Review of the paper: “Experimental analysis of the effect of dynamic induction control on a wind turbine wake”, by D. van der Hoek et al.

General comment

In this paper, an experimental investigation is performed, with the aim of evaluating the impact of dynamic induction control on wake development. The in-wake flow velocity is measured through PIV. Extremely interesting the analysis on the tip vortex behavior. Such an analysis appears to be even more interesting given the fact that it is completely done experimentally: the effort that the authors did to perform such an analysis and to interpret the results is praiseworthy. To my best knowledge, this work represents (unfortunately with some limitations) the first attempt to understand the working principle of dynamic induction control using only experimental data.

I recommended “minor revisions”, because there are two concerns that I would like the Authors to address before the final acceptance of the paper. Such issues are reported as “important comments”.

Important comments:

1. Section 2.2: Since the pitch variation employed in the dynamic induction control strategy is not centered around the pitch settings of the greedy control (9 deg), the resulting technique is to be viewed as a combination of both steady and dynamic induction controls. The dynamic induction control is triggered by the periodic pitch variation, while the steady control is due to the steady bias between the baseline pitch and the mean value of the DIC pitch motion.
   This can be also viewed in figure 3 in terms of thrust coefficient. Clearly, the DIC is here a combination of steady variation of thrust (one may compare the mean value of the blue line and the value related to the black dash-dotted line), and a periodic variation of the thrust itself, aimed at promoting a faster wake recovery.
   This fact may lead to misinterpretation of the results.
   Notice that this issue would have been totally avoided, if the baseline control had been implemented with constant pitch equal to about 10.5 deg (i.e., the value associate to the average of the DIC pitch).
   Please, explain possible impacts of this inconsistency (it’s hard to understand from the presented results whether the impact is negligible or not, at least for the scope of the paper), explain the reason for this choice.
2. Sec 2.3, lines 152. Moving the wind turbine to different positions may imply that the rotor may feel a different flow field (due for example to the fact that the flow modifies within the open chamber, both in terms of velocity and turbulence intensity). Please, give some insight into the possible impact of this issue. Why did the authors decide to keep cameras and led modules fixed and to move the turbine?

Minor comments:

1. Section 2.1, line 75-76: Although one may envision that it will be possible to extract the thrust from the tower root bending moment, it could be interesting for readers to have access to some details of the adopted procedure. For example, what is the impact of nodding moment and tower drag on the measurements? How were these effects taken into account?

2. Section 2.1, line 83: “calibration function”: same issue of the previous point. Some details (or at least a reference, if possible) may be beneficial.
3. Section 2.2, It seems that the control is only set for partial power region. If so, this should be noted in the text.
4. Sec. 3.1: Where is the wind speed U_∞ measured? This question is connected to the second important comment. There could be a significant difference between the velocity in the different positions at which the turbine is located. This may also explain the difference in the measured thrust coefficient (especially that between x/D=2 and x/D=3, in fig. 5). Moreover, why wasn’t the wind speed measured in a previous test with all measurement devices (seeder, cameras, etc.) present, but without the turbine? In this way, one would have had a more reliable velocity measure, at the positions where the turbine would have been installed.
5. Sec 3.1, line 183: “we believe that the principles… can still be investigate …”. Here again, many of the incongruencies could have been avoided with a slightly different testing execution. Please, explain the reason why the Authors believe that.
6. Fig. 10 and 11, it’s a pity that an test with steady induction control with a pitch setting equal to the mean value of the DIC pitch wasn’t carried out. One could have had access to what is due to steady induction, and what to dynamic induction.
7. Line 287-289: The analysis is extremely interesting; even more, if one considers that it came out of experimentation. However, the contour of the baseline case at 2D is qualitatively similar to that of the DIC case (first plot case b). Why do Authors say “Here, it is seen that the vortices merged before a distance of x/D = 2”?