Authors’ response to Anonymous Referee #1

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

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General comments with answer

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.

> The present authors really appreciate the referee’s comments and in-depth review. We have strived to take into account all of the comments made. This has resulted in significant additions in the revised manuscript, which greatly improve the quality of the present work.

Specific comments and remarks with answers

– Introduction: At the beginning of the introduction, I suggest to briefly mention the lock-in mechanism before giving characteristic parameters of the lock-in.

The authors agree with and appreciate the referee's suggestion. In the manuscript, the definition of the lock-in mechanism was mixed with the description of its characteristic parameter, the lock-in range. This made the sentence somewhat cluttered and confusing. To improve this, as suggested, a new sentence has been added to the revised manuscript before the aforementioned description briefly introducing the VIV and lock-in phenomena (lines 26–28 of the revised manuscript with tracked changes).

– P. 2, line 33: What is meant by "complex geometries"? Complete rotor blades?

By complex geometries, the authors meant geometries in general which require meshes with a very high number of elements in order to be simulated effectively with CFD. This includes for example three-dimensional geometries without exploitable symmetries, such as wind turbine blades, or geometries with small details that induce large gradients in the flow field, such as towers equipped with strakes. The sentence was not clear and has been rephrased in the revised manuscript (lines 40–41).

– 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.

We agree with the referee. The literature review has been expanded where deemed appropriate to give more details and describe the essential findings of the cited works. Recent contributions by Lupi et al. have been added. See lines 49–56.

– P. 3, line 63: Can you please describe the term "galloping" a little?

The following description of galloping has been added to the revised manuscript (lines 78–80): "[. . .] where galloping refers to the canonical cross-flow aeroelastic instability (not related to vortex shedding), and low-speed galloping being a special case of galloping occurring at inflow wind speeds below the VIV critical velocity and potentially interacting with it".

– P. 3, line 86-89: I would shift the sentence "Despite..." to the previous paragraph and insert a paragraph break before "The parameters...".

We agree with the referee. Moving these sentences improves the flow of the text. This has been changed in the revised manuscript, we thank the referee for the suggestion.

– P. 4, line 109ff: Here a brief overview on the structure of the manuscript should be given rather than a summary of findings.

The revised manuscript has been updated as requested. This change improves the overall readability of the text, for which we thank the referee. See page 5.

– 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?

As the referee points out, it would be desirable to have a mesh refinement leading to quasi-isotropic cells all along the wake. This is sometimes done in vortex shedding and VIV simulations, via multiblock or unstructured meshing techniques, but only for low Reynolds number flows, as the cell count can quickly explode. Such wake-refined meshes have been used e.g. by Vlastos et al. (2024) for a Reynolds number of 2.6×10^4, or by Lee et al. (2014) for a Reynolds number of 5000.

Typically, meshes for vortex shedding and VIV at high Reynolds numbers focus on having a good refinement in the boundary layer to accurately capture the fluctuating separation point, and also on the near wake behind the body to capture the vortex formation and shedding. When using hybrid RANS-LES modelling (like the DDES model in the present work), the resulting meshes are typical RANS meshes in the boundary layer (as for an attached flow condition), and typical LES quasi-isotropic cells in the near wake only. This kind of mesh has been used before e.g. by Skrzypiński et al. (2014); Lian et al. (2023); Ebstrup et al. (2024); Ludlam et al. (2024) and Pirrung et al. (2024). The mesh in the present work has an aspect ratio under 3 in the near wake (up to one chord length downstream).

Having a low resolution in the far wake leads to only the largest eddies being resolved, and the smaller ones being modelled, and ultimately to even the large eddies being dissipated. Nevertheless, it is not expected that this will significantly affect the vortex shedding and VIV mechanisms, as long as the region in which recirculation may reach the airfoil (near wake) is well captured, and the transition between the near and far wake provides correct large-scale dynamics (Spalart, 2001).

Multiblock or unstructured meshes would probably be preferable in this case, either to improve accuracy or to reduce computational cost. However, in the present work it was decided to employ a similar mesh to those from closely related publications (Skrzypiński et al., 2014; Lian et al., 2023) for comparability and simplicity. The mesh is refined as much as possible at the near wake considering the computational cost of the simulations.

A comment in this line has been added to the manuscript, to better justify the mesh topology and discretization decision (page 8, lines 213–216).

– P. 6: Were the CFD calculations carried out for fully turbulent boundary layers?

Yes. This information has been added to the revised manuscript (page 6, lines 188–189).

– 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.

This is certainly an interesting question which was not addressed in the manuscript. The only structural parameters involved in the VIV mechanism, after appropriate nondimensionalization, are the mass ratio, $\tilde{m}$, the structural damping value, which in this work is set to zero, and the ratio between the structural natural frequency and vortex shedding frequency, which in this work is an independent variable. Thus, only the mass ratio needs to be representative of a wind turbine blade.

The mass ratio for a wind turbine blade section is defined as $\tilde{m} = m/(\rho_\infty c^2)$, where m is the mass per unit length and c is the chord. Thus, this non-dimensional parameter is dependent upon a combination of blade size and structural design. Therefore, there is no direct relationship between mass ratio and blade size.

The mass ratio is an important parameter in VIV, as it may take a wide range of values , ranging from order 1 or 10 for underwater flexible bodies, e.g. $\tilde{m} \approx 2$ for an offshore riser (Mukundan et al., 2009), to order 100 for some structures in air, e.g. $\tilde{m} \approx 300$ for the NREL-5MW wind turbine tower (Jonkman et al., 2009), as outlined by Jauvtis and Williamson (2003).

For wind turbine blades, the present authors estimate the mass ratio for the NREL-5MW (Jonkman et al., 2009) and DTU-10MW (Bak et al., 2013) blades to be about $\tilde{m} \approx 15$, for the NREL-UAE blade (Hand et al., 2001) it is $\tilde{m} \approx 30$, and for the IEA-22-280-RWT blade (Zahle et al., 2024) it is in the range $\tilde{m} \in [12, 23]$. The present value of $\tilde{m} = 32.7$ is closer to that of the shorter NREL-UAE blade, and of the same order as those of the other aforementioned blades.

The pertinent paragraph has been rephrased in the manuscript to better reflect the representativeness of the present structural values for wind turbine blades (page 9, line 235).

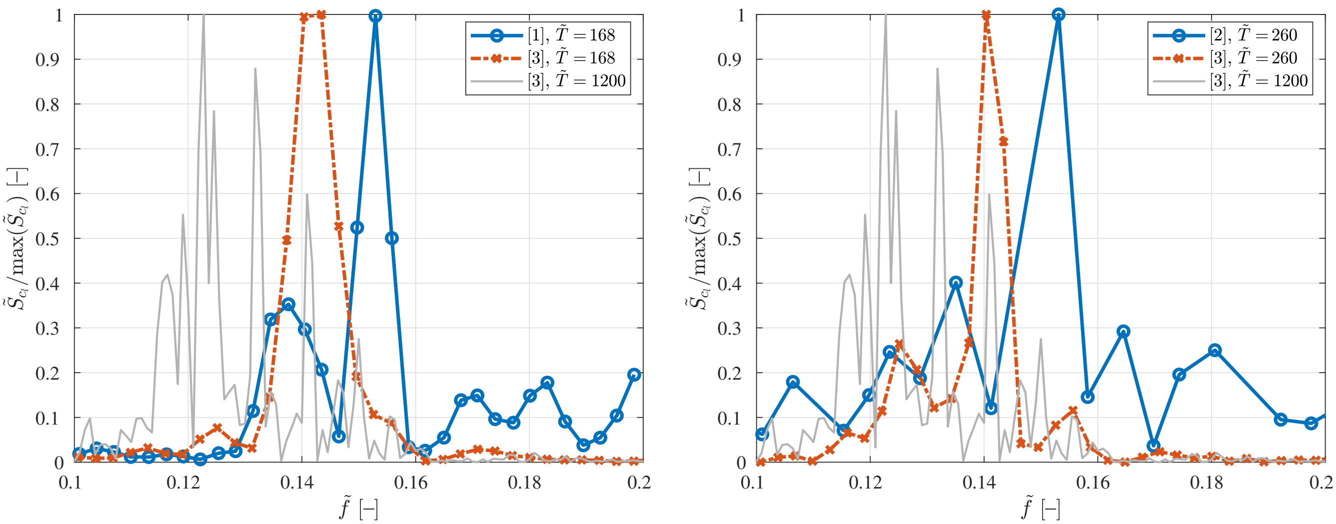
– P. 7, line 174: What is meant with “civil structures”?

The authors were referring to civil engineering structures in general where wind loads are design drivers, and that can be assumed to have comparable mass ratios, as opposed to underwater structures or very flexible structures in air such as transmission lines. The term was confusing and did not add much value, so the sentence in which it appeared has been rephrased (page 9, lines 234–235, also as per the referee’s previous comment).

– 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.

The referee raises a fair point, as the results presented in the previous literature on this same problem, namely Skrzypiński et al. (2014) and Lian et al. (2023), were described but not compared directly and comprehensively to the results by the present authors.

A comparison is shown in Fig. 1 between the lift coefficient spectra as presented in Skrzypiński et al. (2014) and Lian et al. (2023) and the spectrum from the present results. The frequency axis of the data from Lian et al. (2023) has been made non-dimensional using $c = 1$ m and $U = 30$ m/s ($\mathrm{Re} = 2 \times 10^6$). Two spectra from the present results are shown using two different time series lengths; one series length corresponds to the whole simulated time series ($\tilde{T} = 1200$), and the other series length matches approximately that used for the aforementioned published PSDs. The magnitude of all the spectra are normalized with respect to their maximum values, as done in Skrzypiński et al. (2014), to make the comparison clearer. All simulations correspond to the same static airfoil, under the same flow conditions and using some form of Detached Eddy Simulation strategy. The present work spectra are estimated using a periodogram with a Hamming window, as stated in Skrzypiński et al. (2014) (no information on the PSD estimation procedure is given in Lian et al. (2023)).



(a) [1]: Skrzypiński et al. (2014), [3]: Present work

(b) [2]: Lian et al. (2023), [3]: Present work

Figure 1. Normalized PSD of the lift coefficient fluctuations obtained using two different time series lengths, and compared with literature results. Static airfoil DDES-3D case at Reynolds number $\mathrm{Re} = 2 \times 10^6$.

In both Fig. 1(a) and Fig. 1(b) it can be seen that the PSDs presented in the literature do have a few peaks at a wide range of frequencies, where the second largest peaks have an amplitude of about 0.4 times that of the largest peaks. The PSDs obtained with the limited time series from the present work also show a clear dominant peak, at a non-dimensional frequency $\tilde{f} \approx 0.142$ as seen in both panels. A second peak appears at about $\tilde{f} = 0.127$ for the $\tilde{T} = 260$ case ((b)).

The sharp peaks obtained for the short time series are in contrast with the PSD obtained with the full time series. The present authors argue in the manuscript that the multiplicity of peaks is due to the vortex shedding behaviour changing intermittently over time. This is supported by the scalogram presented in Fig. 7 of the originally submitted manuscript. Thus, for short time series less peaks will be captured, and the authors see no significance in which of the peaks inside the relevant range of frequencies is higher, as the frequency of the highest peak changes with time, as shown in Fig. 8 of the originally submitted manuscript.

On the other hand, the authors acknowledge that we cannot know for sure whether this multiplicity of peaks would have appeared if the simulations from Skrzypiński et al. (2014) or Lian et al. (2023) had been longer. And there seems to be a general shift in the frequencies obtained in the present work towards lower values, when compared to the previous literature, which remains unexplained.

Regarding the DMD analysis, unfortunately, the interval between the saved snapshots of the whole flow field results is too large to extract useful temporal modes of the flow field. Also, as the peaks in the PSD relate to different time intervals of the simulation, the authors are not confident about whether a standard DMD analysis would shed more light on the present phenomenon, requiring perhaps more advanced techniques such as multiresolution DMD or empirical mode decomposition (de Souza et al., 2024). This is currently out of scope, but may be explored in the future. We appreciate the suggestion, and this possibility has been added to the manuscript (page 29, lines 647–650).

– 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.

The referee is absolutely right, and we thank the referee for the detailed reading of the manuscript and the effort to make sense of it. That sentence is a leftover from a previous version where Table 1 included additional data of URANS simulations at different Aspect Ratios. The sentence does not add value to the discussion and has thus been deleted in the revised manuscript (page 18, lines 443–444).

– 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?

We agree with the referee and in the revised manuscript this paragraph has been modified to be more clear and specific (page 9, lines 242–244).

By “sectional engineering models”, the present authors refer to the wide range of engineering models developed to predict the VIV response of bluff bodies, for 2D or lumped-element models. These models form the basis for three-dimensional models accounting for different aspects of the spanwise inhomogeneities. Classical examples of VIV engineering models are the spectral model with non-linear aerodynamic damping of Vickery and Basu (1983), and the wake oscillator model of Tamura and Matsui (1980).

The parameter identification and proposal of modifications to these models for the airfoil case, based on the present work results, is a work in progress by the present authors. Some examples of the use of simulations or experimental data to inform such models can be found in Meskell and Pellegrino (2019) and Rigo et al. (2022).

Typical modifications of 2D or lumped-element models to account for 3D phenomena include (but are not limited to) enforcing a load coherence function in spectral models (Vickery and Basu, 1983), or using a continuous distribution of diffusively coupled wake oscillators (Balasubramanian and Skop, 1996).

– 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 indeed 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.

We fully agree with the referee that, as it is currently written in the manuscript, the choice of $\mathrm{St} = 0.132$ can be deemed arbitrary. The rationale given for that choice is that the lock-in curves of the URANS-2D and DDES-3D cases should agree. This is supported by the assumption that URANS-2D is capable of capturing the VIV phenomenon when the motion amplitude is large and growing, as this results in a largely spanwise coherent shedding. Thus, the chosen Strouhal was not meant to characterize the static case vortex shedding frequency, but rather to serve as a parameter which defines the critical wind speed $U_{\mathrm{crit}} = f_s c/\mathrm{St}_{\mathrm{crit}}$ and therefore the lock-in range. It could be thought of as a "critical" Strouhal number, "critical" in the sense that it defines the critical wind speed.

Regardless of this point, the PSD of the lift coefficient fluctuations gives us another argument to support the 0.132 Strouhal as a value with a significant role in the vortex shedding process. In Fig. 2, this PSD is shown, together with a Gaussian function fitted to it. It can be observed that the central frequency of the Gaussian fit, $\tilde{f}_{\mathrm{Gauss}} = 0.131$, is really close to the chosen Strouhal value of 0.132. In panel (a) of Fig. 2, the Gaussian fit does not seem very appropriate, but changing the y-axis to a logarithmic scale, as done in panel (b), shows that there is an underlying distribution of the spectral content, which is well represented by the Gaussian fit. This can be identified even better in panel (c), where a wider range of frequencies is shown in log-log axes; the PSD of the lift coefficient increases significantly for a limited range of frequencies around the chosen Strouhal frequency. The present results alongside the discussion in the manuscript indicate that the central frequency of this narrowband process is more significant than the frequency of the highest peak.

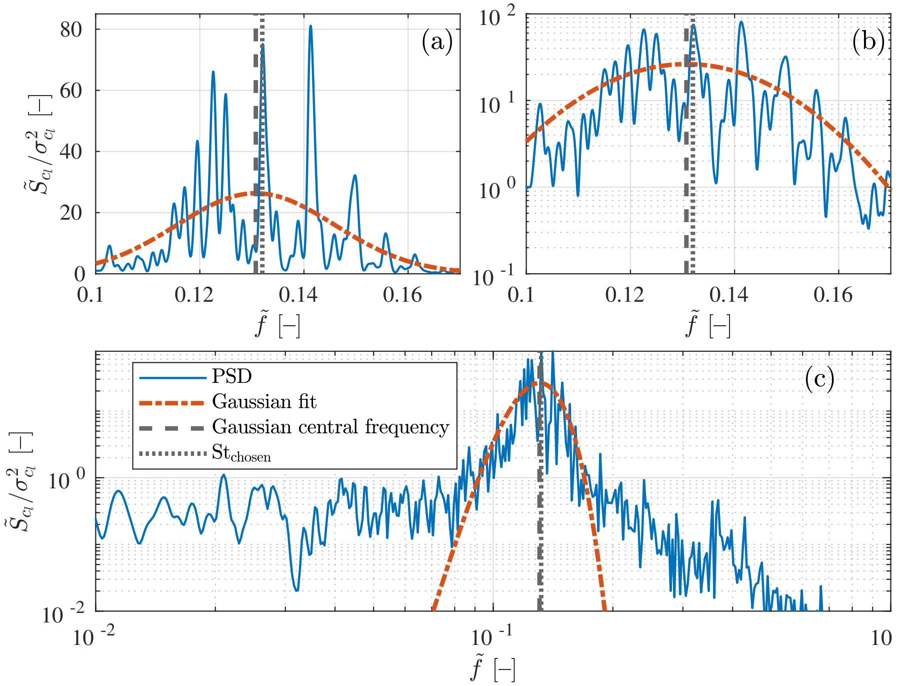


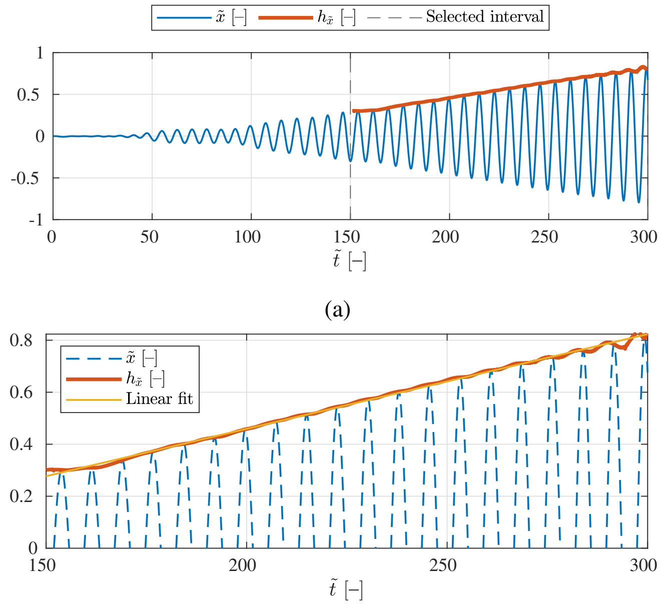
Figure 2. Different representations of the normalized PSD of the lift coefficient fluctuations, (a) in linear axes, (b) in logarithmic y-axis, and (c) in log-log axes and for a wider range of frequencies. Also shown in all panels are the Gaussian fit to the PSD and, as vertical lines, the central frequency of said Gaussian and the Strouhal number chosen for the DDES-3D nondimensionalization, $\mathrm{St}_{\mathrm{chosen}}$. Static airfoil DDES-3D case at Reynolds number $\mathrm{Re} = 2 \times 10^6$.

Approximating the vortex shedding spectral content to a Gaussian function has been done since early on by Vickery and Clark (1972), and is the basis of the spectral engineering models family for the prediction of VIV (Vickery and Basu, 1983; Arunachalam and Lakshmanan, 2015). The Gaussian approximation has been also used to identify the Strouhal number from full-scale experimental data by Kurniawati et al. (2024). In all these cases the Strouhal number is defined as the central frequency of the Gaussian approximation.

This analysis of the lift coefficient frequency content based on the Gaussian fit has been added to the revised manuscript to justify the choice of Strouhal number. See Figure 10, and pages 18–20.

– 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.

The referee raises an important matter for this work. The curve for a DDES-3D case is shown here in Figure 3. It is observed that the response is as clear as for the URANS-2D case, and that the linear fit is good. This figure has been added to the revised manuscript, as it is of great help to comprehend the results. See Figure 12, and pages 22–23.



(a)

(b)

Figure 3. Illustration of the amplitude growth rate calculation procedure. (a) Time series of the non-dimensional edgewise displacement, $\tilde{x}$, along with, in a thicker line, the envelope of the time series calculated with the Hilbert transform, $h_{\tilde{x}}$. Additionally, the limits of the time interval selected for the linear fit to $h_{\tilde{x}}$ are marked with vertical dashed lines. (b) Time series of the non-dimensional edgewise displacement $\tilde{x}$ within the time interval selected for the linear fit, along with the estimated amplitude $h_{\tilde{x}}$, and the straight line fitted to $h_{\tilde{x}}$ in this portion of the envelope. The amplitude growth rate is defined here as the slope of the fitted line. All results are for a DDES-3D FSI simulation at $f_{\mathrm{St,D}}/f_{\mathrm{s}} = 1.02$.

Additionally, in the revised manuscript it has been stressed that the growth rate is well approximated by a straight line for both URANS-2D and DDES-3D simulations, as it was not completely clear before. We thank the authors for raising this interesting issue which we think improves the quality of the work.

– 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?

We acknowledge that the point raised by the referee is an excellent one.

The different "critical" Strouhal numbers obtained for URANS-2D and DDES-3D cases show that some correction would be needed in order to predict the lock-in velocity range of an actual airfoil.

It may be safe to assume that DDES-3D results are closer to reality than URANS-2D results, but the Strouhal number may still depend on the aspect ratio of the simulated airfoil (Skrzypiński et al., 2014). Thus, a reliable calibration for a wind turbine blade would have to be performed using higher-fidelity data, such as higher aspect ratio simulations (which may be computationally prohibitive) or experimental data.

Due to the inherent complexity of the vortex shedding and VIV phenomena, it is difficult to venture generalizations from a limited set of observations. Nonetheless, the present authors find the results in the manuscript to be significant, and we are still exploring them in more depth.

If we were to define a correction or calibration of the Strouhal number, we propose two ways. One is simply to obtain the Strouhal number from the high-fidelity case (from long-enough time series, and possibly using the Gaussian approximation presented as an answer to the referee's previous question). The other proposal is to obtain the initial amplitude growth rate for a few wind speeds in order to determine the lock-in range, and compare this range to that obtained with the lower-fidelity results. Both of these approaches are based on the assumption that the initial VIV responses obtained with the high and low-fidelity cases are comparable except for their "critical" Strouhal number. This is a strong assumption, supported by the simulation results presented in the manuscript, but the authors are cautious about its validity outside the present case study. The authors are working on substantiating this validity, as the savings in computational cost can be significant, e.g. for the development of VIV engineering models based on URANS-2D simulations.

Obtaining a universal correction does not seem immediate to the present authors. Possibly, a specific correction would be needed for each combination of airfoil geometry and Reynolds number, if their change is significant.

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Authors' response to Anonymous Referee #2

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

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General comment with answer

The topic and the activity are very interesting and worthy of investigation. The authors performed a complete and in-depth analysis and the results are very well presented with many details.

The reviewer has few major concerns regarding the numerical methodology and the analysis of the results.

The authors thank the referee for the concise, relevant and valuable comments made.

In the following answers, we hope to adequately address the referee's concerns. In doing so, several improvements have been made to the revised manuscript.

Specific comments with answers

– The radius of the domain seems to be small, being just 30 times the airfoil chord. It was demonstrated that a 2D analysis needs more than 100 chords (preferably, 500 chords) to avoid any blockage effects (https://turbmodels.larc.nasa.gov/naca0012_val.html). How do the authors justify this choice?

The referee is absolutely right in pointing out that the computational domain should be as big as possible to reduce the influence of the freestream boundary conditions on the results.

The authors have checked setting the domain to 50 chords and 100 chords (with the consequent increase in mesh radial cell count) and have found no evident differences on the loads or VIV response. In the present implementation, the solver employs the method of Riemann invariants at the far field so that the freestream conditions are more accurately prescribed.

Having a domain size of about 30 chords is considered standard practice both for lifting bodies (Golmirzaee and Wood, 2024) and the authors have found it being widely used also for bluff body simulations (e.g. Skrzypiński et al. (2014); Martín-Alcántara et al. (2023)), even though some effect of the boundary conditions will be present.

Regarding the blockage, this domain size has been checked to induce a blockage ratio under 2%, which is considered small enough (West and Apelt, 1982).

So, all in all, the justification for the choice of the domain size derives from the usual compromise between accuracy and computational cost; where the value of 30 chords is deemed appropriate, and is comparable to those in the works by Skrzypiński et al. (2014) and Lian et al. (2023), which serve largely as a basis for the present work.

Some of these additional details have been added to the revised manuscript to better justify this choice. See page 7, lines 198–200 of the revised manuscript with tracked changes.

– How was the 1c spanwise height of the domain chosen?

This a very relevant question which was not properly addressed in the manuscript.

The spanwise length of one chord was chosen for two reasons: (1) to have a direct comparison with the previously published results by Skrzypiński et al. (2014) and Lian et al. (2023), and (2) because it is large enough for the three-dimensional vortical structures to develop, all while allowing for a fine spanwise discretization at a reasonable computational cost. The authors are working on an extension of the present work to larger aspect ratios, as a means of bridging the existing knowledge gap in the VIV behaviour of airfoils and full wind turbine blades.

A new paragraph has been added to the revised manuscript to justify the choice of spanwise length. We thank the referee for pointing out this omission. See page 6, lines 168–171.

– URANS-2D FSI simulations are run for 1300 non-dimensional time units each, while the DDES-3D simulations are run for 300 non-dimensional time units. How do the authors can demonstrate that the length simulated intervals are sufficient for capturing all the relevant features and the collected data are statistically significant?

The authors are grateful to the referee for pointing this issue out, as it is essential that it is clearly addressed in the manuscript.

It certainly would have been desirable to run the DDES-3D free to move airfoil simulations for longer times, so as to obtain converged or cyclostationary results, but this was too computationally expensive.

The scope of the manuscript is thus limited to the assessment of the differences and similarities between the cheap URANS-2D simulations and its DDES-3D counterpart only for the initial amplitude growth transient. Longer DDES-3D simulations would be required to extend the analysis to other relevant features of VIV such as the maximum amplitude, as stated in the conclusions.

All of the comparisons in the manuscript between the URANS-2D and DDES-3D VIV results are based on the same time interval $\tilde{t} \in [150, 300]$, which is contained in the growth transient in all lock-in cases.

Regarding this growth transient, 300 non-dimensional time units are enough to capture a significant portion of it; it is a long-enough time to avoid the initial vortex-formation transient and to have a consistent characterization of the aerodynamic forces. It is this limitation in the simulated time for the DDES-3D simulations, typical for this kind of simulations ((Skrzypiński et al., 2014; Lian et al., 2023)), which motivates the novel use of the growth rate as a means to characterize the VIV lock-in region. The 300 non-dimensional time units are found to be sufficient to robustly characterize the growth rate.

A new figure has been added to the manuscript, presenting the DDES-3D airfoil displacement for these 300 non-dimensional time units to better demonstrate the converged behaviour of the response (within the displacement growth transient). The authors are grateful to the referee for pointing this out, as the manuscript was not clear enough. The inclusion of this new figure has been also been motivated by a comment by another of the referees, as a means to show the clearness of the linear fit used to obtain the growth rate. See Figure 12, and pages 22–23.

– It is not clear which is the physical phenomena associated to the St=0.132 that leads to comparable range of lock-in between the two approaches

The referee raises a very interesting and relevant point. The present authors are not certain of the physical mechanism which leads to a lock-in behaviour given the multiplicity of shedding frequencies found in the DDES-3D case.

A sensible hypothesis for this result is that the shedding frequency which determines the lock-in behaviour corresponds to the central frequency of the narrowband vortex shedding process. This narrowband process encompasses all of the multiple peaks shown in the previously presented PSD, and can only be ascertained when plotting said PSD in a log-scale. This central frequency happens to be really close to the chosen value $\mathrm{St} = 0.132$.

Another referee also posed a question along the line of this comment, in response to which a new analysis of the lift coefficient frequency content, based on fitting its PSD to a Gaussian curve, has been added to the revised manuscript. This new analysis may help in the understanding of the physical relevance of the chosen Strouhal number. See Figure 10, and pages 18–20.

– Finally, the reviewer suggests to specify in the abstract that the airfoil is at 90° incidence

This modification makes the abstract clearer, and has thus been implemented in the revised manuscript.

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Authors’ response to Anonymous Referee #3

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

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General comment with answer

The authors present a very interesting, detailed and well designed numerical analysis to investigate the vortex shedding regimes and lock-in response of a wind turbine airfoil. To this aim two simulation approaches with different levels of fidelity are proposed (URANS 2D and DDES 3D).

Although the considered configuration is very simple, in my opinion the paper proposes interesting findings, among which the novel metrics for the definition of the lock-in region.

The paper is well written, although there are some minor technical corrections and suggestions for improvements that I have proposed in the following.

The present authors are very grateful to the referee for these words, as well as for the following comments. The referee’s corrections and suggestions have resulted in significant improvements to the revised manuscript.

Specific comments and answers

– Abstract: lines from 7 to the end are not really suited for an abstract. I suggest moving them elsewhere and put more focus on the most important scientific novelties and findings of the work.

We thank the referee for this suggestion, as it certainly improves the quality of the abstract. The abstract has been modified in the revised manuscript.

– Introduction: I appreciated the variety and number of cited works. In order to better position their work within the proposed literature survey, I suggest to the authors to better (and explicitly) mention the novelties and findings of this work which had not been touched (or touched differently) by previous works.

Thanks for your comment. The novelties and findings have been better placed in the context of previous works in the introduction section of the revised manuscript. See page 5 of the revised manuscript with tracked changes.

– Section 2.3: to better compare computational costs of URANSE and DDES solvers, a table would be more suitable than many lines of text. Moreover, the choice of the quantities of interest for the analysis (max(x), std dev of Cl and mean Cd) should be better explained and motivated. Finally, lines from 202 to 214 (but in general the whole Section 2.3) would be better positioned at the beginning of the numerical results section.

We appreciate the referee's suggestion. Having the computational cost details in a table is certainly an improvement. This has been modified in the revised manuscript (see Table 1).

The authors acknowledge that the choice of the quantities of interest was not well motivated. This has been improved upon in the revised manuscript, adding value to the work, for which we thank the reviewer. See page 12, lines 310–314.

Section 2.3 has been moved in the revised manuscript as per the reviewer comment, with which we agree. Section 2.3 is now Section 3.2 of the revised manuscript.

– Section 3.1.1: one of the reason for the different frequency content observed for Cl in URANSE and DDES simulations should be the number of vortical structures modelled in the different solvers. I think that this aspect should be better investigated by the authors. In this regard, the comparative analysis of flowfield snapshots from both simulations is needed.

The referee raises here a valid and interesting topic.

We agree with the referee in that the modelling of the vortical structures is key for the prediction of the intermittent vortex shedding behaviour. The results in Skrzypiński et al. (2014) and preliminary tests by the present authors showed that 3D URANS cases do not predict this irregular shedding.

The present authors argue that the differences between the URANS-2D and DDES-3D frequency content are due to the spanwise interaction of the three-dimensional vortices, rather than the number of vortical structures modelled or resolved at a given spanwise plane. This is based on the findings made by Najjar and Balachandar (1998) (for the case of a flat plate at 90° and a Reynolds number $\mathrm{Re} = 250$), where it is shown that a 2D DNS simulation would not predict the irregular shedding of vortices seen in their 3D case, and it was proposed that the different shedding regimes were connected to the evolution of the three-dimensional vortical structures (streamwise ribs). Thus, the present authors consider that both a 3D simulation and a DES-like turbulence model would be needed to predict the intermittent shedding presented in the manuscript. This extended explanation has been added to the revised manuscript (page 15, lines 384–387).

A comparative analysis of the flowfield snapshots for URANS-2D, DDES-3D in the H regime and DDES-3D in the L regime can be seen in Fig. 1. In the new figure, the smaller vortical structures resolved by the DDES-3D case are visible. It is also observed that there are more significant differences in the flow features between the H and

L regimes than between the H regime and the URANS-2D case, whose average flowfields are very similar. This suggests that the difference in frequency content may not be solely attributed to the number of resolved structures, but also to the intermittent vortex shedding regimes. The present figure has been added to the revised manuscript, along an accompanying discussion. See Figure 18, and page 27, lines 629–637.

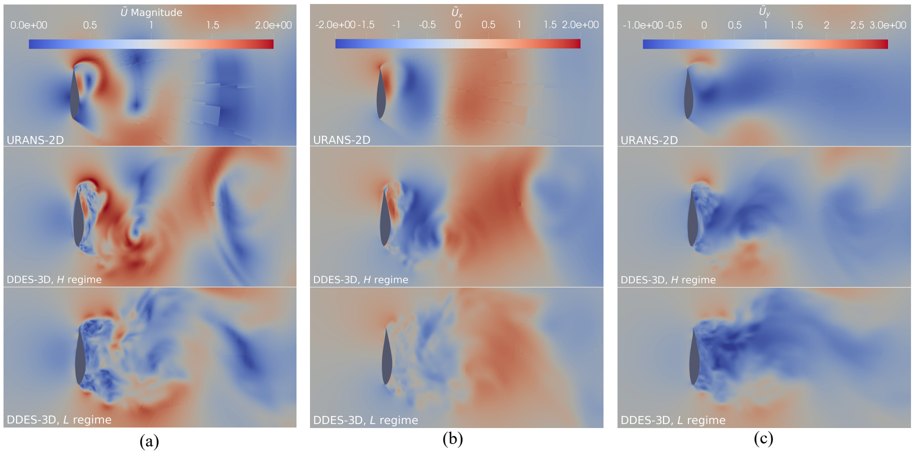

Figure 1. (a) Instantaneous non-dimensional velocity magnitude $|\tilde{U}|$, (b) instantaneous non-dimensional lateral velocity $\tilde{U}_x$, and (c) instantaneous non-dimensional longitudinal velocity $\tilde{U}_y$. For three different cases: (top) a time instant in the URANS-2D static simulation, (middle) a time instant in the DDES-3D H regime of the static simulation, and (bottom) a time instant in the DDES-3D L regime of the static simulation. The DDES-3D results correspond to the midspan slice.

– As a further general comment, the authors use an in-house solver which is mentioned to be well validated. Nevertheless, often in the paper they cite results from other authors which deal with a similar configuration (e.g. lines 268 onwards for a flat plate) but the authors don't investigate those cases with their solver. Another example is related to the oscillating cylinder case which is mentioned in the introduction. The authors should put some effort in finding in the literature a test case (either numerical or experimental) which is relevant (or simply similar, e.g. a pitching airfoil undergoing dynamic stall) to the VIV problem and which they can address with the proposed solver. This validation task is, in my opinion, fundamental to improve the value of this work. Finally, when mentioning results which can support their present findings, I suggest the authors not simply mentioning them but also including them in the paper by acknowledging the authors. For instance, the work by Lian which is cited in lines 489 and further. Moreover, this specific work would be suitable as a validation test for the present solvers even if the authors mention that Reynolds numbers of Lian case are much smaller than those typical of wind turbine airfoil flows.

The in-house solver used (MaPFlow) has been validated in 2D and 3D simulations during five PhD theses, the most relevant to this work being Papadakis (2014); Diakakis (2019) and Theologos (2022). Additionally, the solver has been validated against experimental data for separated flows in Manolesos et al. (2014); Papadakis and Manolesos

(2020) and Manolesos and Papadakis (2021). Lastly, some collaborative code-to-code comparisons have also been undertaken such as those by Sørensen et al. (2016) and Prospathopoulos et al. (2021).

A more detailed account of some of these previous validation works has been added to the revised manuscript (page 6, lines 179–182).

We would like to point out that the authors' original intent was to verify the present results against those of Skrzypiński et al. (2014); Lian et al. (2022) and Lian et al. (2023), but the shortcomings of these works (as mentioned in the manuscript) make direct comparisons somewhat impractical. Nevertheless, such a comparison has now been added to the revised manuscript as a new subsection before the main results, as it certainly adds value and confidence to the results presented. See Section 3.1 (including Figure 4). Note that the results from the previous works are not simply mentioned, but also included in the paper for direct comparison. These comparisons had been already investigated by the present authors, but were not included in the manuscript as they could be confusing to interpret, given the shortcomings of the previously published results.

All in all, we agree with the referee that the present work would benefit from further validation. The comments made here by the referee are certainly very valuable, and we will consider them in our future work.

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