

General comment

“Floating wind turbine motions signature in the far-wake spectral content – A wind tunnel experiment” investigates the relationship between a floating offshore wind turbine (FOWT) motion and wake dynamics. The authors consider a realistic FOWT motion, and they want to compare its impact on the wake signature to that obtained using idealized motions (harmonics, etc).

For this research work, they perform wind tunnel experiments with a porous Actuator Disk. The model is equipped with a motor that allows to represent heave, surge and pitch motions of the floater (the other motions are negligible for the considered “wave” case). When idealized motions are considered, the signature of those motions is visible in the spectral content of the wake velocity fluctuations (when high amplitudes and high frequencies are imposed). When a realistic FOWT motion is considered, the impact of this motion on the wake power spectra is negligible. The authors conclude that one should be careful by using idealized sinusoidal signals for the FOWT motions as their impacts on the wake signature are different from those obtained using a more realistic signal (at least for the considered size of wind turbine, i.e. $D = 80$ m).

The paper is well written and pleasant to read. The research work is very interesting, as it clearly highlights differences in terms of wake response between realistic and idealized conditions. The objectives are clearly highlighted, and the study is very rigorous.

However, they authors could more elaborate on the impact of the realistic motion on the wake dynamics in general. Some paragraphs could also be clearer, and some results should more clearly presented or more discussed. Please see below for more specific comments

Specific comments

1. P2,L30 --- : I am surprise that there are no more LES studies that focus on the impact of the wind turbine motions on the wake signature. Or perhaps, if they do exist, these studies always consider idealized motions?
2. P2, L45 – L50 : comparison with work of Belvasi et al. (2022) The authors have exactly the same set-up? The work of Belvasi et al. (2022) also proposes to investigate differences in wake statistics between idealized and realistic motions? What does this present study add? According to the authors, the work of Belvasi et al. (2022) already highlighted that realistic FOWT motions have an impact on the wake center position, and not on the wake deficit, and on the TKE. The authors should highlight more clearly what this "complementary" research paper will add.
3. P3, L67 -> L70 : In this paragraph, both motions should be mentioned (idealized and realistic). The authors also write that they will only investigate heave, surge and pitch, and it is not clear at this stage why they consider these only.
4. P4-P5, section 2.2 : The authors should give an order of magnitude of the wind speed considered in this research work (both for the wind tunnel and the FOWT).
5. P4-P5 : An image of the experimental set-up (porous disk, ...) should be useful (even if it is surely available in other papers).
6. P5, Section motion modeling (2.3) : It is not clear for me : the authors have motion signals coming from Orcaflex, for a particular wave case, and they will use this signal as

a realistic motion (“realistic case” of the paper), right? And, based on this signal, the authors will also isolate characteristic amplitude and frequencies in order to elaborate idealized floater motions? This last part should be more elaborated or described in Section 2.3. This becomes clear later in the paper but this is not the case in this paragraph.

7. P5, L121: What do “first order” and “second order” mean?
8. P8, L175 : *“The corresponding power-law profile exponent α is 0.11, which is within the proposed range between 0.08 and 0.12.”* Where does this “proposed range” come from?
9. P8, L187-188 (discussion of Figure 6) : *“Nevertheless, even if the experimental spectra are within the range of the reference spectra provided in VDI (2000) (shaded zones), they are shifted towards higher frequencies compared to the Kaimal model”*. What do the authors conclude about this shift? As the sentence is formulated, the reader expects a conclusion. This shift is also visible for U and V, but not for W. But I agree that your modeling of the ABL is quite very similar to realistic conditions (the authors rigorously verify their set-up).
10. P12, L218-L19 : why do you show the spectral content for this case in particular ?
11. P12, L227-229 (discussion of Figure 9) : why do ϕ_{\max} of points located at $y/D = \pm 1$ increases (compared to their nearest neighbour) and why do the authors conclude that this delimitates the wake bounds?
12. P13, L236-237 (discussion of Figure 10): *“This result is consistent with literature that finds a maximum of receptivity in the reduced frequency range of [0.2-0.3] (Li et al., 2021)”*. The results of Figure 10 show that the peak is higher for $\text{fred} > 0.3$, so the maximum is not yet reached for $0.2 < \text{fred} < 0.3$. This is not consistent with Li et al, 2021. Do the authors expect lower ϕ_{\max} for higher fred ? or higher ϕ_{\max} ? The authors should be more critical about their results.
13. P16, Figure 12 : this figure is very unclear (same symbol and same color for different amplitudes, ...).
14. P14-P15, discussion about realistic motion (1dof and 3dofs) : The authors could refer to the literature (i.e Belvasi et al, 2022) to enrich the discussion. Indeed, the authors mentioned in the introduction that Belvasi et al 2022 showed the impacts of the FOWT motion on wake statistics (centerline, velocity deficits, etc). Could the authors relate their results to those studied in the reference?

Technical comments

P1, L21-22 : A word is missing in the sentence? *“The authors state that both constructive and destructive **XX** is possible between the dominant scales in the wake and a potential downstream turbine,...”*

P3, figure : [H] on the right of the figure

P7, L150 : the authors should add the two notations (ϵ_{ul} and $\epsilon_{\sigma U}$) in the text before the equations, and remove the point before the equations

P8, L182 : the notation z_0 is not introduced (normally, the notation for the roughness length is well known, but this should be defined)

P12, L232 : the authors write “*For instance, the idealised characteristic 1-Dof motion regimes (cases S1, H1 and P1) ...* “. The other cases presented in Figure 10 are also idealized. This sentence should be reformulated (“For instance, the idealized cases S1, H1 and P1 ...”)

Figures 10, 11 and 12 : ϕ_{\max} and not ϕ in the labels