Aeroelastic Stability of Idling Wind Turbines by Kai Wang, Vasilis A. Riziotis, Spyros G. Voutsinas.

Comments to each section:

1. Well written introduction to the area.
    a. The 10MW reference turbine was created in a national project called "Light Rotor" in the beginning of the InnWind project where it was also adopted. I suggest that you just omit and maybe refer to DTU Wind Energy instead.
    b. In line 25 page 3, you write that the Coleman transformation approach will not work for periodic systems, where I assume that you refer to the yaw misalignment you mention in the sentence before this one. It could be a little clearer.
2. Good description of the effects of pitching on the mode shapes, and of the velocity triangle of an slowly idling airfoil in yaw and tilt flow at different azimuth angles.
3. In this section, the tools are described. I found the first part about the structural modelling quite lengthy and I am not sure (at least at the time of reading it) if it is needed for the understanding of the aeroelastic stability of an idling turbine. The description of the unsteady aerodynamic models are much more important to my opinion and here only half a page is used.
    a. I suggest extending the aerodynamic part with information that can enable the reader to validate the results, e.g. what time constants are used in the dynamic stall part of the model, what airfoil data is used in the analysis, does the model work well in negative stall? Later in the results section you write "*Certain engineering dynamic stall models (e.g. Beddoes-Leishman) automatically switch to almost steady-state aerodynamics at very high AOA (well beyond CLmax AOA) (see Hansen et al, 2004) while ONERA model is fully deployed at all AOA. Therefore, an analysis using steady state polars is meaningful because it provides the range of anticipated damping predictions among different models.*" This paragraph should be moved to the model section and you should elaborate on the ONERA model "is fully deployed at all AOA. What is meant by this comment? Does it include a valid model for deep stall? It would also strengthen the paper to show selected lift and drag loops for selected stall-induced vibrations.
    b. I also suggest limiting the structural part.
4. Interesting results. I have several comments:
    a. The quality of the plots and captions could be improved. In Figure 9 and Figure 12, the legends are not describing all plotted curves. It is not clear from the caption of Figure 11 what the dots are representing. The low damping of flapwise modes in Figure 13b is very difficult to see.
    b. It is clear that the understanding of the ONERA model is very important and I strongly suggest that the model section is extended.
    c. The very strange looking red lift loops in Figure 25 and 26 seem to indicate a deficiency of the ONERA model. They look strange because they are almost perpendicular to the static curve. I could not find a comment on these strange loops from the authors and I strongly suggest that you explain them based on a better presentation of the ONERA model in the model section.
5. The conclusions does not clearly mention the problem with the linearization of the ONERA model which could be important.

Editorial changes:

1. I suggest to write “nonlinear” instead of “non-linear” and “non linear”.
2. You could consider start a new sub-section on page 5 after talking about the changes in mode shapes when pitching the blades out to feather, which similarly could have its own sub-section starting in line 13 on page 4.
3. Same subsection division could be done for the tools section.
4. I do not like the term “Eigenvalue stability simulations” (line 29 on page 14). Eigenvalues may say something about stability of an equilibrium but they are computed using an eigenvalue solver and not simulated.