Wind Energ. Sci. Discuss.,
doi:10.5194/wes-2015-3-RC1, 2016

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

Anonymous Referee #1

Received and published: 21 February 2016

I recommend that the manuscript "Periodic stability analysis of wind turbines operating in turbulent wind conditions" by Riccardo Riva, Stefano Cacciola, and Carlo Luigi Bottasso is published after the authors have considered the comments below and in the attached PDF. The paper can be published "as is" except for some minor issues with equations in the appendix.

Suggested changes of content:

1. 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.

2. Please check Equation (A6). Should it have been either?

$$\tilde{x}_\tau(k+1) = \Psi^1 \tilde{x}_\tau(k)$$

$$\tilde{x}_\tau(k) = \Psi^k \tilde{x}_\tau(0)$$

or

$$\tilde{x}_\tau(k+1) = \Psi^k \tilde{x}_\tau(1)$$

3. 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.

4. 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.

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

6. 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.

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

Editorial changes:

1. 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?

2. 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.

Please also note the supplement to this comment:
http://www.wind-energ-sci-discuss.net/wes-2015-3/wes-2015-3-RC1-supplement.pdf

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