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Interactive comment

Interactive comment on "Periodic stability analysis of wind turbines operating in turbulent wind conditions" *by* Riccardo Riva et al.

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We thank the reviewers for the detailed analysis of our work and the constructive inputs, comments and suggested improvements. A revised version of the manuscript has been prepared taking into account the reviewers' recommendations. A list of point-by-point replies to the reviewers' comments is reported in the following.

Detailed replies to reviewer 1

Reviewer *The authors could have referenced the paper by Skjoldan and Hansen (2012), "Effects of extreme wind shear on aeroelastic modal damping of wind* *turbines" (DOI: 10.1002/we.1495) showing e.g. that the aerodynamic damping of the first fore-aft tower mode is decreased in extreme wind shear.*

Authors Our paper focuses on the lowest little-damped modes of a wind turbine operating in normal wind conditions. In this sense, the first fore-aft tower mode and the extreme wind shears are not relevant for the present discussion. We have however now cited the suggested paper with reference to the trend of the damping factor of the whirling modes in normal operations.

Reviewer *Please check Equation (A6).*

Authors We have corrected the formula, as requested.

Reviewer *The authors could have referenced the paper by Skjoldan and Hansen (2009), in Appendix A with the review of the Floquet theory and the dynamics of periodic system because the final expressions for the modal decompositions can also be found in that paper. The helpful concept of participation factors for each mode by Bottasso and Cacciola (2015) is mentioned with the extension to the concept of output participation factors. Although lengthy, it is useful to have the entire theory in the appendix and the derivation of Equation (A44) is valuable (noting that I have not checked each step of it). The authors could have mentioned how the truncation is done.*

Authors We recognize that in proving the formula for the expansion of the State Transition Matrix over the modes and the harmonics, we have not provided enough references. We have now cited the suggested paper, as well as two more. The convergence of truncated Harmonic Transfer Functions has been studied in an article written by H. Sandberg, that we have now included in the list of references.

Reviewer *The authors seem to talk about several modes for the second order parametric Mathieu-Hill equation, which makes me wonder if the authors have the understanding that a LTP system with N DOFs have more than N modes (see line 25 page 18). A LTP system with N DOFs has N modes that may have an infinite number of harmonics in its periodic mode shape; the amplitudes of these harmonic components are given by the participation factors that one of the co-authors has introduced. Please consider to make this understanding clearer.*

Authors We are clearly very aware of this behavior, which has in fact been described in this and other prior publications by us. Nonetheless, we recognize that that sentence might be misleading, so we have now revised the text. In particular the word "modes" has been replaced with "super-harmonics".

Reviewer *Is there a typo in line 25 page 22? The damping ratios seem better predicted by the PARX method according to the relative errors.*

Authors Yes, there was a typo. We have corrected the sentence, by substituting POMA with PARX.

Reviewer *Lines 16-20 on page 26: I do not understand why the confidence level of the fitting across the results of the PARMAX models over the rpm range can tell anything about the accuracy of the individual models. Maybe I have misunderstood this statement.*

Authors The observation is correct, since the confidence levels only indicate where the results obtained by a new identification might lie. We have thus revised the

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statement, which is now written as “From the gray bands one can infer that each frequency and damping factor identified at a specific rotor speed is coherent with the others, as all the estimates define a clear trend. On the other hand, a significant but acceptable uncertainty still characterizes the participation factors.”.

Reviewer *Would it be possible to overplot the HPSDs obtained from the PARMAX models in Figure 6?*

Authors This figure already shows a large amount of information, and we think that it would be quite confusing to add five more curves. Figure 4 shows a comparison between the measured and the predicted blade root edgewise moment, although for a different wind speed. The agreement between the measured and the predicted outputs has always been very good, hence computing the HPSD from the predicted outputs would have yielded nearly the same results of the POMA. The main differences between the HPSDs computed from the measured and the predicted outputs would be:

- A lower frequency resolution for the HPSDs computed from the predicted outputs, since the PARMAX needs much shorter time histories with respect to the POMA.
- Mismatches in the high frequency half plane. This is due to the fact that the PARMAX does not match well the high frequency portion of the output spectrum. However, due to the little number of frequency shifts used for the HPSD, these errors do not propagate much into the lower frequency range.

Reviewer *I assume that the authors refer to the IEC classes for turbulence. The 5% TI used herein is not according to this standard so it is difficult for the reader to* *make this connection. Maybe consider to write the TI range for the wind speed range for the class B turbulence.*

Authors We have slightly reworded this sentence to make it more precise.

Reviewer *It would be better to write the modal frequencies and damping ratios of the blade and tower modes involved in the model. The actual stiffness and damping coefficients of the model does not say much.*

Authors We have chosen to show the values of the springs and dampers coefficients, since they directly appear in the equations of motion. This way, a reader would be able to replicate our results.

Reviewer *Maybe mention the initial conditions leading to this excitation.*

Authors Initial condition are described in the first lines of section 4.1.1, which state: "The blade edge-wise mode was excited by imposing the initial edge-wise angles of all blades equal to a unique non-zero value, whilst all other states were set to zero at the initial time".

Editorial changes

Reviewer *Sometimes the "i" and "j" are the imaginary number and sometimes indices. Could the authors define another symbol (such as $\imath = \sqrt{-1}$) and make it consistent throughout the paper?*

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Authors We agree with the suggestion, and adopted for the imaginary unit the symbol $\imath$.

Reviewer *The language is easy readable however sometimes more lengthy than needed. It can be shortened but I will leave it up to the editor to request such measures.*

Authors We simplified some of the sentences, where possible.

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