Second review of the paper entitled "Differences in damping of edgewise whirl modes operating an upwind turbine in a downwind configuration"

The manuscript, in this new version, improved as I expected. In particular, the new introduced parts and figures related to the excitation method are exactly what I expected to see to demonstrate the goodness of the proposed approach. Moreover, the demonstration that the employed perturbation can separately excite the whirling modes represent a good value for the paper (at least in my opinion).

Minor technical comments:

- By setting gravity to zero, the variation of stiffness is neglected: The Authors wrote in the reply "Further analysis of this effect could be done with Floquet-analysis, but this is outside the scope of this work.". I fully agree with this. Please, consider the possibility to add somewhere in the text this point.

- The procedure for the phase consistency check is still a bit confusing. Probably, I haven't understood the need for this procedure. In fact, if one records different signals in the very same instants of time (with the same time step and the same known initial time), the phases computed by the Fourier Transformation results to be consistent. In a simulation environment, this can be easily done. In a real environment, probably, the phase consistency is an issue to solve when different sensors with different sampling time are employed. Is this procedure borrowed from real field applications, even though in silico it is not strictly necessary?

- Check sentence: "Further investigations should evaluate the influence of the upper especially on the mode shapes of the whirl modes".