# WES 2023 paper review: Realistic turbulent inflow conditions for estimating the performances of a floating wind turbine

from Cédric Raibaudo, Jean-Christophe Gilloteaux, and Laurent Perret

July 10, 2023

## General comments

The present paper proposes a method to generate realistic turbulent inflow for floating wind turbine simulations, where the inflow generated is corresponding to the far-wake behind a floating turbine, around 8D downstream. The paper is overall well written, clear, short but explains sufficiently the aim of the study and the content is quite convincing. The work done is novel as it combines experimental approach to generate realistic inflow conditions for floating wind simulations based on stochastic method. The paper is well suited to be published in the Wind Energy Science journal. However, certain aspects should be revised and some parts be enhanced in order to improve the quality of the manuscript. These are detailed hereafter.

## Specific comments

### Literature review and motivation

The review of the literature and motivation of the study are well presented, showing the need to better account for wake effects of floating wind turbine in aero-elastic simulations. However, some recent studies are omitted in the citation and would add more value to the literature review to show the importance of floating wind turbine's wake dynamic. Among others:

- Chen, G., Liang, X. F., Li, X. B. (2022). Modelling of wake dynamics and instabilities of a floating horizontal-axis wind turbine under surge motion. Energy, 239, 122110.
- Li, Z., Dong, G., Yang, X. (2022). Onset of wake meandering for a floating offshore wind turbine under side-to-side motion. Journal of Fluid Mechanics, 934, A29.
- Messmer, T., Hölling, M., Peinke, J. (2023). Enhanced recovery and non-linear dynamics in the wake of a model floating offshore wind turbine submitted to side-to-side and fore-aft motion. arXiv preprint arXiv:2305.12247.

These three papers show numerically and experimentally the impact of floating wind tubines' motions on wake recovery and dynamics.

The paragraph which starts at line 83 to 94 could be revised. As discussed after in this paper review, do you really account for wave-induced surge motion in your experiments? I would suggest to write more general about surge motion. Please see "technical corrections" for some improvements.

## 2. Experimental set-up, 3. Mean structure of the flow, 4. Scaling

Overall these three sections are well presented and important for the reader. The experimental set-up is well described and enough information are given to enable reproduction. There is, however, some confusion with the inflow conditions which should be clarified. In line 102, you write: “without model, the free-stream velocity is $U_\infty = 4.2$ m/s and the turbulent intensity about 0.5%”. The inflow used for the experiments is, if well understood, an atmospheric boundary layer with a mean TI of 8 % (as said after in the paper and to be seen in the figure 3). So you should avoid such a sentence that confuses more the reader and does not help for the understanding. Also, even if it is cited, the characteristic of the inflow (profile of TI and estimation of the integral length scale) could be specified.

Regarding the disk used, the $C_t$ value seems quite low in comparison to real wind turbine operation where $C_t$ is usually in the order of $0.8 - 0.9$. Why did you choose such a low $C_t$? This surely impacts the development of the wake and represents above rated conditions that are less of interest in my point of view. A few sentences on this choice would be appreciable.

In the section “3. Mean structure of the flow”, you describe the mean flow in the wake at 4.6D and 8.1D. You also refer to previous studies of yours on the same set-up. You could discuss more the wake in terms of its development, is the wake at 8.1D the fully developed far-wake or is it still in the transition region. Regarding the high inflow TI, you might at 8.1D already be in the far-wake but the low $C_t$ might shift the region where the far-wake starts. I specifically make this comment because on your TI profiles in the y-axis from your previous paper, we clearly see the two peaks in the TI at x = 4.6D as well as at x = 8.1D, which are characteristic of the so-called shear layers that have not merge yet. Also what is the amount of recovery at 8.1D (and power available), this is then important when you compare the power produced by a turbine placed in this region.

The part about the scaling explains well the comparison between the model to a real 2MW or 5MW turbine. The last paragraph (lines 156 to 159) could be re-written. As you write, the wave-induced motion are happening at a frequency that is too high to be reproduced experimentally here. In order of magnitude, for a wind speed of 10 m/s, wave-induced motion gives $F_m^+$ of 0.8-1.0 which is above the range you investigated. In your case, it seems you cover more the low frequency-large amplitude types of motion (i.e driven by the moorings). You could be clearer on that, especially because previous studies of Chen et. al (2022), Li et al. (2022) and Messmer at al. (2023) showed how the wake dynamics depend greatly on $F_m^+$. So in this study, you cover a specific range of motions (around the surge natural frequency) but not all types of FOWT’s motion.

I would suggest that you reorganise the three sections, you could as you did, start by the experimental set-up then write about the scaling and then detail the cases you investigated.

## 5. Stochastic reconstruction using POD-LSE multi-delays

The methodology used to reconstruct the velocity fields could be more detailed, so that the reader do not need to read other references to understand your approach. For instance, how do you exactly determine $a_i$, this is only partially described and could be enhanced.

From the results presented in figure 4, should we conclude that 100 modes are enough to resolve 80 % of the flow? If not, you could detail what do the eigenvalues represent.

It is good that you show the spatial modes (figure A.1). I think you could even include them in the paper, since they describe some important structure in the flow. You could also comment more about the physical meaning of these structures. As you write, it looks like mode 1 is associated to lateral meandering wherease mode 2 could be some pulsing mode and mode 3 a combination. What is your interpretation on that?

The results of figure 5 are very interesting and could be more interpreted. Is there any reason why you plot pre-multiplied spectra rather than the normal spectra? If yes, you could explain why. Panel (a-3) shows that the mode 1 is excited by the movements for the higher frequencies ($F_m^+ =$ 0.11-0.14) but not the lowest. In contrast mode 2 is excited by the three frequencies. What is your interpretation on that?
You mention that the wake of the fixed disk shows high-energy peak at $f^+ \approx 0.13$, which is seen in the psd of the time coefficient of mode 1 but not mode 2, this is most certainly the natural meandering frequency of the disk. This might also explain why you see some peak for $F_m^+ =$ 0.11-0.14, very close to $f^+ \approx 0.13$. With respect to real floating wind turbines, it would be interesting to see in which range the natural frequency of meandering is observed. From my experience and from previous work, it is in the range $f^+ \approx 0.1 - 0.4$.

In the sub-section 5.3 "physical interpretation of the modes dynamics", you show some correlations between the time coefficients and the position of the center of the wake but do not really interpret these results. I wonder what the lowest correlation for $F_m^+$ for mode 1 between the time coefficient and the wake center really mean? If you would do this analysis with the wake edges, i.e in the shear region which separates the inner wake to the incoming flow, perhaps the results would be quite different and you might observe a higher correlation with mode 1 for $F_m^+ \approx 0.11 - 0.14$ showing that these frequencies excite even mode meandering.

In the sub-section 5.4 "reconstruction using multi-time delay LSE", it is to me not clear how you reconstruct the wind field based on the data you have. Please update this section to make it easier to understand. As you write, the method you used to reconstruct the wind field lead to a quite large loss of information. In fact, as Figures 7 and 8 show, the reconstructed field is less energetic than the original field, particularly for $f^+ > 0.25$, which may explain why the reconstructed RMS profiles are smaller in magnitude. Do you have any ideas how to modify your reconstruction method in order to better account for these smaller structures in the flow? You then use this reconstructed flow for the simulations, how do you think this less resolved flow impact your results? You should comment on that in the paper.

## 6. Implementation for FAST simulations

This section is about the utilisation of the reconstructed wind field for FAST simulations, which is a central part in the paper. Does the turbine placed in the flow at 8D is fixed or also enabled to move, you should mention it.
I find good that you show some reference simulations in figure 9, which show that for a given mean wind speed, the higher the TI, the higher the power produced. I interpret this as following: the higher the inflow TI, the larger the fluctuations, and to the power three, the higher fluctuations include more power than for the lowest TI which is extracted by the turbine and does explain why power produced increase with an increase of TI. Your simulations are done below optimum ($TSR \approx 6.6$), is there any particular reason for that?

How realistic is the final interpolated flow with respect to a real wake flow in offshore conditions? Did you have enough time of experiments to reproduce 6400 s at full scale how do the data repeat throughout a reconstructed wind field?

When you present the results of the power produced by the turbine in the wake, you write that the power is about 87 % less than the unperturbed turbine. Could you comment this with respect to the amount of recovery of the wake at 8.1D (this goes with my above comments that you should estimated the amount of power available in the wake, somehow equivalent to the amount of recovery). So from the figure 3, if the mean wind speed in the wake is 0.55 $U_\infty$, then the power available is $\sim 0.17U_\infty^3$, which is in line with your results. It is however really a standard value at 8D downstream of a turbine? Don't we except at this location more recovery and thus more power available for a downstream turbine? In Porté-Agel et al (2020), Figure 13 shows the power produced by a wind farm on the different rows and we can see that the second row produces around 0.6 of the power of the first row, which I think is more realistic. This suggests that the cases you examined may not be the closest to reality, do you agree?

You then show differences in the power produced between the cases (turbine placed in the wake of the fixed and the surging turbine). I wonder where the differences you observed come from. Are you measurements enough resolved that you can ensure these results are outside of the uncertainty range? Comparing the power available in the flow for each of these cases could help to explain the results. Maybe a look at the mean TI in the wake as well. How do the approximations of the reconstructed field play a role in these results?

### Conclusion

The conclusion is a good summary of your work. I would however be careful with the strong conclusion that in the wake of the moving turbine, you have up to 10 % less power produced by a turbine placed in it. This is in the context of your study but might not be a general statement.

## Technical corrections

The following are some technical corrections that could be made. Overall, many sentences should be checked and rewritten if necessary, and particular attention should be paid to the tenses used.

- Abstract: the scale 1:750 is with respect to a 5MW wind turbine but your set-up is a proper scale-down of a 2MW system. Before writing about surge motion, you could introduce the topic of floating wind turbine and the extra 6-DoF.
- line 16: "in recent years" or "over the last years" instead of "for the recent years".
- line 18: you could add a reference to this statement
- line 28: sentence to be reviewed
- line 32: maybe one reference about wake region would be appreciable (Porté-Agel et al. (2020), Vermeer et al. (2003) or Neunaber et al. (2020)).
- line 36: sentence to be reviewed

- line 90-94: I would suggest to avoid writing “will be presented”, in a paper I would rather write “is presented”. Then “the velocity fields are reconstructed...” instead of “need to be..”
- Figure 1: the letters (a) and (b) could be centred, the image could be bigger and vectorised scheme for panel (b) could be used, i.e which conserves the quality when zooming in.
- line 102: sentence to be removed
- line 138: the chosen prototype*
- Figure 5. why do you present results for mode 4 if you don’t show it and don’t write about it?
- lines 206-207: to be corrected
- lines 223-225: make this sentence more clear
- Figure 7. you should adapt the line style so that we clearly see the differences between the original profiles and the reconstructed profiles and change the legend.
- line 226: reaches 60 % of what??
- line 236: “are shown here” not “were shown”.
- line 248: change the title