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Interactive comment on "Differences in damping of edgewise whirl modes operating an upwind turbine in a downwind configuration" *by* Gesine Wanke et al.

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The paper compares the damping of an upwind 2.1 MW wind turbine against its downwind counterpart. Several variants of the upwind and downwind configurations are compared in the analysis. Emphasis is put on the assessment of the damping of the edgewise whirling modes (BW and FW) which are the lowest damped modes of the turbine. Aeroelastic frequencies and damping as well as mode shapes are directly calculated from simulated nonlinear analysis time-series. The main innovation of the present work is that the authors directly correlate the damping of the edgewise modes with the out-of-plane motion of the blades which is distinguished into their self

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Discussion paper

motion component but also the component due to the tower torsion. The paper is well structured and written and the results presented are interesting. Therefore, in the reviewer's opinion the paper deserves publication after some revision is made to the original text based on the comments below. 1) Not much is said about the method used in the assessment of the aeroelastic frequencies and damping. For example if it is based on peak values characterization on nonlinear time series (it seems to be so), the pros and cons of this simple approach should be highlighted in section 2.1 or 2.2. For example how good the predictions would be in case of multiple frequencies, closely spaced. The same holds for the mode shapes identification method. The authors are recommended to cite some references and to add some more discussion on the theoretical background of the methods used in their analysis. 2) It would be also nice to include some comparison of the results of the present nonlinear analysis method against the results of the linear eigenvalue analysis in a case in which HAWCstab2 simulations are valid (eg. standard upwind configuration). In particular the comparison of the predicted mode shapes is critical because much of the discussion that follows is based on these predictions. 3) Another point that it should be further elaborated in the conclusion section is to what extend the results obtained and the conclusions drawn can be generalized to any WT configuration or they are turbine specific. Additional minor comments that should be discussed by the authors as well as grammar/syntax corrections can be found in the accompanying pdf.

Please also note the supplement to this comment:
https://www.wind-energ-sci-discuss.net/wes-2019-88/wes-2019-88-RC2-supplement.pdf

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