Response to the Editor

The paper is still subject to 'major revisions' although it most likely is a minor issue that needs to be rectified (essentially a question of wordings regarding if the observed instabilities are numerical or physical). However, it is assessed by the reviewer to be of importance to phrase it correctly in order not to mislead unexperienced researchers.
Dear Editor, we have tried our best to revise the paper as suggested by the Reviewer. We would like to thank you again for the coordination of the review process of this study.

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Reviewer #2

In the latest version of the manuscript "How does turbulence affect wake development in floating wind turbines? Some insights from comparative LES simulations and wind tunnel experiments", the authors satisfactorily addressed almost all of the comments. The authors show a clear effort to solve all of the comments, to improve the manuscript. The only open comment is still the one regarding whether the flow structures seen in the laminar simulations are created by numerical effects.

First of all, I would like to acknowledge that this is a good paper overall and it should be published in some form, if adequate steps are taken. Nevertheless, the open issue is a critical one and should be resolved. The best-case scenario would be the inclusion of results with improved discretization (or other additional simulations that could clarify this issue). From the authors' response, I understand that is not possible. In that case, I agree with the editor and the authors that the additional discussions could be an alternative to additional simulations. However, I am not convinced by the new discussion introduced on page 17:

- I do not believe the grid resolution is sufficient to capture shortwave instabilities. The comparison of the tip vortex core with experiments and other simulations of (Cioni et al. 2013) makes no sense for the current simulation. As stated in (Cioni et al. 2013), the initial core size in ALM simulations is a function of the kernel size. Therefore, the vortex core size of the experiment or other simulations is not related to the vortex core size of the current simulations. With a ratio of Kernel function width and local mesh size equals to 2.4, the initial core is not believed to be well-resolved. A value of 2 for this ratio (comparable to 2.4) has been observed to not represent the core accurately by numerous studies (Shives & Crawford, Wind Energy, 2013; Meyer Forsting & Troldborg, TORQUE 2020; Kleine et al., JFM 2023). There is vortex core growth due to diffusion, however, it is unclear if the growth would compensate for the lack of initial resolution. For example, Ribeiro et al. (2025) used a ratio of 7, to represent the vortices well. Anyhow, this is dependent on the numerical method. If the authors have data that show that the vortex core is resolved enough to capture short-wave instabilities, I suggest that they show it.
- According to Ribeiro et al. (2025), "they have been shown to occur in reality, but are only numerically captured via blade-resolved, scale-resolved simulations". I cannot be sure that they cannot be numerically captured via ALM. However, I have never seen a reliable paper that confirmed they are. I have seen instances where structures created by the ALM were misidentified as shortwave instabilities, but were then revealed to be something else.
- Even if the resolution and the method could capture shortwave instabilities, I cannot agree with the similarities the authors claim between the structures in Figure 10 and shortwave instabilities. For example, in figure 10(a), these "instabilities" are practically aligned in the direction of the flow, for different helices. In other words, the position z/D of the structure in one vortex matches perfectly the position z/D of a structure in the subsequent vortex. For me, it seems extremely unlikely that a phenomenon that is dominated by the dimensions of the vortex core would align so perfectly with the rotation period. From the preliminary simulations I have seen and run in the past, these structures look very similar to structures created by numerical effects related to the mesh. Unfortunately, I cannot provide a reference, because these were all preliminary simulations. The only reference I could find was: "Kleusberg E, Schlatter P, Henningson DS. Parametric study of the actuator line method in high-order codes. Technical report, KTH, 2017", which is not available online. It is available as an annex to "Kleusberg, E., 2019. Wind-turbine wakes-Effects of yaw, shear and turbine interaction. PhD Thesis, KTH, 2019", if you can find a physical copy.
- I agree with the phrase "Therefore, the results presented herein for the laminar case may not exactly match an equivalent experiment."

Despite all that, the work is good, the discussions are relevant and the presentation is of high quality. I do not intend to impede the publication of an interesting paper. However, in its current form, unless I am mistaken, I believe it could misdirect an inexperienced researcher such as an MSc or recent PhD student. Therefore, I suggest two possible paths:

1) The original suggestion, that further simulations be performed, to verify if these structures are created by numerical effects; or
2) That a discussion be added to the paper, in which it becomes very clear to the reader that the main hypothesis for the appearance of these structures is due to numerical effects.

If the second option is followed, I believe one way to carry out that would be:
- To acknowledge that the mechanisms for the creation of the observed flow structures were not identified;
- To indicate, very clearly, that the leading hypothesis is numerical errors;
- You may choose to disclose that the main hypothesis of numerical errors could not be confirmed by better-resolved simulations due to the costs of numerical simulations;
- To caution the reader that some of the numerical results might differ from experiments because of it;
- To reinforce that the analysis might still be relevant, even though it might differ from real flows, because it could give insights regarding best practices for numerical simulations;
- If shortwave instabilities are mentioned, my recommendation is that it becomes clear to the reader that this is an unlikely hypothesis (unless the authors can supply relevant data to support it) or that they are probably numerical shortwave instabilities that are not necessarily related to physical shortwave instabilities of vortices;
- To review the other sections to verify if this limitation would require changes.

We appreciate the additional details included by the Reviewer and the well-motivated scientific criticism that allowed a further critical analysis of our work. The paper has been revised according to the recommendations provided by the Reviewer at point #2 of his/her notes. We also checked that the message is conveyed coherently along the study.