Comments to the paper no. **wes-2021-70**

Experimental analysis of radially resolved dynamic inflow effects due to pitch steps

The authors present a very interesting, detailed and well designed experimental analysis of the unsteady behavior of the wake-induced inflow in a model-scale wind turbine undergoing step changes in the pitch angle. Moreover, a very important effort is done by the authors to derive some general conclusions about the numerical modelling of dynamic inflow on the basis of their experimental evidences.

The topic of the paper is very interesting and, as the authors state several times, it is very important to have a detailed radial analysis (and not only global) of wake-induced unsteady inflow for the assessment of the available engineering models as well as higher fidelity solvers.

The paper is well written, although there are some minor technical corrections that I have proposed at the end of this document. As a general comment I found the paper quite long. I understand that the presented results are many and they require suitable comments and description. Nevertheless, I encourage the authors to try to make the paper more concise to avoid the reader getting confused by so many details. For example, some of the results could be included in a specific Appendix whilst only the most important ones are retained in the main text.

My indication is to **ACCEPT** the paper only after **MINOR REVISIONS** following the comments listed below.

SPECIFIC COMMENTS

1. **Figure 2**: although the description of the experimental setup is very clear, I suggest that the authors include a 3D sketch of it to replace Figure 2. This would greatly help the reader in getting a quick overview of the setup and of the instrumentation.

2. **line 113**: The authors state that the rotor blades are collectively pitched by 5.9°. In Figure 3 the pitch angle before imposing the step seems to be 5°. I suggest to include in the description of the rotor the blade pitch value corresponding to the considered operating conditions.

3. In **Section 2.1.3** the authors make reference to the work by Herràez (2018) for the description of the experimental technique used for the definition of the measurement line (the bisectrix of two rotor blades). For the sake of reading clarity, it would be beneficial to provide a bit more details supporting the choice of that line of measurements by clearly explaining the effect of each blade (bound circulation) and of the shed/trailed vorticity, and providing brief motivations for the limitations of this technique in catching the effect of trailed vorticity (which is very relevant close to tip and root of the blade). In this regard, I suggest also that the rotor plot in figure 4b should be made in 3D in order to better get the information about the point of view and of the direction of circulation on the blades. Finally, in Fig. 4A, the measured velocity components are not clear: is the tangential component a radial one? I believe that a 3D view of the rotor could help also in this regard.

4. If I understood correctly, in **Section 2.1.3** the results of Fig. 5 should be comparable with the data in Herràez 2018. If so, it would be nice to plot the reference numerical data on top of the presented results. Moreover, in the cited paper the induced (perturbation) velocity reaches 0 value at the bisectrix

of each pair of blades, i.e. at azimuth 60°, 180° and 300°. In the present results this is not verified (even if the mean value of the measurement, about 4 m/s, would be eliminated from the data) so the authors are invited to clarify these discrepancies. The operating conditions (wind speed, rpm) are not reported. In the paper by Herràez it is clear that the numerical data are obtained in a phase-locked way, i.e. considering blade 1 at 12 o'clock position and computing the velocity along a circle of radius r. Differently, here the authors seem to consider a fixed point whilst the blade is rotating. Please clarify this point.

5. **Section 2.2:** in the end of the paper the authors correctly state that the proposed methodology for the estimate of the dynamic inflow time constants can be very important for the enhancements of engineering models. From the presented analysis it is also clear that the synthesized time constants depend on several parameter such as the radial position, the pitch direction and (?) TSR. So it would be very interesting to include in the paper a brief discussion on how this methodology could be generalized to be used in an engineering model which must be applied to several load cases in the design of a wind turbine.

6. **Section 2.3**: the authors explain that in the load reconstruction from induction measurements lift and drag coefficients are obtained by Xfoil. Has any correction to take into account for 3D effects been considered? It is well known that sectional loads are typically underestimated if purely 2D polars are used in the framework of BET/BEMT theories. My impression is that loads reconstruction could be improved by the use of 3D-corrected polars. For the NREL 5 MW rotor these are available in the report by NREL describing the turbine characteristics.
In this section it is also mentioned that the model by Pirrung et al. 2017 is used to take into account unsteady airfoil aerodynamics. Even though the paper is cited correctly and easily found in the literature, for the sake of clarity it would be beneficial to add (here or in a devoted Appendix) the main details of this model.
Finally, I did not understand the sentence on line 231 "The typical time lag….": maybe it is related to some aspects of the model by Pirrung? In any case I suggest to rephrase the sentence to clarify the role of the two mentioned time constants.

7. I did not fully understand the sentences from line 255 to 260. My understanding is that the aim is to determine *a priori* a range of variation of the AOA in unsteady (pitch step) conditions. Please rephrase the sentence to make it more clear.

8. From the presented induction results it is evident that, in general, the time constants for model 1c and 2c depend on the considered radial position and on the pitch direction. Moreover, they also depend on the distance of the fitted measured velocity field from the rotor disk. Do the authors have any evidence that they also depend on the TSR? In order to use the 1c and 2c model within an engineering aerodynamic tool, it would be beneficial to have a relationship that somehow links the time constants used for fitting the induction to the operating conditions and the radial station. Moreover, do the author have any proposal on how to generalize the values of the synthesized constants in order to use them in different load cases (different pitch step but not only, for example yawed flow or floating wind turbines)? In other words, do the author think that the time constant values found in this analysis could be used for other turbines in other operating conditions?

9. **Line 306**: the authors highlight that for the tangential induction factor, a different value for t0 was used with respect to the axial case. Moreover, the variation of t0 along the radius is quite relevant. To my understanding, this radial variation was not present in the axial induction. Moreover, a different value of t0 is used also for the fitting of the velocity field further downstream (fig. 17). The author

should comment on this difference and on the sensitivity of the time constants with respect to the choice of t0.

10. **Figure 17**: might the drop in velocity at 0.2 R before the new equilibrium somehow be explained by the effect of the nacelle? Which is the radius of the nacelle?

11. **Figure 19:** The caption is not clear (as well as the text on line 368). In particular I did not understand the definition of the wake ramp. Maybe a sketch could help in this regard.

12. **Section 3.3**: the paper includes several results. I don't think that in this section the (SG-no corr) results are really necessary as all the other ways of computing loads presented in this section do not include the mentioned correction.

13. **Line 390-391**: I did not fully understand this statement.

14. **Line 394-395**: as already pointed out in my previous comments, might the use of the 2D polars instead of the 3D corrected one form an explanation for the deviation of LDA recon loads with respect to the strain gauges measurements? Moreover, at the end of the discussion the authors state that the main driver for the observed differences between reconstructed loads and SG measurements are the structural interactions. I suggest to investigate also the effect of 3D flow phenomena that are not fully included when using purely 2D polars.

15. **Line 408-416:** the theoretical procedure described in these lines to obtained the results in Fig. 23 is not clear to me. Please rephrase the paragraph for the sake of clarity.

16. **Line 453**: it would be nice (not only here but in general in the discussion of the results) to indicate the percentage radial variation of t_slow and t_fast because the have very different magnitude and from the plot is not immediately evident.

TECHNICAL CORRECTIONS

1. Throughout the paper large use of personal forms (like "we", "us", "our"…) is made. I find the impersonal forms to be more appropriate for a scientific paper. Please revise the whole manuscript taking care of this aspect.

2. In the abstract and in the introduction both present and past tenses are used when referring to literature results and also presented results: please make a coherent choice. I would suggest to use always the present tense.

3. line 84: pitching speed should be indicated in rad/s

4. line 130: "blades induction"

5. line 236: replace "along" with "with" and "from" with "described in".

6. line 248: "low load case shows"

7. line 292: replace "shorter" with "smaller"

8. line 299: symbol t_fit was never defined before this point

9. line 525: replace “adopt” with “adapt”