Response to referee 2's comments

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The authors thank the reviewers for the constructive comments and suggested improvements. A revised version of the paper has been prepared considering the reviewers' comments. A list of replies to the reviewers' comments is reported below.

RC2

RC2 a)

The major problem is that HAWC2 is applied to analyze a parked rotor. Note that the verification article the authors cited applies to operating wind turbines with rotating blades. To the reviewer, the validity of the results is subjected to questions. Since surrogate models are applied, why not using CFD or a lifting line model?

The authors acknowledge that Blade Element Momentum (BEM) theory is not applicable for a rotor in standstill. In this work, the aerodynamic model used in HAWC2 is the Near Wake Model (NWM) which has been implemented in HAWC2 and compared against analytical solutions and measurements for standstill conditions [1].

The verification article for HAWC2 has been changed to [1].

The information about the NWM model has been included in the paper. The second paragraph of the first section has been rewritten as

"SIV have been studied using the Blade Element Momentum theory (BEM) based solvers in the limited yaw angle range around moderate stall regions. The Advanced Aerodynamic Tools for Large Rotors (AVATAR) project details a comprehensive SIV study of BEM-based aeroelastic solvers against a higher fidelity CFD based aeroelastic solver [2]. The results show that BEM-based solvers tend to over-predict standstill instabilities due to the utilization of static airfoil data. But since the basic assumption in BEM theory that the rotor can be modelled as a disc does not hold in standstill conditions, other models such as the Near Wake Model (NWM) have been proposed for aerodynamic modelling in standstill conditions [1]. The Near Wake Model has been validated against analytical solutions for an elliptical wing and measurements against the NREL Phase VI rotor in standstill conditions. While the

NWM model deviates from the measurements in certain conditions, it is still a better choice than the Blade Element Momentum (BEM) model."

The reason for not using CFD or lifting line methods in spite of using surrogate models is that the time and effort required are still high for an initial domain exploration. This point is included at the end of the first paragraph of section 3 as

"A HAWC2 is preferred over CFD simulations because of the computational time involved. While a typical HAWC2 simulation takes around 20 minutes, CFD simulations typically take much larger computational time. Additionally, the complexity of setting up the simulations is higher than HAWC2 simulations. For an initial domain exploration, it is advantageous to use solvers like HAWC2, which is still costly for a 5-dimensional problem. The initial exploration results can help decide the focus of higher fidelity CFD simulations and lifting line methods that can be used for a detailed study of the instabilities."

RC2 b)

Table 1 shows a number of considered variables. Please clarify why they are selected here and whether there are other more important parameters

The variables in Table 1 are chosen because the wind speed, yaw angle, shear and veer affect the aerodynamic damping, and temperature affects the structural damping. There are more parameters related to environmental conditions, and blade geometry affecting SIV. We have chosen five parameters taking into account the computational complexity. Of course, the framework can still be used to study the problem effectively, but we have limited the number of dimensions to 5 for a demonstration. The following lines have been added at the end of section 2.
"While there are other parameters that affect SIV, the number of variables is limited to five due to computational considerations".

RC2 c)

Some of the statements are not justified. For example, "The reference temperature for the normal damping values is considered as 15 °C, and a decrease of 50% in the structural damping is assumed at -10 °C" Are there any grounds that can be used to support this statement?

The authors thank the reviewer for pointing out the necessity to justify the consideration about the assumed effect of temperature better. The authors would like to say that to the best of their knowledge, the exact nature and the amount of variation of structural damping of composite materials with temperature depends on the material type and orientation of the fibres. The amount of variation considered in this study have been reported in glass fibre composites, for example in figure 4 in [3].

This information is now included in the paper. The following lines have been added at the end of the third paragraph of section 3.

"While the exact variation of structural damping with temperature depends on the fibre material used in the blades and their orientation, glass fibre composites have been reported to show the amount of variation considered in this study [3]."

RC2 d)

The language is acceptable, but a check should be done to avoid a mixed use of American/British spelling in the manuscript

The authors thank the reviewer for this comment and have corrected the document to avoid mixed use of American/British spelling.

References

[1] G. R. Pirrung, H. A. Madsen, and S. Schreck, "Trailed vorticity modeling for aeroelastic wind turbine simulations in standstill," *Wind Energy Science*, vol. 2, no. 2, pp. 521–532, 2017.

[2] J. Heinz, N. N. Sørensen, V. Riziotis, M. Schwarz, S. Gomez-Iradi, and M. Stettner, "Aerodynamics of large rotors wp4 deliverable 4.5," Tech. Rep. D4.5, ECN Wind Energy, Petten, The Netherlands, August 2016.

[3] H. Zhen, W. Xiang, X. Xiao-lin, L. Zhi-Peng, and W. Li-Ying, "Study on glass fiber/epoxy gradient damping composites," *Asian Journal of Chemistry*, vol. 25, no. 7, p. 3831, 2013.