently mentions achieving 'doubled' or 'tripled' power gains in high induction compared to baseline tilt/yaw control. Please be more specific in phrasing here to avoid confusion: mention explicitly the percentages, and the setup (e.g. Cossu 2020b has a three-row setup, achievable power gains are different than when looking at two rows as in the current study).

- Starting from line 131, the author discusses that he believes increasing thrust in tilted conditions should not impact turbine loading compared to standard operation, since the overall thrust force would not be higher than in the latter. However, Fleming et al (Renewable Energy 2014), have shown that tilt control can have a significant influence on blade bending and drivetrain torsion. Further increasing thrust could aggravate such issues. I believe that turbine loading could be an issue at higher induction scenarios such as considered here, and that conclusive statements warrant a detailed analysis using aero-elastic codes. This should be mentioned in the manuscript.

Minor comments

- Line 59: typo oveinductive should be overinductive

- Line 82, the formula for C_T' contains a π which shouldn't be there

- The appendix states use of a Schumann BC at the wall. What is the roughness length imposed at the bottom and, more importantly, what is the resulting turbulence intensity at turbine height? This tends to have a significant impact on power deficits and hence achievable gains.

- A 3x3 km periodic precursor domain is probably too small to generate fully realistic turbulent flow structures. Does the author expect this to affect results in any way?

- The author rightfully mentions surprisingly little research efforts into combining yaw and induction control. An additional study that could be mentioned here is "Munters, Meyers, 2018, Optimal dynamic induction and yaw control of wind farms: effects of turbine spacing and layout. J Phys Conf. Ser 1037, 032015", which investigates combined dynamic yaw and overinduction for a series of different wind-farm layouts

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