Referee Response
Power Output and Downstream Wake Modifications of Turbines Mounted on Tension-Leg Platforms Subjected to Fully Developed Ocean Gravity Waves

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In what follows, we paraphrase referee comments. The comments are immediately followed by our responses in italics.

We appreciate the great care and thoughtful comments and suggestions made by the referees.

Major Comments

- AR: The key novelty is that we use high performance computing and eddy-resolving LES models of stable and unstable atmospheres, a very comprehensive model of the platform mechanics and of the sea and full coupling between these to answer the question of whether turbine output is significantly affected by gravity waves in a fully developed ocean. We are not concerned with the details of the fluid flow, which is the subject of other papers. Rather, with this comprehensive model, we aim to answer a very concrete question: do wave motions of a steady state ocean and their effect on a specific platform/turbine affect power output? We stop short of saying that our contribution is novel, but our literature search did not yield anything comparable

  RC: Thank you for your answer. This better clarifies the idea and objective of the paper. While this answer does not fully satisfy my question, it better clarifies the scope of the paper. So the authors plan to fill the current gap in the assessment of power production of TLP floating wind turbines in realistic conditions using a high-fidelity model that couples wind and wave conditions and an accurate representation of floater dynamics? If that is the case, this could be a reasonable answer to my question. However, this should also be reflected in the manuscript and after this first iteration of review no significant changes have been made to the manuscript to clarify the objective and literature gap.

  RC #2 some improvements have been made to the structure of the paper, however significant issues remain, including repetitions and details being misplaced in different subsections without a clear plan. Some examples concern : 1) part of the work's objective is stated in the chapter about LES simulation 2) details about the precursor simulation and inflow are provided outside of sect. 4.1 3) Some details about the numerical setup are duplicated and shown also in the problem statement 4) Some details about the test case are inserted in a section "Other simulation details" which feels too vague and in general is quite disorganized. I wonder if a section concerning the test case/inflow conditions could be introduced in a way that contains all the relevant information.

  We made substantial changes to the introduction, especially mindful not to repeat ourselves. We still have the atmospheric model details split, but now in Section 4 we highlight modeling aspects that lead to an LES producing the results discussed in the following section. The appendix portion states the LES and focuses on numerical aspects of the simulation.

  RC #3 The authors did not improve the current investigation of results in terms of mean power output or address my question #3. While the scope of the paper is now clearer (focus on the investigation of the mean power of a floating wind turbine) the investigation on this reduced scope remains superficial. I have provided some possible basic questions which could provide a base for a more in-depth the investigation of results. In fact, in the current manuscript the work does not add sufficient details or explanation about the observed trends (view the first round of review)

We thank the reviewer for this comment and agree the topic can use significant improvement. We have reformatted the wind farm simulation results section in the revised manuscript to provide mean horizontal wind speed as well as differences in the wind speed and wind direction between the floating and fixed platform results. We have addressed the source of the differences, which is a combination of chaotic divergence of initially similar solutions inherent to the Navier-Stokes equations, the introduction of active yawing in the farm simulation that is absent in the single turbine simulations, and the presence of flow "streaks" inherent to pressure-gradient forced atmospheric boundary layers that persist even in time averages. Differences between the rows in the time-average are due to time-persisting streaks in the turbulent precursor inflow, and differing power downstream is due to aggregation of chaotic divergence in perturbed wakes due to floating motions as one progresses downstream. On average, the farm power production is 0.27% larger (relative) in the floating case for the farm, which is within an order of magnitude of the roughly 0.1% predicted power production difference ten turbine diameters downstream in the single turbine cases, considering the different turbulence intensities. We have also improved clarity of the figures to show visually what the rows correspond to. While individual turbines may show chaotic divergence in power production up to 4% between floating and fixed platforms, the farm average over all 30 turbines is still quite small in terms of differences in power production between floating and fixed platforms.

Minor comments

- 3. The work of Wang et al, is not contextualized here and the statement feels extraneous to the rest of this section. Please rephrase. It is unclear which modification have been made by the authors, but the work remains not contextualized and extraneous to the rest of the text.

  In the revised manuscript, the paragraph has been modified to provide additional context to the work of Wang et al, and how it relates to the large eddy simulations in the current work.

- 5. Line 58: The term dissimilar feels vague, please provide some more details. Despite the comment by the authors the description has not been improved

  We have updated this description. It reflects how unremarkable the changes were.

- 9. Figure 2 and manuscript, the authors use different abbreviations (i.e., BS CS) without previous definition. Please state the first time the meaning of each acronym and symbol. RC: The authors refer the reader to the work of Betti et al. as indicated in the rebuttal. However, I would say it is not enough to redirect the reader to another work for the definition of the notation, which should be self contained within the manuscript. If no difference is present between this work and the one of Betti et al in terms of implementation another option could be removing the figure and corresponding equations, referring to the original publication.

  We have modified the manuscript to address missing definitions.

- 12. I am unsure how the Actuator disk is used to simplify the turbine dynamics. The actuator disk model provides a model of the wind

  AC: turbine aerodynamics. Please rephrase.

  RC: The answer is not relevant to the question at hand. The text still states dynamics even if a comment has been added to clarify that AD is meant as a simplification of the wind turbine aerodynamics even if the term dynamics remain, which I still believe is not as accurate as aerodynamics

  We apologize for the editorial error in our previous response to this question. We have added text to the section in question to help the reader better understand why actuator disc simulation is simpler and more computationally tractable than actuator line simulation. In short, actuator lines require fine resolution with grid spacings on the order of 1-2m and apply Blade Element Theory to individual blade segments based on local wind velocity, rotation rate of the turbine, local blade geometry, and a fairly complex set of equations to project that into thrust and torque forces. Actuator discs instead rely on 1-D momentum theory combined with reference tables based on actuator line simulations to compute the *applied forces onto the flow based on inflow sampling 2 turbine diameters upstream. We have validated the simpler actuator disk wake deficits against actuator line simulations in Appendix B4.*

- 13. This section is particularly confusing for the reader. The authors describe the test case but then switch within the same section to a description of the employed AD model. I suggest improving the structure of the manuscript. For example, an idea could be dividing the section into two: one section concerning the test case (i.e., definition of the turbine model, farm model and ambient conditions) and one concerning the numerical framework, including (possibly separated) the LES approach and the aerodynamic modelling of the rotor)

  RC: The authors have made some modifications to the manuscript but I believe significant confusion remains in the text and details remain misplaced, such as those concerning the turbine thrust, which are included in the problem statement or details about the boundary layer which are placed in the initial part of Sect. 4.

  We restructured the material in section 4 and the Appendix. Please see the above comments regarding the model definition.

- 15. Line 182, left hand or right hand side? There is only one term on the left hand side off Eq. 17. The authors should better describe the different terms in Eq. 17. How is the platform velocity reduced to a single cosine term?

  AC: addressed what side. Simple geometry is the answer to the question. See Figure.

  RC: The authors have modified the equation and explained the definition the lack of nomenclature does not make the manuscript clear for the reader. I would recommend definying the different terms, following also the comment 9.

  Definition of terms has been added for clarity.

- 24. Line 272, wave? Maybe? In general I wonder if a section should be present to provide some details about wave conditions, i.e., wave height etc, addressed editorially

  RC: In this context it Is not clear what the answer from the authors means. I could not find the requested details in the new version of the manuscript and no justification for this is given in the rebuttal. Please follow up on this comment and add some details about the sea-state conditions. I would imagine that the sea-state could indeed have an impact on the outcomes of this study. For example, more severe sea-states could induce more oscillations in rotor thrust which could aid wake recovery.

  A critical distinction among ocean gravity wave models is whether they are models of waves arising from distant storms and other transient sources of swell and the gravity wave spectrum that arises when the ocean reaches a statistical steady state. The latter accounts, overwhelmingly, of the most common waves, in deep waters, i.e. waves that are smaller than the depth of the ocean on which they arise. The Pierson-Moskowitz spectrum (and its relative, the JONSWAP spectrum for developing conditions) was empirically-derived for the US Navy to describe the 'fully developed' sea state over long enough fetches (regions of oceans). It has over half a century of verification and review, and is widely accepted in oceanography and ocean engineering. Given its long and illustrious history, it was our editorial decision to keep the wave model description and conditions as a matter for the reader unfamiliar with this bit of ocean science to read up on. Nevertheless, the reference is the best starting point for the reader with no background.

  Fully developed conditions lead to or imply that random phase approximations and statistical homogeneity has been reached in the ocean waves. These conditions, however, are widely recognized as being only approximately achieved in the natural setting. We have expanded this discussion in the introduction and in the conclusions section so that someone with no background in wave modeling has a better sense of what we are doing and an improved sense of how our results should be interpreted. But some work is expected from the reader if they are really interested in this topic. For example, the non-expert reader will not know that the more common oceanic condition of a developing ocean, which would require the use of JONSWAP, will not change our results: the JONSWAP spectrum is significantly more peaked and thus closer to the sinusoidal forcing conditions we tried in this paper and this case showed little *changes in the difference between power outputs of fixed and moving platform cases. The synthetic sinusoidal perturbation, however, is notable because it does not have a random phase.*

Most important to our discussion, the PM spectrum takes as sole input the wind speed at a prescribed distance above the mean sea level and produces a full spectrum and therefore a full description of the waves in the ensemble sense. In this work we consider the full range of turbine operating speeds and thus a full range of ocean conditions.

Clearly, consideration of how episodic swells and transient waves can affect power output is worthy of consideration, however, this should be done at much finer resolutions and should be inspired by how these more rarely occurring waves affect platforms in key localities. This point has been added to the text, and further, we posit that our computational framework can handle these more specific scenarios by a simple change in the wave forcing model.

- 26. Additionally, how was the grid size selected?

  AC: Addressed: goal was to get ¿ 10 grid cells across the turbine's swept plane diameter

  RC: The authors have indeed added some details about the selection of the grid size in comparison to the actuator disk. However, it is not clear if the cell size is adequate to correctly solve the wake of the floating wind turbine. This verification would be required to guarantee a high-fidelity representation of the farm output. In fact, if the domain discretization is not sufficient, it could lead to inaccurate prediction of the wake deficits and consequently of the power output.

  The grid spacing of 10m has been validated against actuator line simulations in terms of reproducing an appropriate wake deficit, and the mesh size is consistent with previous studies on actuator line and disk accuracy (Blaylock et al, 2022 and Hsieh et al, 2021). It is therefore expected to provide appropriate modifications to the wake deficit and power generation due to perturbations of the inflow arising from floating platform motions in fully developed ocean conditions as well.

- 27. Line 278 Details about the test conditions (turbulence intensity, wind speeds) should also be moved to a different section.

  AC: addressed editorially

  RC: While a new section "Other simulation details" have been added to the manuscript, I believe that this new section is quite confusing, as it includes both details about the test conditions but also about the numerical set up. I believe the clarity of the manuscript could drastically improve if an adequate section such as "environmental (or test) conditions" would be added and separated from the rest of the details. In general, "Other simulation details" remains too vague and does not support any reader in finding the necessary details from the study.

  We agree that this needed to change to aid the reader in quickly finding necessary information. We have changed the title of this section and distributed the information in a more logical way.

- 31. Line 310, why are the details about the initialization of the boundary layer reported here and not with the description of the precursor simulation?

  AC: addressed editorially

  RC: The relevant section has not been moved despite the answer from the authors. While the authors might disagree with my idea, this should at least be somewhat justified in the rebuttal.

  See above. We redistributed the material. We also made sure that all of the details were in the manuscript so that the interested reader may be able to duplicate our results. (The code is made available).

- 36. Line 351, the authors are referring to the power of the single turbine simulation? Or to Figure 5? If they refer to Figure 5 it should be clarified, as this values are obtained by only using a performance map of the turbine using the wake velocities as input, rather than an AD simulation of a waked turbine. In general, also no comment is provide to Figure 4, for example comparing the different TI levels. In general, since the authors are already simulating a complete wind farm, why was figure 6 included? The figure can provide, for example, a useful comparison with the wind farm result

AC: We clarified that the statement is in regard to all single and multi turbine simulations (thus all simulations) in the paper. Added commentary on Figure 4. Explained why we do different TI levels. Removed Figure 6 and quantified the differences for u inflow > 11 m/s

RC: While some small improvements to the manuscript have been performed the description of the results remains superficial. While the authors wrote explicitly why different turbulent values have been included there is no actual comparison of the results for different turbulence intensity. What can we deduce from these results?

We agree that this can come across as somewhat counter-intuitive, but the results for all turbulence levels are quite small in terms of power differences between floating and fixed platforms, and this is in large part due to the TLP design admitting relatively small motions. When there is any turbulence in the inflow, the effects of floating motions on the flow are small, and we wanted to verify that this is the case in multiple turbulence intensity regimes. This is indeed confirmed in the results. We added the sinusoidal perturbations as an extra exploration to understand the effects of stronger and more coherent floating motions as one would observe in an occasional swell situation. While that resulted in greater motions in the near-wake region than one observes with fully developed ocean conditions at different turbulence intensities, in the far-wake region where another turbine is likely to be, the differences are once again quite small even in the larger sinusoidal forcing simulation.

- 44. For the remaining discussion of the mean power output, please refer to the main comment 4. The analysis should be drastically improved, as the authors state that the focus of the paper is the analysis of the mean power output.

  AC: the editorial changes make this issue addressed

  RC: While the authors state that the issue has been addressed, the description of the mean power output has been not modified and no additional analysis has been provided. In the current state, the analysis remains superficial and does not meet the standards for publication in wind energy science. For example, in Figure 7 the mean wind speed is reported for the motion cases but not for the fixed-bottom case. Could this plot provide additional details to the analysis? Additionally, the variation of power output across the different turbine rows in the motion cases in comparison to the fixed-bottom one remains not addressed in the manuscript. What is happening in the wind farm that could cause this differences, which are not observed when a single turbine is considered? In fact the single turbine results presented by the authors in the previous section showed very small differences in predicted power output for the motion cases, independently of the turbulent intensity levels.

Additional comments

Regarding the single turbine versus farm results, the farm is over a greater area, allows active yawing (yaw is fixed for single turbine simulations), and the perturbations due to floating motions over successive turbines in the streamwise direction amplify due to chaotic divergence inherent to the Navier-Stokes equations as one progresses downstream – something that is not admitted by a single turbine and limited domain size in single turbine simulations.

- In the investigated test case, it has been shown that the mean power output is mainly not affected by the motion of the platform. To provide a valuable contribution, the authors should clearly classify the operating conditions in terms of sea-state as it cannot be assumed that the wake response and consequently the power output of downstream machines will be the same for different wave conditions. Additionally, I wonder if some motion parameters should be reported for the turbines, to highlight the relevant amplitudes and frequencies of motion. This additional data could also aid the comparison of the current results with further analysis in the literature where, for example, different types of platform could be considered, which under the same ocean conditions could show different dynamic responses.

  Reiterating from commentary made above in this referee reply, we considered the full wind speed operating regime of the turbines and thus we explored fully all fully developed multi-spectral gravity wave conditions. We have added some commentary in the conclusions section to emphasize this point. We agree, other platforms should be investigated, with and without active motion control, alas, this would be for another team to investigate.