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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.

Anonymous Referee #1

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General comments: The paper deals with the study of the difference of in-plane damping factors if an upwind turbine is operated in downwind configuration. Moreover, a trade study has been performed in order to evaluate the effect of some design parameters (i.e. shaft length, tower torsion and cone angle) on the damping factors.

Damping factors are estimated from suitable timeseries obtained from an aeroelastic code, using a simple data analysis tool (i.e. the logarithm decrement).

The paper is of interest for the wind energy community. Moreover, from the manuscript one may imagine that there should be a possible industrial appeal in trying to operate

an upwind rotor in downwind configuration. This is surely a plus of this work.

However, there some points to correct/clarify/improve so as to produce a manuscript which is worth publishing. These points are listed as "Major comments" here below.

Major comments:

- I strongly suggest enlarging the description of the procedure adopted to excite the whirl modes. In particular: is this already present in literature? Fore example there is something similar in "Thomsen et al, A Method for Determination of Damping for Edgewise Blade Vibrations. Wind Energy 2000, 3:233-246". Is there a proof (or reference) that the phase and the order of the harmonic excitation (120 deg with a sequence blade 1-2-3 and 3-2-1) are able to excite independently the backward and forward whirl modes? This is an important point, as the method chosen to the compute the damping (i.e. the log-decay) does not perform well if more than one mode is present in the measure. One ore more figure with the simulation outputs while they ring down (with possibly an FFT to check the effective harmonic content) to beinserted in the text (even if the methodology is well known) could be useful to demonstrate this fact.
- It is not clear the reason to show all the results of section 3.2, 3.3, 3.4 and 3.5 (very interesting results representing the core of the paper) for the RTT rotor (that is the rotor with reduced flexibility). In fact, it has been proved a big impact of tower torsion on whirl damping. Using a tower too rigid (or better unrealistically rigid) may lead to misleading results as the relative effects of other quantities (e.g. cone and shaft length) could appear more pronounced respect the most impacting quantity, that is the tower flexibility. Please comment.
- A turbine with few degrees of freedom has been used. As far as I have understood, the tower bending (fore-aft and side-side) flexibility are not present in the turbine model. This simplification seems a bit strong to study whirling charac-

teristics. I suggest clarifying this point, especially if Authors believe that such a simplification may alter the obtained results.

- Check in the entire text the correct spelling of “forward”. It is often written as “foreward”.

Minor comments

- Introduction, line 14-16. The sentence should be either rephrased or complemented by a reference. In fact, one may easily imagine that blade tip to tower clearance could be an active constraint also for downwind configurations in abnormal conditions involving shut-downs, where large forward blade deflections are to be expected. I understand that for downwind configurations this constraint may be somehow “relaxed”, but I would consider “are not subject to such constraint” a bit too strong for such a complex problem.
- Line 50: “overall” instead of “over all”.
- Line 76: The sentence “The turbine flexibility is reduced to the rotor flexibility and tower torsional flexibility . . .” is not clear. Please, rephrase.
- Line 85: Are coupling terms due to pre-bend and shaft tilt expected to play a prominent role in in-plane vibrations?
- Line 96: Setting the gravity to zero entails two significant effects. First, as written by the Authors, the periodic loading at rotor frequency caused by blade weight is nullified. Second, the periodic change in the blade stiffness is neglected. In fact, when a blade is upward, it compressed by its own weight, leading to a lower stiffness; on the other hand, the opposite happens when the blade is downward. This causes a periodic variation of the rotor/blade properties, which may have a significant effect on the turbine response.

- Line 104: “excited” instead of “exited”
- Section 2.2: The use of the Coleman transformation should be better explained:
  - Why only for 9 m/s?
  - The possibility to distinguish between the blade self-motion and substructure motion through the Coleman transformation is interesting but deserves additional descriptions. I may say that the two sources of vibration can be separated simply by looking at the spectra of the loads, as they should show up at two different frequencies. Hence, without the need of the Coleman transformation. But on this point, I may have missed something. . .
  - Line 123: I would use “Coleman” capitalized.
  - Line 126: “latter” instead of “later”.
  - Line 129-133: The procedure adopted to guarantee the phase consistency needs additional explanation. Is it a standard practice?
- ‘Results’ section, figure 1 and line 140: Consider the possibility to write the normalization definition (“normalized with the damping of the upwind RTT configuration at 9 $ms{-1}$ of the forward whirl mode”) also in the text to ease the reading.
- Line 154-156: “This indicates that the difference in damping is driven by the interaction of the aerodynamic forces on the rotor with the tower torsional motion”. I consider this result important. Can the Authors say something about the tower bending motion? I expect whirl modes to be dependent also on the entire flexibility of the support structure, not only on the tower torsion.
- Line 155: “when the aerodynamic forces are not present, the damping of both forward and backward modes are identical”: it is difficult to see it from the figure, as from the text I would expect two coincident lines.
- Line 159 (186): “cannot” instead of “can not”.

- Line 174: “Overall”?
- Line 176: check the sentence, “displacements of to the substructure”
- Figure 2: The figure and the treatment seem interesting and important in the economy of the paper, but it should be better explained. How were those graphs generated? Are they results of the simulations, possibly transformed through the Coleman transformation? If so, why aren’t they damped?
- Line 203: “From” instead of “form”
- Line 203: “is due to a difference in out-of-plane motion.” It should be clarified whether it is a generic out-of-plane motion or the one entailed by tower torsion.
- Line 206: “The edgewise damping can be increased”, to be checked. Do the Authors refer to the whirl damping or to the blade edgewise one? If I have understood correctly, it should be the latter.
- Section 3.2: in general, a good section. But what about the cosine components of rotor and substructure? Those components may generate out-of-plane vibrations contributing to the total damping of the whirl modes as well.
- Figure 3, 4 and 5: I would use “normalize whirl damping” avoiding the use of “edgewise damping” which may be misinterpreted with the blade edgewise damping.
- Chapter 4: In the model, only the tower torsion is considered. Do you expect that the tower bending may play a significant role? if not, why?
- Line 294-295. “From an edgewise damping point of view downwind configurations could benefit from towers with lower torsional stiffness”. Very interesting results. Could a lower torsional stiffness have negative effects from some other points of view?

- Conclusion, line 308-309: The Authors mentioned only fore-aft tower bending. Is there a reason not to consider the tower side-side? For example, in [Bottasso and Cacciola, Model-independent periodic stability analysis of wind turbines, Wind Energy 2015] and [Allen, Sracic, Chauhan and Hanse, Output-only modal analysis of linear time periodic systems with application to wind turbine simulation data, Mech. Syst. Signal Pr. 2011] the whirl modes are clearly visible from tower side-side (also fore-aft) response.

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