Response to the Editor

I regret to write you that one of the reviewers are still not fully satisfied with your revised paper. As I understand it, the main issue is that the reviewer wants you to demonstrate that the instabilities causing vortex breakdown in the laminar case is not due to numerical issues: *'It is not clear that the flow structures observed on the vortices between 2D and 3D are physical mechanisms in the laminar case in Fig. 10 (a) and (c). These flow structures look similar to flow structures created by numerical effects. Therefore, the mechanism of vortex breakdown in the laminar case may be due to numerical effects. Please improve the spatial discretization in the wake and show that the results of Fig. 10 are not dependent on the grid.'*
I suggest that you to carry out the proposed additional computation, if possible, as this is a cornerstone in the analysis. If this is not possible, you need as a minimum to discuss it in the paper. Finally, there are suggestions some minor modifications, that should be easy to implement.
Dear Editor, thank you for your coordination of the review process. We are anyhow grateful to the Reviewer as constructive criticism is essential to improve the scientific quality of the studies. While an additional calculation is not at hand, we have included significant new discussion in the paper to address the doubts raised by the Reviewer. We hope that the new version of the paper can be now worthy of publication in WES.

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Reviewer #2

Review of "How does turbulence affect wake development in floating wind turbines? Some insights from comparative LES simulations and wind tunnel experiments". The authors satisfactorily addressed most of the previous comments in this version of the manuscript. There has been a clear improvement. My main concern is related to the old main comment #2, which I consider still open. Since this is a critical point, I reinforce the need for more studies to show that the results are not dependent on the numerical method. Additionally, there are a few remaining specific comments that I recommend addressing before publication. The comments that I consider to be still open are listed below:

Main comments:
1. (Old comment #2. It is not clear that the flow structures observed on the vortices between 2D and 3D are physical mechanisms in the laminar case in Fig. 10 (a) and (c). These flow structures look similar to flow structures created by numerical effects. Therefore, the mechanism of vortex breakdown in the laminar case may be due to numerical effects. Please improve the spatial discretization in the wake and show that the results of Fig. 10 are not dependent on the grid.)

I appreciate that the authors took steps to select the mesh. However, unfortunately, I do not consider this issue solved. This is a major issue that could raise questions regarding how well the simulations reproduce the physics of the flow. The second half of the authors' response is focused on the Length Scale Resolution, which is a metric related to turbulence and the resolution of small turbulence scales. Maybe I was not clear in my comment. This is an issue of stability and numerical perturbations, not turbulence. When performing a simulation without imposing any perturbation, the numerical method itself introduces perturbations in the form of numerical errors. These numerical perturbations commonly have very specific frequencies and wavenumbers, usually related to spatial and/or temporal resolution. These numerical perturbations may grow in the "laminar" part of the flow, forming flow structures that would not occur in a real flow. The flow structures seen in Fig. 10(a) and (c), between 1.5D and 2.5D, resemble some of these numerical flow structures. My suggestion is that the authors improve the spatial discretization for the case shown in Fig. 10(a), keeping the same regularization kernel width (not the same \zeta). If the flow structures around x/D=2 are modified, then there is an indication that the numerical method and numerical parameters are affecting the results.
We appreciate the additional details included by the Reviewer that allowed a further critical analysis of our work. Unfortunately, we are unable to run a finer mesh as the computational resources that we used (the calculation has been run on a national super-computer for which a competitive grant was obtained) are not currently available to us. The use of such calculation infrastructure was needed because the calculation is very demanding.
We do understand the concern of the Reviewer and respect his/her scientific doubts: we have tried our best to address them in the paper better now that they are clearer to us. The flow structures between 1.5D and 2.5D resemble - and have been interpreted as - short-wave instabilities in the tip vortices. As detailed in the paper, this instability mechanism has been observed experimentally in helical tip vortices and is caused by strain forces on the vortex core that are generated by neighboring vortices. In the absence of other external disturbances, this mechanism appears to be the primary instability leading to tip vortex breakdown. Because this instability involves disturbances at wavelengths similar to the vortex core size, the latter must be resolved adequately. Given the size of the vortex core in the ALM simulations, and the mesh size

in the current LES simulations, we argue that this is indeed the case. All appropriate references are provided in the amended manuscript. We admit, however, that the observed mechanisms may not be observed in reality, at least not to the same extent, as the tip vortex core is larger in the ALM simulations than in the experiments. In addition, as the Reviewer and some recent literature have pointed out, the cartesian grid may introduce small numerical perturbations, which could exacerbate the observed phenomena. We have pointed out these limitations clearly in the manuscript. We also believe that the fact that the primary instability mechanism changes when turbulence is included in the flow solver is indeed significant to this study and further highlights the importance of including such free-stream disturbances when studying FOWT wakes. We are confident that the Reviewer's concern is now more appropriately addressed.

Specific comments:

1. (Old comment #6) The name of the solver is now included at the beginning of section 3 and some references were provided. However, there is no description of the code and the references do not provide technical information about it. The only reference to (Richards et al., 2024) that I could find was a website. It was not properly referenced as a website in the manuscript, but I could not find an academic paper or technical report with the information provided in the references. If (Richards et al., 2024) refers to a technical publication, please provide more details so the reader may find it. From my point of view, a simple reference to a general website without technical details of the method is not sufficient for an article in an academic journal such as Wind Energy Science, which is a timeless registration of the work. Without the details of the numerical method, the work is not reproducible. Please, include a brief description of the numerical methods used by the solver and provide references that describe them in detail.

We generally agree with the Reviewer's concern that enough detail should be given to be able to independently verify this work. In our first draft we focused on providing enough details to fully describe the ALM model. The settings of the flow-solver are often solver-dependent and set-up dependent, as details as the turbulence closure, computational grid and equation solution approach all influence them. Based on the Reviewer's request, we included additional details regarding the numerical schemes that were used in the URANS and LES approaches in sections 3, 3.1, 3.2, and 3.3. Since CONVERGE is a closed-source CFD code, the most appropriate reference to the flow-solution approach is the solver manual. Therefore, this reference was added, and a disclaimer was made about the possibility of asking it to the industrial authors. In addition, we also included some additional references where the solver is applied to wind turbine solution problems. In summary, in the revised version, details regarding the flow solver, CFD settings and boundary condition specification can be found in sections 3 and 3.1, details regarding the computational grid are provided in sections 3.2 and Appendix B, details regarding the free-stream velocity fluctuation generation are provided in Appendix A, and all details regarding the ALM employed in this study can be found in section 3.3. Finally, details regarding the simulation length and statistical convergence of the results are provided in section 3.5. We would like to keep the narrative of this study as straight as possible to the point. In our opinion the manuscript is already heavy in details regarding the case set-up and solution method, and to avoid overburdening the reader we have not included the flow solution equations explicitly in the paper.

2. (Old comment #9) I congratulate the authors for the change from URANS-Hybrid to URANS-STG. I agree that the name is now adequate. However, the reference to the old name can be found in the introduction of section 4 ("h" stands for "hybrid"). Also, section 4.4 employs mostly the old nomenclature, which makes this section confusing and not consistent with the rest of the manuscript.

The authors would like to apologize for the confusion. The section has been revised and updated to reflect the new nomenclature. No reference to the old terminology should be present anymore in the final draft.

3. (Old comment #31) In the caption of Figure 31, it is not clear if \Delta WD is the amplitude of WD or the peak-to-peak amplitude. The text says "amplitude", which usually means peak-to-mean amplitude, but the formula is for peak-to-peak amplitude.

The Reviewer's comment is correct. The amplitude shown in the plots is calculated as $\boldsymbol{\Delta WD = (WD_{max} - WD_{min})/2}$, where $\boldsymbol{WD_{max}}$ and $\boldsymbol{WD_{min}}$ are derived from the phase-averaged cycle, as already specified in the caption. A clarification has been added to the paper.