Wind Energ. Sci. Discuss., referee comment RC2
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Comment on wes-2021-30

Anonymous Referee #2

Referee comment on "Investigation into boundary layer transition using wall-resolved large-eddy simulations and modeled inflow turbulence" by Brandon Arthur Lobo et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-30-RC2, 2021

The manuscript describes the investigation of boundary layer transition on a wind turbine airfoil in freestream turbulence with different levels of intensity. The flow data has been obtained by wall-resolved large eddy simulations. The numerical approach appears sound, results are presented reasonably clear and a thorough discussion of the relevant aspects is provided. The article is well written, good to read and it certainly is of relevance to the community.

Revisions of the manuscript should address the following points:

- The grid refinement study is a very relevant point in the validation of the computational method. Therefore it should be performed a bit more detailed than only presenting instantaneous Q-surfaces. Besides the comparison of mean field quantities also a look at resolved vs. modeled turbulence would be beneficial.
- The geometry and mesh setup is not completely clear. A fixed velocity condition is mentioned for the farfield - does this imply, that the angle of attack is already included in the base mesh or does this lead to a prescribed outflow condition on the upper boundary?

How is the farfield pressure condition handled? With a distance of 8 times chord length between airfoil and farfield the domain appears to be rather narrow to avoid having pressure influence from the airfoil. Is there any non-reflective treatment or a validation for the domain size?

- Fig. 2 shows a decay of turbulence intensity upstream of the wing.

How is the prescribed intensity value defined, at which location is it supposed to be achieved and how is this assured? Since the wing disturbes the decay of freestream turbulence, have simulations of an empty field without a wing been performed for calibration? Especially for Ti=11.2% the curve shows an overshoot in the beginning followed by a strong decay and then some variations. Is the data statistically converged - if yes how do you interpret the fluctuations of intensity?

- The article is definitely very interesting and novel in terms of this specific airfoil. Several similar studies of systematic FST variation featuring other airfoils (SD7003, NACA0018 etc.) have been performed both numerically and experimentally. Please refer to these and compare them with the present findings.
- For the case of Ti=11.2% it is stated that no separation appears in the averaged flow field. It could be expected that still separated regions occur in the instantanous flow field. Please add a brief discussion of the instantaneous state.

Specific comments:

Fig. 1: For a better comparison the images would benefit from including streamwise coordinates and an indication of separated zones.

P.12, l. 312: double occurance of "Burgmann and Schröder"

P.20 l. 421: number mismatch: "streaks" is plurar and "delays" refers to singular