Referee report WES-2020-29

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1 Summary

The authors present an aeroelastic simulation of a brimmed diffuser on a small wind turbine (3 kW). The brimmed diffuser is referred to as a wind lens. In some circumstances the brimmed wind turbine is known to be subject to vibrations. The authors attribute this to the excitation of eigenmodes by vortices shed periodically from the brim and perform an aeroelastic analysis of the system. First, modal analysis is performed based on a finite-element simulation. Then, low Re CFD simulations are performed on the wind lens and the vortex shedding frequency (a function of wind speed) is compared to the eigenfrequencies of the system and the aeroelastic response is investigated.

2 Interest

The paper addresses a real but rather specific problem, with limited general interest. No attempt is made to generalise the conclusions to other similar systems.

3 Low Reynolds approach

The authors have chosen to perform their CFD analysis at Re = 288 (by scaling the viscosity) rather than the Re = 3e5 corresponding to the real operating conditions. The reason given is "to diminish the instability of turbulence". The authors should explain exactly what they mean by this. The authors do not explain why they believe it is warranted to run the simulations at such a low Reynolds number, apart from a cursory reference to the original and viscosity-scaled models having the same mass ratio. This is a major shortcoming of the manuscript, leading me not to recommend publication in its present form. I see at least the following issues:

- Can one assume that the Strouhal number is roughly the same at Re = 288 and Re = 3e5? It appears to me that this assumption is implied. The discussion in 4.3, though certainly useful, does not demonstrate that this assumption is justified.
- At 3e5, the vortex shedding is expected to be 3D. How can this not affect the excitation?

This should be addressed. Calculations at higher Re should be seriously considered.

4 Rotor

The rotor appears to be completely absent from the calculations. This is obviously computationally expedient but possibly questionable. The rotor will at the very least cause the wind speed at the rotor to be lower than the incoming wind. How was this taken into account? Are tip vortices from the blades expected to interact with the vortices shed by the brim of the diffuser? I assume it was verified that the rotor cannot excite the diffuser, but it should be mentioned.

The authors should mention explicitly that the rotor was left out, why they believe this can be done or had to be done and why a low-fidelity approach (actuator disk and related models) was not considered. I understand that a full rotor simulation is outside the scope of the manuscript.

5 Modal analysis

The value of the paper could be improved with experimental or operational modal analysis, as the authors appear to have access to a physical setup. This is not difficult to do and would provide a very useful validation of the numerical modal analysis.

The authors should seriously consider experimental or operational modal analysis.

6 Estimation of the critical wind speeds

I disagree with the statement that the presence of harmonics points to the presence of three separate vortex modes. Harmonics will appear in the FFT from the moment the variation the signal is not a perfectly sinusoidal function. A single mode is therefore perfectly possible. There also appears to be little basis for the statement the first purported mode is primarily related to lift and the others to drag. Every vortex shedding mode will have a signature in both the lift and drag forces, even though obviously the period of drag variations is only half the period of the lift variations. To avoid misunderstanding: I do agree of course that the harmonics play a role in the interaction with the eigenfrequencies.

This should be corrected. If the authors believe I am mistaken, they are welcome to demonstrate the presence of truly separate modes.

7 Lock-in

The lock-in phenomenon deserves a more thorough discussion. Its identification in fig. 16 is not convincing. For a formal definition of lock-in, the authors may refer to Kumar, Navrose, and Mittal, Physics of Fluids 28 (2016) [doi: 10.1063/1.4967729] .

I recommend that the authors argue the presence of lock-in better.

8 Writing and figures

The writing is clear and almost without grammatical errors. The figures are clear.

9 Minor comments

- The authors use a compressible solver in the CFD. Why do they expect compressibility to be important?
- Two letter symbols such as St and Re should not be italic