First and foremost, the authors would like to express their sincere gratitude to the editor and the reviewers for the time dedicated to reviewing our manuscript for the second time and for providing their valuable reports. In what follows, we address each comment individually. Additionally, a revised version of the manuscript with all the changes highlighted is included at the end of this document.

Comments from Reviewer 2

1. *Regarding the sentence in the abstract: "In low-turbulence, meandering and vortex rings act as decoupled phenomena. Replicating the analysis on a horizontal plane at hub height, it is observed that lateral meandering is uniformly intense under this inflow condition. Conversely, under ABL conditions..." and similar statements throughout the manuscript, I suggest clarifying that the primary driver of wake meandering is the shear in the undisturbed inflow, rather than the turbulence intensity alone. Therefore, expressions such as "low-turbulence inflow" should be replaced with more precise wording, for example "low-turbulence no-shear inflow" or equivalent formulations.*

Response: We thank the reviewer for this insightful comment. Following your suggestion, we have updated the terminology throughout the manuscript to be more precise, replacing "low-turbulence inflow" with "low-turbulence no-shear inflow" in the Abstract (lines 7, 12, 13, and 19) and in the Introduction (line 102).

2. *Expressions such as "atmospheric scenario" or "atmospheric wind tunnel tests" should be made more precise, for example with "ABL scenario" and "ABL wind tunnel tests," respectively.*

Response: We thank the reviewer for the recommendation. The text has been revised accordingly, and the expressions have been changed to "ABL scenario" and "ABL wind tunnel tests" in lines 579 and 590, respectively, to ensure scientific precision.

Comments from Reviewer 4

- *Comparison with Schliffke et al.: The authors have explained and clarified that the setup of Schliffke et al. was chosen as a reference for a strongly sheared ABL. However, the choice of Ct = 0.65 is rather uncommon for FOWTs and appears to be specifically tailored to the porous disk used by Schliffke et al. From my point of view, this study provides an excellent environment for comparison with the results of Schliffke et al. Therefore, it is rather confusing for the reader that no such comparisons have been performed. I can only assume there is a specific reason why this has not been done or presented. It would be very helpful for the reader to include a proper explanation as to why no comparison between experiment and simulation is provided.*

  Response: We thank the reviewer for highlighting the benchmarking value of this setup. In the following, we clarify the choice of parameters and discuss the comparisons performed with the experiments of Schliffke et al (2024).

1. **Representativeness of $C_t = 0.65$ for Utility-Scale FOWTs:**
   As previously noted, the choice of $C_t = 0.65$ was selected to match the reference physical setup of Schliffke et al. (2024). Nevertheless, we appreciate the reviewer's observation regarding the representativeness of this value for full scale Floating Offshore Wind Turbines (FOWTs). In this regard, we would like to note that thrust coefficients in the range of 0.6 to 0.8 are commonly reported for utility scale offshore turbines operating in below rated and near rated conditions, which are also the regimes where wake interactions and floating platform dynamics become particularly relevant for offshore wind farm design. To illustrate this point, several well known industrial reference turbines exhibit design values within this range:
   - The standard **NREL 5-MW Reference Wind Turbine** operates with a thrust coefficient of approximately $C_T \approx 0.68$ under typical conditions near rated wind speed.
   - The **IEA 15-MW Offshore Reference Wind Turbine** reports a thrust coefficient of approximately $C_T = 0.799$ within its operational envelope.

   Therefore, a value of $C_T = 0.65$ can be regarded as representative of typical operating conditions encountered in utility scale offshore wind turbines, particularly in the regimes considered in the present work.

2. **Comparisons with Schliffke et al. (2024) already in the Manuscript:**
   Regarding the comparisons with the experimental work of Schliffke et al. (2024), we would like to clarify that several comparisons were already included and discussed throughout the manuscript. Since the experimental dataset consists of velocity spectra obtained at discrete spatial locations using a hot wire rake, we chose to focus primarily on whether the CFD model reproduces the main physical mechanisms, spatial energy distributions, and dynamic trends observed experimentally, rather than on direct point by point profile comparisons. Specifically, the manuscript includes the following comparisons with Schliffke et al. (2024):
   - **Inflow and Boundary Layer Validation (Section 3):** The numerical setup was designed to reproduce the strongly sheared atmospheric boundary layer (ABL) conditions reported in the experiments.
   - **Dynamic Threshold for Motion Signature (Lines 447–453):** The numerical results suggest that convective structures within the low frequency range do not yet exhibit sufficient strength to significantly alter the wake signature. This behaviour is consistent with the experimental threshold of $S_t > 0.19$ (reduced frequency $f_{red} > 0.19$) reported by Schliffke et al. (2024) for the appearance of a clear motion signature at $8D$ downstream under ABL flow.
   - **Peak Energy Frequency (Lines 457-463):** The simulations indicate that the highest energy amplification levels occur close to $S_t \approx 0.30$ under ABL flow, in agreement with the peak region identified experimentally by Schliffke et al. (2024).
   - **Spatial Distribution of the Motion Signature (Lines 528–532):** The CFD results show that the highest signature values are concentrated along a vertical line above the disk center (top tip region), which is consistent with the experimental observations reported by Schliffke et al. (2024).

To improve clarity for future readers, the manuscript has been revised to make these comparisons more explicit, particularly in the Introduction and Conclusions.

- *I followed the discussion on unsteady aerodynamic effects together with Reviewer 1 during the introduction and also made a comment myself in the first round. The new formulation: "Schulz et al. (2024) define these as quasi-steady effects, as they are driven by* *the instantaneous apparent wind speed experienced by the rotor due to platform motion." might be misleading, since it could be interpreted that Schulz et al. define the total load oscillation as quasi-steady. It would be beneficial to clarify this further, e.g., "Schulz et al. (2024) state that these oscillations can be considered as a superposition of quasi-steady effects caused by the instantaneous changes in the apparent wind speed experienced by the moving rotor and the impact of potential unsteady aerodynamic phenomena."*

- **Response:** We thank the reviewer for pointing out this potential misinterpretation. We agree that the proposed wording provides a clearer representation of the physical phenomena described by Schulz et al. (2024). Accordingly, the text in lines 30-33 has been revised to clarify that these oscillations arise from a combination of quasi steady and unsteady aerodynamic effects.