Review of the manuscript:

Ricardo Fernandez-Aldama et al.:

“Characterization of vortex shedding regimes and lock-in response of a wind turbine airfoil with two high-fidelity simulation approaches"

General comments:

In this work 2D URANS calculations on a wind turbine airfoil and 3D DDES simulations on the extruded section at cross flow conditions are performed and analyzed. In addition to calculations around the static airfoil, simulations are performed for the elastically clamped section where motions in chord-wise direction are permitted. In the coupled simulations, the flow velocity is varied in the area of the lock-in and the resulting characteristics of the two simulation approaches are compared. The lock-in area is characterized using a new parameter proposed by the authors which is based on the amplitude growth rate. By adapting the choice of a reference frequency for the DDES simulations, it was possible to find qualitatively well-matching lock-in curves for both numerical approaches. The results are analyzed in a sound manner and largely placed in the context of results from other studies, providing relevant insights into ViV for the community.

To me this is in general a nice work and I enjoyed reading the manuscript. However, in my opinion the DDES mesh could have been better adapted to the present task and I consider the resulting spectrum (for the static case) with several closely spaced peaks of almost equal amplitude unusual (see below) and in need of explanation. At this specific point, I am missing flow-physical analyses to explain the result. If the authors can investigate and explain this further, I would be happy to support publication of the manuscript. I have a few more comments and questions and would be pleased if the authors could take them into account.

Specific comments and remarks:

- Introduction: At the beginning of the introduction, I suggest to briefly mention the lock-in mechanism before giving characteristic parameters of the lock-in.
- P. 2, line 33: What is meant by "complex geometries"? Complete rotor blades?
- P. 2, line 40ff: This is a fairly short overview on ViV studies on the cylinder with largely older citations. I miss here more recent work, e.g. by F. Lupi. The descriptions could be a little more detailed and contain the essential findings of the work.
- P. 3, line 63: Can you please describe the term "galloping" a little?
- P. 3, line 86-89: I would shift the sentence "Despite..." to the previous paragraph and insert a paragraph break before "The parameters...".
- P. 4, line 109ff: Here a brief overview on the structure of the manuscript should be given rather than a summary of findings.
- P. 6: The mesh topology and discretization for an airfoil at attached flow conditions is chosen although it is examined at cross-flow direction. In the case of cross-flow, Karman vortex streets detach cyclically from the leading and trailing edge, respectively. The vortex sheets are likely modeled in LES mode and I would expect refinements and quasi-isotropic grid cells in the domains of the vortex sheets. Did the authors also investigate such task-adapted meshes and assess the resolution in the LES domain and why did they opt for conventional mesh topology?
- P. 6: Were the CFD calculations carried out for fully turbulent boundary layers?
- P. 7, line 169ff: Structural data was taken from the paper by Skrzypinsky et al. and it is stated that the data is representative of a "realistic wind turbine blade". Could you please give some more information for which blade size the structural data is representative.

- P. 7, line 174: What is meant with “civil structures”?
- Fig. 5: The spectra of the lift coefficient for 2D URANS and 3D DDES look qualitatively and quantitatively completely different. As expected, more flow structures are obviously resolved by the DDES, which leads to several peaks in the spectrum. However, contrary to other publications of numerical or experimental studies on airfoils at cross flow conditions, these have almost the same amplitude and are close together and. In other publications, including the paper by Skrypinsky et al. cited by the authors, the result shows a clearly dominant peak. I therefore cannot understand the present results without further analyses and explanations by the authors. Here I miss an analysis of the flow field (e.g. using DMD), which explains the characteristics of the spectrum resulting from the DDES. In addition, this result was only placed in the context of studies on the flow about a cylinder. A comparative discussion with published results for airfoils at cross-flow would be nice.
- P. 14, line 330ff: With reference to Table 1, it is stated "...that the reported St values become larger as the aspect ratio increases...". However, Table 1 only lists results for 2D and for AR=1, so that no statements can be derived about the impact of the Aspect Ratio, unless the table contains errors in the given values.
- P. 8, line 179ff: The sentence "The present ... properties." is quite unspecific. What are "sectional engineering models"? To what extent can the results of the present work be used for such models and what modifications would have to be made?
- P. 15, Sec. 3.2.1: To evaluate the ViV behaviour and the lock-in, the eigen frequency of the structure is normalized with the vortex shedding frequency of the static case. The DDES results in several peaks in the spectrum (Fig. 5 and discussion above). The authors have chosen St=0.132 for the normalization, which provides good agreement of the lock-in curves with the results of the 2D URANS. A peak in the spectrum (Fig. 5) is indedd recognizable at St=0.132, but the amplitude is larger for a peak at a higher frequency. Without further analysis of the flow fields in the static case and without further justification, the choice of St=0.132 appears somewhat arbitrary.
- Fig. 9: Fig. 9 shows the amplitude growth rate for a 2D URANS result. I would be interested to see what the curve looks like for a 3D DDES and whether the growth rate can be determined with similar clearness.
- Conclusion: The lock-in curves for 2D URANS and 3D DDES show a surprisingly good agreement for the present choice of the DDES reference Strouhal number (see above). The authors therefore conclude that much cheaper 2D URANS could be used to characterize ViV. However, the lift spectra of 2D URANS and 3D DDES for the flow around the static airfoil differ regardless of the fact that the 3D DDES provides several peaks. Therefore, assuming that the DDES results are closer to reality, the 2D URANS results would have to be corrected to infer absolute frequencies or lock-in velocities. Do the authors have any idea how this correction should be done and do they have any idea whether this correction can be universal?