The new submission of the work by Restrepo et al. has shown only some improvements in comparison to the first submission despite the significant number of issues identified by this reviewer and additional referee. In general, some of my questions have not been addressed, even if the authors state so in the rebuttal. Additionally, the answer of the authors to most of the questions is far from sufficient and requires further elaboration. Please find below all the previous issues that have not sufficiently been addressed and the new issues that have arisen after the authors' modification. In conclusion, I am afraid that the manuscript will not meet the standards of Wind Energy Science if the raised issues are not addressed in an adequate matter. In particular, the main issue lays in the limited analysis of the mean power output results. After the first round of reviews, the authors have drastically reduced the scope of the paper, which now only analyses wake deficits and mean power output; however, the analysis on the mean power output is too superficial to provide valuable contribution to the field. Significant details concerning sea-state conditions, dynamic platform response, and analysis of farm results drastically limit the possible impact of this work.

General 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.

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)

Minor comments (I have maintained the previous numbering to highlight which comments have not been addressed satisfactorily):

3. The work of Wang et al, is not contextualized here and the state-ment 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.

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

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.*

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

AC: turbine aerodynamics. Please rephrase.addressed by saying “deficit” instead of “structure”

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*

13.This section is particularly confusing for the reader. The authors describe the test case but then switch within the same section to adescription 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.*

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.*

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.*

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.*

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.*

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.*

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?*

44. For the remaining discussion of the mean power output, please refer to the main comment #4. The analysis should be drasticly 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

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.