Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2020-29-RC2, 2020

Interactive comment on "Fundamental effect of vibrational mode on vortex-induced vibration in a brimmed diffuser for a wind turbine" *by* Taeyoung Kim et al.

Anonymous Referee #2

Received and published: 9 June 2020

The paper proposes an interesting study on the aero-elastic response of a wind lens, used as a diffuser for a ducted horizontal-axis wind turbine. The topic is relevant for the journal and considers an interesting topic for researchers and technicians involved in the design of ducted wind turbines. Moreover, the study involves a combined aero-structural time-resolved analysis of a realistic wind lens, proposing an approach that might be used in future studies for the design of such devices, that have the potential for improving the wind turbine energy harvesting. The paper is well structured, well written, and the figures are easily readable and of good quality (the introduction might be probably written in a more concise and effective way).

Despite these general positive aspects of the paper, the referee is however surprised by the way in which the authors carried out the CFD analyses reported in the paper, which introduces a very relevant issue in the paper quality and technical relevance. The major points of discussion on this aspect are reported below:

1 Authors decided to focus on 2D simulations, which is reasonable due to the high computational cost of aero-structural simulations, however more specific justification on the technical relevance of this choice is required.

2. Authors make use of a compressible-flow model. Since the Mach number should be well below 0.3, this choice does not seem justified and might even lead to numerical issues if proper preconditioners are not used. Please clarify in the text.

3. The authors use a deforming mesh strategy to account for the deformation of the structure during motion; some information on the numerical technique used for deforming the mesh should be reported (at least with a citation). Did the Authors experience any issue with cell quality due to mesh deformation during the simulations?

4. No model of the turbine within the wind lens is considered, at least a comment on this aspect should be reported.

4. Authors decided to reduce by three orders of magnitude the Reynolds number of the problem, altering the flow regime from turbulent (the actual one) to laminar. Authors state that similarity in aero-elastic response is confirmed despite the alteration of the Reynolds number, but they do not explained how and why. As well known, turbulence is a non-linear phenomenon which does not only feature unsteadiness and instability, but which alters the gross properties of the flow and, hence, the values of lift and drag coefficients, as well as the vortex shedding frequency. Why do the Authors believe that such an arbitrary change in flow regime does not lead to unrealistic results?

This last aspect is by far the most critical of the paper. As well highlighted by Figure 11, the aerodynamic forcing acting on the wind lens is determined by the detachment of vortices connected to flow separations in different areas of the wind lens profile. Such a phenomenon is dominated by viscous/turbulent effect and there is no proof that the shedding frequencies, which ultimately allow constructing the Campbell diagram in Figure 12, are estimated in a realistic way by the CFD model. Authors are clearly aware of this and discuss the issue in page 19, without however giving a convincing explanation of the validity of their approach. In this context, it is not even clear the motivation for such a severe simplification: with present-day CFD technology it is possible to simulate highly turbulent flows with U-RANS, employing suitable turbulence models (such Spalart-Allmaras or k-w SST models). Such models might be not entirely reliable when dealing with separated flows and, in general, aerodynamic instabilities, however they were shown to predict correctly the Strouhal number of vortex shedding; this referee believes that the uncertainty is introduced by altering the flow regime from turbulent to laminar is much higher than that introduced by the use of a turbulence model.

As a final consideration, this referee believes that this paper documents a general aero-structural methodology which is scientifically interesting, but featuring a very critical technical issue on the aerodynamic side which ultimately reduces the technical relevance of the entire work. Before the paper is considered for publication, a revision is required in which the Authors justify better their choices and demonstrate, with at least a U-RANS simulation with a rigid wind lens, that the vortex shedding frequencies for the realistic Re of 300000 are nearly the same as those predicted for Re = 288.

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