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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Aeroelastic Stability of Idling Wind Turbines" *by* Kai Wang et al.

Anonymous Referee #2

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General comment:

The paper presents a comparison between linearized and nonlinear stability analysis for a 10MW idling wind turbine. The aim of the work is not restricted to the stability analysis, in fact an important part of the paper is devoted to the comparison between results obtained by nonlinear simulation, linear eigenvalue analysis and work computation.

The paper represents a good piece of research, especially if we consider that the topic is not trivial and eventually difficult to be explained and studied. To this end, it is important to notice that there are some points to clarify before the paper could be considered for the final publication. Those points are listed in the "major comments" section.

One of the main issues of this paper is that the system, in the case of stall-induced

vibrations and fully unsteady aerodynamics, is far from being linear. Hence, the concepts of eigenvalues (frequencies and damping factors) must be used with a pinch of salt (see points 4 and 5). Moreover, the relation between this paper and the related TORQUE one is to be clarified in the introduction (1 and 2).

Finally, the choice of time-freezing stability for the analysis of the periodic behavior of the turbine could be better discussed (Point 3).

Consequently, my recommendation is to accept the paper after major modifications.

Major comments:

1. Since the paper is submitted for the "Special Issue – TORQUE 2016" on WES, I think that the related TORQUE paper should be referenced within the text along with a suitable description of the differences/improvements between the two articles. In the current state of the manuscript, the TORQUE paper is not mentioned nor is the innovative content of WES paper with respect to the TORQUE one clearly explained. This said, it is important to stress that the entire section 3 ("description of tools"), although new with respect to TORQUE manuscript, describes a standard multi-body code, hence it cannot be strictly considered as innovative. Moreover, in section 4 (Results and discussion) there are more or less the very same results already inserted into the TORQUE paper. Even though the work is clearly interesting and well done, the authors should better describe its innovative content with respect to their TORQUE publication, especially in terms of methodology/results. The comparison between steady and dynamic aerodynamics can be a good point.

2. As said previously, Section 3 seems to describe a standard multibody solver. If so, probably this section can be removed or better simplified. This can be useful for example to insert in the manuscript a brief description of the ONERA stall

model which may help readers comprehend the obtained results.

3. Pag 3, line 25 to end of page. Clearly, for anisotropic rotors, stability analyses based on the Coleman transformation does not provide exact solutions. But it is important to stress that it keep giving good approximations as some papers underlined (e.g. Skjoldan Hansen, 2009, referenced in the present paper). Here, what the authors have applied is the so-called "Time-freezing stability" (fix the rotor at a specific time instant (or even azimuth angle), compute the linearized model and in turn the frequencies and damping factors). For a generic periodic system, results obtained with this procedure are typically not accurate. I would say that between time freezing-based and Coleman-based analyses, the latter is to be preferred. However, given the very low rotor speed, the time-freezing analysis may give meaningful results, but this is an open point. The authors should clarify this aspect also because, as highlighted in the results, some modes may have negative damping in an azimuthal range and positive in another. In this case, how is the resulting behavior? Damped or undamped?

4. According to the eigenvalue analyses, mode M7 is unstable as it has negative damping for each azimuth angle. From Figure 19, however, it seems that the system is stable, as there is no divergent behavior in the signal. Of course, the vibrational content is amplified because of stall and unsteady aerodynamics, but the system itself is stable. Please, clarify this point.

5. Section 4.3 (Work computation results) is really important for the completeness of the manuscript. I like it a lot for two main reasons. First it gives an alternative way of verifying the stability and second (but probably more important) because, being the wind turbine model with the unsteady aerodynamics fully nonlinear, the computation of the work results to be a methodology more appropriate to study the system stability. To this end, this section can be extended a bit. For example, the vibrations experienced in steady inflow at 42.5m/s and yaw 30 deg (see figure

19) can be imposed to the blade and the work can be computed within a rotor revolution to consider the system periodicity. This seems more significant than imposing the motion obtained from the time-freezing eigenvalue analysis.

Minor comments:

- Pag 4, line 15: How did you compute the eigenvalues displayed in Figure 1?
- Figure 2, 3 and 4: is it possible to plot the underformed configuration with a solid line in order to ease the comprehension of the modal shapes?
- Pag 16, line 9: Why are damping factors and frequency independent of the azimuth? Is the gravity considered in the model? In fact, one may expect different frequency depending of the position of the blade induced by the azimuthal dependency of gravitational loads (i.e. the blade, when upward, is compressed by its own weight, hence its stiffness lowers. The opposite when the blade is downward)
- Figure 24. Is the azimuth angle assumed fixed? Why not using the simulated outputs? (see also point 5 of major comments).
- Pag 25, lines 18, 19: "Blade's 1 motion" should be substituted with "blade 1 motion" or "motion of blade 1"
- Pag 28, line 5: Substitute "Eigen value" with "eigenvalue"

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