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Interactive comment on "Full scale deformation measurements of a wind turbine rotor in comparison with aeroelastic simulations" *by* Stephanie Lehnhoff et al.

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The authors thank the reviewer for the time to read and review the paper. We appreciate your detailed feedback and take this into account in the revised manuscript, where changes are highlighted in red. Each comment is further addressed below. The reviewers' comments in the supplement are addressed and highlighted in the revised version of the paper.

++ major issue ++ reviewer: The major issue in this paper is that the authors found the in-plane displacement to be greater than the out-of-plane one. This is in contradiction with the behavior of any turbine, and should be carefully investigated until the source

of the problem is discovered.

authors' response #1: It is important to note that the results shown are for in-plane and out-of-plane vibration amplitude, and not the absolute displacement from the undeformed state. The amplitude of vibration of the out-of-plane motion can certainly be lower than the in-plane motion, as this is dependent on the turbulence level, shear, wind speed, etc. (contrary to the absolute displacement, which, as you mention, is much higher in the out-of-plane direction, in particular for wind speeds close to rated speed). In addition, the measured behavior is in accordance with the aeroelastic simulations. Therefore, the authors do not see an issue with the shown results.

++ remark #1 ++ reviewer: It would be nice to add a few statements on how the markers on the speckle pattern are matched with the numerical model.

authors' response #2: From the speckle pattern, a distinctive radial position is chosen. This same radial position is chosen in the numerical model for comparison of results. This is added in the revised manuscript.

++ remark #2 ++ reviewer: I think that it's worth citing "Health monitoring of wind turbine blades in operation using three-dimensional digital image correlation" from Rong Wu et al.

authors' response #3: Thank you for that suggestion, we included the paper in the state of the art section.

++ remark #3 ++ reviewer: I understand that the illumination conditions are a challenge for DIC, but if I remember well, some authors applied phosphorescent markers, and did the test during the night.

authors' response #4: This is a possibility and we already conducted some pre-tests with this in our laboratory. But it either reduces the time slots for measurements, as this is only feasible during the dawn without artificial illumination, or it significantly increases the effort under application of artificial illumination. In this paper, the functionality of DIC should be proven, before the effort for further measurements is increased.

++ remark #4 ++ reviewer: Almost all of the mathematical formulas are not clearly written. The authors should specify the meaning of all symbols, and the indices of the summations.

authors' response #5: We completely updated this section in the revised paper and hope it is clearer now.

++ remark #5 ++ reviewer: The authors should elaborate on how the geometry of the undeformed blade is taken into account. I'm referring in particular to: cone angle, pre-bend, backward sweep and twist angle.

authors' response #6: The undeformed blade geometry is not known by the university and is not necessary for the measurement of vibrational amplitudes with DIC. We measured the difference of deformation between a reference state during the measurement, which is already deformed, and the following deformed states. Thus, the measured amplitude is not the absolute displacement of the blade but a relative displacement to the reference state, thus the amplitude of vibration.

++ remark #6 ++ reviewer: Without further analyses, I would expect that most of the correlation between the out-of-place deflection, and the flapwise bending moments, is due to the 1P.

authors' response #7: Yes, this is true for a large percentage of the design conditions. The turbine does not run close to any of the eigenfrequencies of the system, therefore it behaves mainly as a system undergoing a forced excitation, as you say, corresponding to 1P and its harmonics. The edgewise frequencies of the system are clearly identified.

++ remark #7 ++ reviewer: The comparison between the strain gauges and the numerical model could be expanded by including the Damage Equivalent Loads.

authors' response #8: This is a good idea for a following work, but it is not the purpose of the article. This would also require to develop a transfer function between

displacements and bending moments.

++ remark #8 ++ reviewer: In figures 20 and 21 the authors compare the DIC measures to 9 numerical simulations, that differ by the turbulence seed. I don't think that this is a fair comparison, as the purpose of doing simulations for multiple seeds is to get accurate statistics. I would thus compute statistics for the numerical simulations (mean, standard deviation, PSD, ...) and compare them to the ones for the measures.

authors' response #9: Figures 20 and 21 were included to show some direct results of a comparison between measurement and simulations. The authors agree, that these plots should not be used for a detailed analysis which is consequently not done in the paper. Thus, in figures 22 and 23, mean and standard deviations were compared to each other. As the simulations with multiple seeds do not differ a lot in the amplitude of OoP and IP deformation, the authors think that those plots are a valid tool for a comparison.

++ remark #9 ++ reviewer: Figures 22, 23, 24, 25, 28 and 29 are subject to the same problem. I think it would be better to have for each figure: - 1 curve for the mean deflection (over time) from DIC, as a function of the azimuth angle - 1 curve for the mean deflection (over time and seeds) from DIC, as a function of the azimuth angle - 2 curves for the 2 * std from the DIC – 2 curves for the 2 * std from the simulations.

authors' response #10: This was done as the DIC measurement does not span a long enough period to cover "several statistic timestamps", i.e. several 10-min intervals. The authors show comparisons of those statistics in Figures 23 and 25 for single simulations and errorbars for both DIC and simulations, including +/- standard deviation. If these curves should be shown for all simulations, the plots would not be clear to read anymore.

++ remark #10 ++ reviewer: I don't agree with using the FFT on noisy data, because the conditions are never constant, and the time series never periodic. I would instead use the Welch's method.

authors' response #11: We think that the FFT can also be used for noisy data, if the boundaries are moving which was the case in our plots. Nevertheless, we checked the reults for the Welch's method and agree with you, that this is the more useful method in this case. We thus replaced the FFT plots with Welch plots in the revised paper.

++ remark #11 ++ reviewer: For a future work I would recommend to run the numerical simulations using a reconstructed wind field.

authors' response #12: The numerical simulations are run according to industry standard methods similar to the activities relevant for model validation during certification activities within this paper. But we agree to your comment and are currently dealing with a comparison of DIC measurements and simulations, based on a reconstructed wind field.

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