

Wind Energ. Sci. Discuss., referee comment RC2
<https://doi.org/10.5194/wes-2021-66-RC2>, 2021
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Comment on wes-2021-66

Anonymous Referee #2

Referee comment on "Design, steady performance and wake characterization of a scaled wind turbine with pitch, torque and yaw actuation" by Emmanouil M. Nanos et al., Wind Energ. Sci. Discuss., <https://doi.org/10.5194/wes-2021-66-RC2>, 2021

The manuscript « Design, performance and wake characterization of a scaled wind turbine with closed-loop controls” deals with the design and the characterisation of the aerodynamic performance of a scaled wind turbine dedicated to wind tunnel testing of closed-loop control strategies. The wind turbine wake is also studied, in order to validate the representativeness of the turbulent wake flow compared to full-scale configurations.

The need for reliable fully closed-loop controlled wind turbine models adapted to wind tunnel capacities is undoubtedly proven. As stressed by the authors, a thorough technical description of the model, as well as the characterisation of its aerodynamic performance for the baseline configuration is crucial, but often neglected in the literature. On the other hand, this description and preliminary characterisation cannot deserve a publication in a scientific journal without any additional outcomes, providing new scientific insights to the community. Particularly, neither the closed-loop control strategies, nor their validations, are presented. Despite the mention to “closed-loop control” into the title, controllers are not used for the present study (only steady-state configurations for pitch, yaw and torque) and the model characterisation is also limited to basic statistics (except the dissipation rate). I would therefore recommend complementing this work with original results in terms of control strategies before publishing it.

Major comments:

- The title mentions “closed-loop controls”, whereas these aspects are not characterised or validated here. Only specifications of actuators are given
- P5, L120-121: “In fact, wake behaviour is independent from the rotor-based Reynolds

number when this parameter is larger than circa 10^5 : isn't it true only for the far-wake? Are Chamorro's results universal enough to be considered as a proof of this independency?

- P5, L125 : "Considering these various requirements and constraints, the rotor diameter was finally chosen as $D= 0.6$ m." : this statement seems a bit straightforward since no Reynolds number value had been given so far for the present model wind turbine. Another key parameter to choose the rotor diameter is the blockage ratio. A discussion on the targeted facilities is needed here.
- P6, from L136: The dynamic similarity is finally not respected since you have an efficiency of 30 instead of 120 in full scale. That should be clearly stated at the end of this paragraph.
- P6, L147-149: "In addition, as shown in Fig. 2, at these low Reynolds a standard wind turbine airfoil as S-806 (Tangler, 1987) suffers from multiple stall-reattachment cycles even at small angles of attack.": one cannot deduce physical explanations on the flow topology by looking at these results coming from Xfoil. One can just say that Xfoil calculations do not converge at these Reynolds numbers. In Xfoil, there are boundary layer transition criteria, how were they set up? It is also important to stress that the Reynolds number will evolve a lot along the blade span and that the given values correspond generally to the maximum Reynolds number that will be encountered at tip blade
- P8, L186 : "The power coefficient is estimated by using BEM": give more details on the BEM computations (parameters, correction options, etc)
- Figure 7: plot also the turbulence intensity profiles in both facilities. They should not be uniform and this information is important for the discussion on the wake added turbulence intensity
- P15, Table 2: the blockage ratio in TUM wind tunnel is 5.8% and not 5%. It is therefore above the threshold of 5%, which is commonly accepted as the guarantee to prevent blockage effects. Check the value and adapt the discussion to it.
- Page 17, Fig. 9: Give more details on the chosen parameters (λ , spanwise location, etc). The indications of velocity should be based on the relative velocity that the blade will experience, not on the inflow velocity only. Additionally, this relative velocity will probably be calculated at the blade tip thanks to the TSR and so, corresponds to the maximum relative velocity that the blade will experience. Reformulate the discussion accordingly.
- Page 18, Fig 10: why the maximum of power coefficient is 0.3 here and 0.4 on Fig 8a? Because HT instead of LT? Would it be the same in LT? Where the blade imperfections could be more influencing?
- P20, L413: "Figure 14 shows the downstream evolution of the velocity deficit at wake center for different thrust coefficients": how is determined the wake center? Give also the value of the wake growth parameter
- P23, L465, Eqs 7: please define more precisely the turbulence intensity: streamwise turbulence intensity? Horizontal turbulence intensity? Total turbulence intensity? Additionally, the subtraction of the turbulence intensity at hub height is relevant for uniform flow conditions. However, for BL flows, the turbulence intensity is non-uniform. This will fully bias the discussion on the added turbulence intensity. I would recommend subtracting the turbulence intensity profile of the inflows.
- P26, L506-508: "This is in agreement with previous studies (Bastankhah and Porté-Agel, 2017a) and in line with the observation that the breakdown of the tip vortices, which occurs at approximately $x/D = 4$, removes a separation layer between the wake and the ambient flow, thereby facilitating the exchange of momentum (Medici, 2006).": The second statement is not proven here.
- P30, L586: "The turbulence dissipation rate was also characterized in this work, for the first time directly from wind tunnel measurements." : it is not the first time. However, maybe the authors meant "the first time for our research group"?

Minor comments:

- P2,L39: give the blockage ratio
- P2, L47 : "The rotor aerodynamics is"
- At several locations "Reynolds number" instead of "Reynolds"
- P3, L78: reformulate "Against this background". Because of this background? In continuity of this background?
- P8, L195: "The rated rotor speed of the scaled model, ..., was primarily determined by the requirement to avoid compressible effects over the blade, as expressed by the condition ..., c_s being the speed of sound.": put this information earlier in the general considerations for the design
- P9, L 226-229: with which tool were these aero-elastic simulations performed?
- P12, L270 : "in the wind tunnel"
- P16, L349 : give the value of this optimum pitch angle, here and in the caption of Fig. 8
- Page 17, Fig. 9 caption: "Aerodynamic efficiency"
- Page 18, L384: "Figure 11a shows the airfoil efficiency as a function of angle of attack for the nominal and tuned polars." They are called "design" and "identified" polars in the figure 11 caption. Harmonize the names
- P18, L384-385: "Results show that, although not identical, the difference between the two sets of polars is small, which seems to indicate a good overall manufacturing precision of the blades": Doesn't it rather indicate that the experimental model behaves as the BEM expected?
- P19, Fig. 12: put the TUM results on the first plane, symbols are hidden by UTD results. Remove the continuous lines
- P20, L410: "The profiles of the two different inflow conditions are similar": The magnitudes of the velocity deficit are similar, but not the profiles.
- P23, Fig. 16: Change the color scale to have a better color contrast in the velocity map. No reason here to extend the color scale from -50 to +50%
- P23, Fig. 16 caption: "in the wakes of the G06 and of the DTU 10 MW"
- P24, L489: how is the axial induction factor determined here?
- P26, Eq 9: remove the bar on density by precisising that you make the hypothesis of incompressible flows.
- P29, Fig 23: plots are inverted (vertical profiles on the left and horizontal ones on the right)?