The paper „ Influence of the inflow conditions on the dynamics of a floating wind turbine wake under harmonic surge motion" by Barile et al. describes the outcomes of multiple Actuator Disk (AD) LES simulations of a floating wind turbine model under surge motion and different inflow conditions, including ABL. As there is currently a gap in the literature involving the understanding of FOWT's wakes under ABL conditions, the paper provides a valuable contribution to the wind energy community and the Wind Energy Science Journal. The overall quality of the results and manuscript is good; nevertheless, after reviewing the paper, I have some questions that should be addressed by the authors before the manuscript can be accepted for publication:

1) Abstract, Line 19: Possible typo, should it not be "phase averaging"?
2) Line 28, I am not sure that the term "latter" works in this syntax. Please rephrase
3) Line 33, The description of the work of Schulz et al., is not clear, especially in the distinction between quasi-steady and unsteady effects. What are these unsteady effects? I believe that in this work Schulz refers to the returning wake effect. I am thinking that there might be no need to provide a description about unsteady effects in this part of the manuscript, and the authors should consider eliminating this part of the sentence.
4) In the UNAFLOW campaign described in the work by Fontanella et al., 2022, the authors used a uniform inflow with about 2% TI. While the wind tunnel can be used to investigate the impact of ABL on structures and wind turbines, no ABL was considered in this or subsequent studies on the same FOWT model.
5) Line 112, I am not sure that the term "considered" is right when describing the hub height. I would suggest simplifying to "the hub height is [...]"
6) What was the blockage ratio during the experiments?
7) Is it needed to provide the governing equations in the manuscript? If I am not mistaken, the authors did not modify them in comparison to their usual implementation. In that case, I am not sure that it is necessary to report them here.
8) Line 135, typo, "applied in", I think it should be "applied to"
9) Line 137: how was the perturbation added in the turbulent case? Did the authors match the turbulent spectra from the wind tunnel? As further details about the inflow are provided in the following sections, the authors should highlight for the interested reader where the additional inflow information is provided.
10) Line 141, how is the method different from the implementation in the literature?
11) Eq (5): I am struggling to follow the reasoning of the authors in this part of the manuscript and I believe that clarity could be improved. When inserting the forces in the domain, when a motion of the platform is considered, I would imagine that the forces are proportional to the relative velocity (i.e., to the vector sum of inflow speed and the structural velocity). Why does the structural velocity not appear in Eq. (5)? Since the thrust coefficient is assumed constant (line 159)

would the impact of motion not affect the inserted loads through a variation in apparent inflow speed?

12) Line 159: As porous disk I imagine the authors are referring to the Actuator disk model? Was the thrust coefficient measured in the wind tunnel? Rather than "assumed" the thrust coefficient would be rather set to match the experiment, in that case. The authors should consider clarifying this. Additionally, was the thrust coefficient under platform motion cases verified under platform motion? Did previous work observe that the thrust coefficient can be assumed constant in the range of apparent speeds considered in this work?

13) Line 161: Why did the authors select a cross section of 10DX10D? If I am not mistaken this would make the cross section of the numerical domain smaller than the wind tunnel one. At this point of the manuscript it is not clear what the relationship between the numerical and experimental results. The authors should clarify why this test case has been selected and why they are matching it in their simulations. If the objective is to use the same thrust coefficient as the one measured in the experiment, should the numerical domain not match the same blockage of the wind tunnel, as this might affect the thrust coefficient?

14) Line 175, Why 5D height? Should the cross-section not be 10DX10D as previously described in the same section? Or is this difference due to different numerical setups employed for laminar and turbulent simulations? If so, this should be clarified and justified within the text

15) Line 182, Why did the authors use a stress BC for both top and bottom boundaries? To match realistic conditions the stress boundary would be placed on the bottom surface only. Additionally, why did the authors use two different models for top and bottom boundary conditions? This should be clarified for the reader.

16) Line 190: what does "into consideration" mean in this context? Please clarify

17) Sect. 2.4, what is the rotational speed? This value is needed to compute the Strouhal number

18) Section 3,Why did the authors use the term "Preliminary" in the section title? What is preliminar about the outcomes of the studies? Any know limitation of the study should be clearly stated in the manuscript.

19) Figure 3,Why did the authors pick the mesh #2 for the analysis of results? It does not feel like the mesh is fully converged, as differences are observed in the wake width and in the velocity profile at the edges of the wake (about 0.5D). Did the authors also check what fraction of turbulence was solved by the LES approach in the wake with the selected mesh, verifying Pope's criterion?

20) Section 3, I wonder if the structure could be improved, as section 3 presents mostly the mesh-sensitivity analysis. Maybe the Q-criterion results be reported in a following section and Section 3 become something like "Mesh-sensitivity analysis"?

21) Line 242, typo, "a converged statistics"

22) Line 327, from the description of results, it is not clear from what result the authors draw a comparison between laminar and turbulent inflow conditions. As investigating the impact of different inflow conditions should be the focus of the analysis (as stated by the authors) the comparison between laminar and turbulent flows takes up a small part of the discussion. To highlight this comparison I wonder if the laminar results could be added to figure 13, at least for figure (c), highlighting differences in energy content, or if the comparison could be better shown in a different way. If the conclusions are drawn only from Figures 8 and 11, I find it interesting that, if I am not mistaken, in the laminar case the coherent pulsation of the first mode is amplified as it is convected downstream, while in the turbulent case it seems to be dissipated.

23) Line 411, I wonder if the term wake meandering is adequate for the description of results with low inflow turbulence. In fact, looking at the arrows in Figure 19, it looks like the mode shows a clearly symmetric distribution around the centre line, which would suggest more a pulsation of the wake rather than meandering, with the wake expanding and contracting due to the rotor thrust oscillations.

24) Figure 21, If possible, the size of the labels should be increased, as I find them particularly small.

25) Line 451, looking at the laminar and low turbulence results of Figure 21 it might be worth highlighting that the intensity of the velocity oscillations is reduced for the turbulent inflow case.

26) Line 470, I imagine that the authors want to highlight how for St=0.47, the half amplitude increases much faster, showing the peak value already at 3D. If this is correct, the sentence "except the highest frequency" might need further clarifying.

27) Line 470, from the description of the low-frequency results it seems that the onset of decay of the wake pulsation happens closer to the actuator disk than for the higher frequency cases. However, looking at results, the trend for the low-turbulence case seems opposite, as the velocity oscillation maxima gets closer to the AD with increasing St in Figure 22, b. I imagine the authors are referring to a faster decay in comparison to the laminar results of Figure 22, a. I would suggest clarifying this point for the reader by making the comparison with the laminar results explicit.

28) Line 474, could the fact that the wake pulsation at these St numbers is unaffected by turbulence be correlated to the observed spectra in Figure 12 a and b? The wake spectra without motion shows a more significant frequency content for lower frequencies, which is then dampened out after about St=0.3, i.e, when the location of the peak seems not affected by turbulence.

29) Line 494, "inclination towards the left" could maybe be improved to inclination in the negative x direction, or towards the AD or something else?

30) Line 508, I wonder if it is correct to describe the wake as the superposition of pulsation and wake meandering. Indeed, if I am not mistaken, it seems to me like

the observed wake meandering is due to the interaction between the wake pulsation and shear layer of the ABL, which deforms the pulsation in the vertical direction leading to the observed velocity patterns. In fact, was there vertical wake meandering in the fixed-bottom ABL case which could justify this comment? My idea would also fit with the following description proposed by the authors about the impact of different vertical advection velocities and the onset of wake meandering (line 520).

31) Figure 23, I wonder how this result combines with the ones in figure 21. In fact, there the authors described an inclination towards the left, while here the same conditions are seen as an inclination to the right (i.e., in the streamwise velocity). While I agree with the latter, I wonder if in Figure 21, we are actually observing an inclination to the left, or if the inclination is still towards the right, but the low-velocity zones are merging across different wake pulsations. In fact, in Figure 23, the shift of the wake pulsation in the bottom side of the wake is about 2D, matching the lines I have drawn in the Figure markup and the expected slower advection due to the ABL. Otherwise, If the authors do not agree with this explanation, how do they connect the different inclinations between Figure 23 and 21?

(b) Low-turbulence flow – $S_t = 0.32$ (c) ABL flow – $S_t = 0.32$
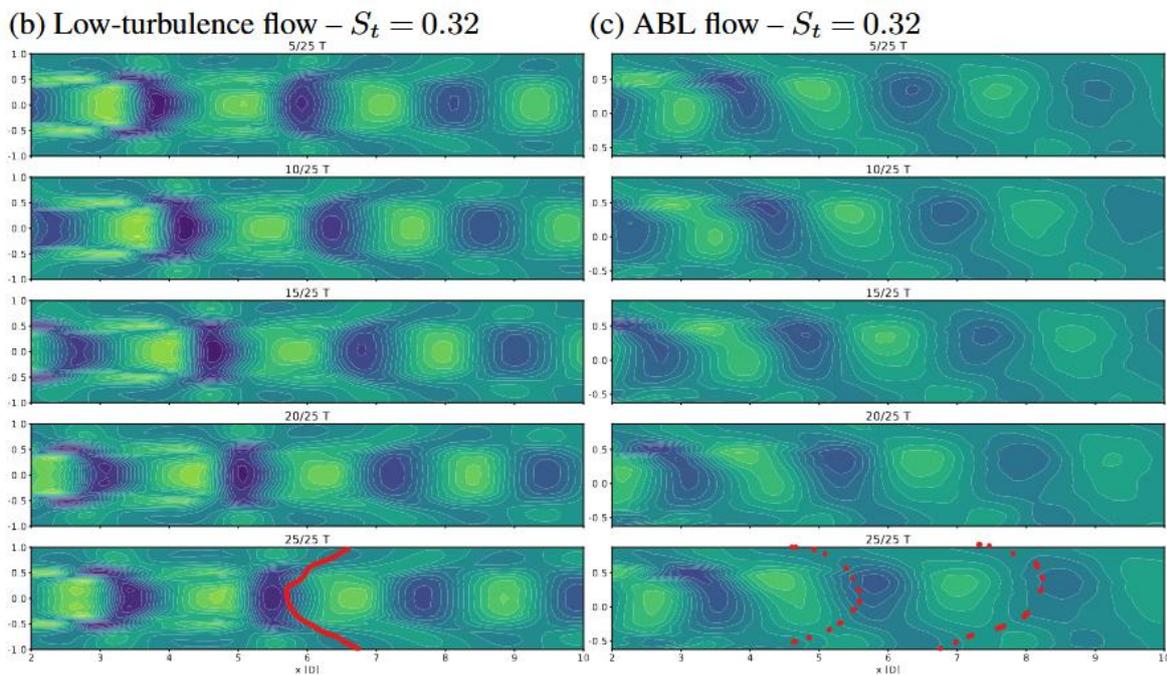
32) Conclusions, Do the authors believe that the AD model can provide similar results to an actual three-bladed turbine model when describing the formation of coherent vortex structures? In fact, for a three bladed turbine under surge motion, the helical tip vortices, which are not captured in an AD model, might impact the behavior of the vortex ring coherent structures. A comment should

maybe included in the manuscript to highlight possible limitations of this study and/or future work.