WES 2023 paper review: Wind turbine rotors in surge motion: New insights into unsteady aerodynamics of FOWT from experiments and simulations

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General comments

The paper focuses on the rotor aerodynamics of a floating wind turbine based on both wind tunnel experiments and numerical approach. The study shows the results of well-conducted wind tunnel testings with a wind turbine model, featuring a moving rotor capable of translating in surge with $f_r = fD/v_0$ up to 1.09 at a high Re of 125K. The experimental results confirm the numerical simulations carried out using free vortex methods. Extensive numerical simulations that complement the experiments show the importance of the returning wake effect on the local aerodynamics of a floating wind turbine. The paper presents a number of new findings that are valuable to the wind energy community and clarifies some points already addressed by previous studies, as follows:

- The relevance of the three parameters namely the rotor reduced frequency f_r, tip speed ratio λ and motion to inflow velocity b_{vel}. As shown in detail in the paper, these three parameters must be taken into account when describing the local aerodynamics of a FOWT and cannot be isolated from each other.
- The large differences in the results for rotor torque amplitude depending on the model used depends on whether the model takes account of the returning wake effect.
- The ranges of rotor reduced frequency f_r, tip speed ratio λ and motion to inflow velocity b_{vel} which produce a different aerodynamic response of the rotor. Similar results are shown for quite different rotor sizes (the TUHH rotor, the OC6 rotor and the IAE 15 MW turbine) which shows the universality of the results.

As such, this paper merits a publication in the wind energy science journal. However, it contains a lot of information, which is sometimes inadequately organised and falls somewhere between a report of the research and the paper itself, which contains the main ideas the author wants to convey and the data to support them (as detailed later). I think it is necessary to reshape some part of the paper and perhaps reduce some parts of it. It's more a matter of form, but it's still important to produce a better version of the paper.

Specific comments

Abstract

Overall, the abstract is quite good and sums up the article and its idea fairly well. I would suggest to add more quantitative details about the range of f_r, b_{vel} for which the different phenomena are observed. Also the validity of the quasi-periodic approach (until which f_r for a given b_{vel}), *line 11*: which region is covered? and *line 13*: which range? Also, which of the models tested seem to best account for the aerodynamics effects described.

1. Introduction

No specific comments apart from the last paragraph where the sections are presented. This could be re-written (section 7 has to be included as well).

2. Theory

This section is important as it describes the different unsteady aerodynamic phenomena that a FOWT rotor blade might encounter. Perhaps you could reduce a bit the description to save some space (but not mandatory).

I have some doubts about the use of the word *transient*. From my knowledge, usually in physics, the term transient is used for any phenomenon described by x for which $dx/dt \neq 0$. And it describes a transition between two states until somehow a steady state is reached. In this case, the system is always *transient*. I would suggest to change the wording to *unsteady* even though you give the reasons why you choose transient. Then in equation (1) this would give *quasi-steady* and *unsteady* to describe the different parts. This can however still be discussed. But I am afraid the term might be misleading as the study investigate cyclic phenomena.

After equation (1), you could express $V_m = 2\pi f A_m$ to help a reader that does not know about V_m.

The formulation of equation (3) looks right even though the derivation could start form a first order Taylor derivative of P, such as: $P(v_0+V_m, \lambda_0+\Delta\lambda) = P(v_0, \lambda_0)+\partial P/\partial v_{(V_0,\lambda_0)}V_m+\partial P/\partial \lambda_{(v_0,\lambda_0)}\Delta\lambda$

You are focusing on the returning wake effect, which is the main phenomenon discussed in the paper. In order to improve the visualization of the effect, you could link figure 1 to figure 2 by showing where we are looking at in figure 1 with respect to figure 2 and perhaps draw a shed vorticity path (shown in green in figure 1) into figure 2. The explanation in the text is quite clear but could also be shorten.

3 Previous numerical and experimental works

This section could be reduced in order to shorten the text. The review of the literature is good and justifies the need to go further in the analysis of FOWT rotor aerodynamics.

4 Numerical methods and setups

This section describes the technical aspects of the numerical methods used, the description could be reduced (if possible or put in the appendix) as references to more detailed papers are provided.

5 Measurement campaign and simulations

This section is important, shows good results but could be re-organised. The set-up used is well designed and the explanations and the discussion are convincing. The region where the quasi-periodic theory is no longer valid should be better indicated. You show that whether looking at the thrust or the torque, the results show the same trend (A.E), do you confirm?

On the one hand, you show that the set-up is designed to induce as little disturbance as possible from vibration and, that only torque measurements are reliable. On the other hand, you describe the methodology for correcting for small variations in rotational speed, which cannot be kept constant. You could include this part in the appendix, as it has less to do with the story of your article. Finally, you show the results of the experiments on torque amplitude together with the numerical results. The way this different information is combined can be confusing. I suggest that you clearly separate the set-up and what you were able to measure with the part that shows the results. Figure 7 and 8 could be included in the appendix.

Regarding the set-up, for future tests, a lower Re which stills provide a sufficiency realistic blade aerodynamic could be considered to reach higher f_r.

minor: you could remove figure 5 from the paper.

minor: I would suggest to use rather mathematical expression for rotor reduced frequency, norm. amplitude etc. in table 3.

For figures 9 and 10 however, I think it is important to be clearer on the real expression of "Normalised torque amplitude". This also applies to further figures later on (figure 11, 12). You could write the mathematical expression of the quantities you are showing to erase any doubt about the meaning.

I found the figure 10 appropriate to conclude about the quasi-periodic behaviour of the rotor for f_r up to 1.09 based on the torque measurement. You could consider plotting on a second graph Q/b_{vel} or Q/A_m vs f_r and the quasi-periodic solution to show quasi-periodicity.

The sub-section *5.5 Simulation of extended motion parameters* could be a new section, if needed when re-organising the paper.

For figure 11 you could also plot the solution from the quasi-periodic state. I quickly re-did the computation and got:
Let's assume first $P_{transient}$ to be zero and $C_p(\lambda(t)) = c_p$ constant (which you do in the paper at some point, *line 788*), than based on equation (1):

$$
\begin{aligned}
< P(t) > /P_{wind} &= \frac{< P(t) >}{1/2\rho A v_0^3} \\
&= \frac{c_p < (v_0 + V_m \sin(2\pi f t))^3 >}{v_0^3} \\
&= \frac{c_p < (v_0^3 + 3 v_0 V_m^2 \sin^2(2\pi f t) + 3 v_0^2 V_m \sin(2\pi f t) + V_m^3 \sin^3(2\pi f t)) >}{v_0^3} \\
&= \frac{c_p (v_0^3 + 3 v_0 V_m^2 1/2 + 0 + 0)}{v_0^3} \\
&= c_p (1 + 3/2 b_{vel}^2)
\end{aligned}
$$

This formulation is not in ad-equation with equation (A-5). It looks like to derive (A5) you consider $< \sin >=< \sin^3 >= 1/2$ which is wrong as it is 0 and $< \sin^2 >= 1/2$. You could perhaps have a look again to the equation and maybe prove me wrong. But if the derivation here above is right this would explain the quadratic behaviour of the normalized power coefficient of figure 11 vs. b_{vel} (here if I understand well the normalised power coefficient is $\frac{<P(t)>/P_{wind}}{c_p}$). But then I don't get why the behaviour is still quasi-periodic, because if it would be the case then you should have the same value of the normalised power coefficient for a given b_{vel} as it does not depend on f_r. Why this is not case here? For $b_{vel} = 0.06$ the formula derived above gives a normalised C_p of 1.005 and for $b_{vel} = 0.08$ gives a normalised C_p of 1.01. It does look like it is in the range of the uncertainties of your measurements and of the numerical simulations, you should perhaps here plot them with the results.

You could show the same results as in figure 19 but with the power. Do you then conclude that quasi-periodic state is valid up to $f_r = 1.9$ and fails for higher frequencies?

The figure 13 is of great interest and I appreciate the interpretation. Regarding the normalisation, why not normalising with the quasi-periodic solution so that a deviation to one is clear sign of the transient behaviour? You should write in the graph 13, $1/2 f_{3p}$ and f_{3p}. Also indicating the regions where transient and non-transient effects are observed would help to improve the graph.

I appreciate the explanation with respect to figure 15, which explain the previous results of figure 13. Up to an airfoil reduced frequency of 0.03 the behaviour is driven by circulatory unsteady airfoil effect and the difference between cases with 1 and 2 airfoils show the appearance of the returning wake effect. In line 603-605 you write: "The difference at higher airfoil reduced frequencies can most likely be attributed to the non-circulatory unsteady airfoil effect, which cannot be modelled by the lifting line method." So why using lifting line method for further tests and not considering panel methods?

6 Simulation studies on the UNAFLOW and the IEA 15 MW rotors

This section is of great importance as it shows the previous results not only apply to the TUHH rotor but also to other already studied rotor (OC6 and the full scale IEA 15 MW). In order to reduce size you might think to combine plots. Overall the discussion regarding the results is adequate and I have no further comments on them.

matplotlib: you should consider using a different line styles for figures 20 and 21 so that the data on a black and white version of the document are differentiable. You can have a look to: `https://matplotlib.org/stable/gallery/lines_bars_and_markers/linestyles.html`

8 Summary and conclusions

Overall the conclusion is good but could include more details, especially about the range of f_r and b_{vel} where the transient appears as well as which model captures the best the phenomena. *line 716:* scenarios*