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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1 REF 1

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

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

- In general, the paper represents an interesting contribution to research on floating wind, representing, to the best of the reviewer's knowledge, one of the first LES simulations of a floating wind farm. For this reason, the paper could represent a valuable contribution to Wind Energy Science. However, there are some concerning issues with the manuscript. Below I have identified four main general issues that need to be addressed before publication.

General comments:

1. The novelty and objective of the work are not sufficiently clear. What is the story of the paper? The authors are presenting these results as a first LES simulation of a floating wind farm in realistic wind-wave conditions (i.e., realistic waves, inflow conditions)? The authors want to confirm that motion of the turbine on the wake (and consequently on power of downstream turbines) is small also in a wind farm setting? Or when including the effect of realistic and varying atmospheric conditions (i.e., different TI levels)? For example, previous studies already exist that have shown that the impact of motion on the mean wake deficit of a single turbine is minimal, but there are currently little to no results involving a

farm setting, where the impact of motion could be amplified along a wind farm. Similarly, there is a lack of understanding concerning the impact of realistic motion and atmospheric boundary layer on wake dynamics and rotor-wake interactions of floating wind turbines.

2. The structure of the work needs to be improved. In the introduction and methodology, the authors switch back and forth between different concepts without a clear path within the manuscript. For example, the authors describe the boundary conditions in Sect. 4.1, but additional details about the inflow and initialization are provided within the results in Sect. 5 (line 310). As a result a lot of details are repeated within the manuscript, such as details for the CFL (in Sect. 4, where it probably belongs, but also in Sect. 5). Or for example, details feel misplaced within the structure of the manuscript, such as Figure 10, which shows some proof about the convergence of the simulation in terms of mean power, but is placed at the end of the manuscript, rather than in the methodology, where details about post-processing, convergence and averaging are rightfully presented.

3. The description and analysis of the results could be significantly improved. While I understand that the focus of the analysis is on the mean power output of the turbines, the discussion on the farm power output is limited. To me, for example, it is interesting how for a single turbine simulation, the predicted power output of a downstream machine based on the wake velocities, is predicted to improve by less than .01% (confirming also previous results in the literature, where the impact of motion on the wake is limited), while in a farm setting the power output of the last turbine could increase by 4%. The authors do not try to explain the source of this difference, or why the mean power output increases for row 3 but decreases for row 1 when motion is included, compensating any power gains for the floating wind farm.

4. While the authors suggest in the title and introduction that the objective of the work is to also investigate wake modifications, there is no connection between the mean power output in the farm and wake modifications. Some investigation on wake dynamics in a farm setting would be an interesting addition to the work. Also the analysis does not investigate the frequency response or the formation of coherent structures in the wake which does not support the choice of title for this paper.

Concerning the major comments: the overall takeaway from the ref comments is that the paper needs to be editorially tightened up, clarifying its focus and pointing out its novelty. Ways in which we address this are: (1) since the focus is the power difference between fixed and moving single and ensemble groups of turbines, we changed the title to a narrower scope; (2) we mention in the introduction that our focus problem is to make a comparison of power output using large scale LES computations of the atmosphere, a vetted model capable of yielding the power information, full mechanics of the platform, and

the coupling of the turbine, atmosphere, platform and ocean. 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.

The results do indeed bring up interesting fluid mechanics issues, particularly in the farm configuration, but the pursuit of these is out of scope for this paper, alas, worthy of a possible followup.

Minor Comments:

1. Title: I am not sure that "wake modifications" feels right for this manuscript. Indeed, the authors are looking only ayt the mean wake deficit and not at other wake characteristics, such as frequency spectra, which could be altered by the motion. In fact literature has shown that the impact of motion on the wake under turbulent inflow might be limited to the onset of coherent velocity oscillations at the frequency of motion (at least for sinusoidal motions).

   changed.

2. Line 27, "the motions there impact on floating platform" . I am not sure "impact" is the right word here, please rephrase.

   addressed

3. Line 42, the work of Wang et al, is not contextualized here and the statement feels extraneous to the rest of this section. Please rephrase.

   addressed

4. Line 50, the works by Bergua and Cioni do not investigate the hydrodynamic response of floating platforms. Hydrodynamics were investigated in phase IV of the project, however no reference is included in this sentence.

   addressed. We corrected the bib entry

5. Line 58: The term dissimilar feels vague, please provide some more details.

   addressed

6. Line 75: The authors suggest that the Actuator disk can capture a range of physical phenomena, however, no example from the literature is provided. This statement should be justified. Additionally, at least a sentence should be written in the introduction explaining that the authors are using an AD model for the simulations (connecting it with the discussion about ALM and AD models and different capabilities)

addressed

7. Line 100: Typo "inWei"

   addressed

8. Line 102: it is unclear wehter "this" refers to the present manuscript or to the work of Wei and Dabiri. Please clarify

   addressed

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.

   addressed. The reader is directed to the Betti paper.

10. Line 156, I could not find details about wind drag in Section 4, please clarify

    These terms have been added

11. Line 167: "collection" I would modify to wind farm, to stay consisten with common terminology within wind energy community.

    addressed

12. Line 168: I am unsure how the Actuator disk is used to simplify the turbine dynamics. The actuator disk model provides a model of the wind turbine aerodynamics. Please rephrase.

    addressed by saying "deficit" instead of "structure"

13. Line 168: 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)

    addressed. We made some editorial changes, and removed some repetition

14. Line 174, I would recommend using Ct to identify the thrust coefficient.

    addressed. The expression is not the same as the later one.

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?

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

16. Sect 4, the title refers to wind turbine forces, however, the definition of the turbine model and loads is not present. Please modify the structure of the paper and section titles for clarity.

    addressed.

17. Line 204, please specify to which dimensions the values refer to (i.e, length, height, width)

    addressed

18. Line 205, What does surface mean in terms of "surface density"? Why was this value chosen?

    addressed

19. Line 223, what does "sponge" mean in this context?

    This has been clarified

20. Line 240, no explanation is provided once again for the employed density value. This value does not seem representative for floating wind.

    we used 1, but perhaps 1.22 would have been a better choice. Nevertheless it makes no difference in the power comparison.

21. Line 247, Eq (20). The definition of the wind turbine thrust appears again and with different notation. It is not clear why this is needed and what the difference is across the two equations. In general this makes the manuscript quite difficult to follow. If possible, details concering the turbine modelling should be concentrated within the same sub-section, or the discussion should be significantly improved.

    addressed editorially

22. Line 268, how is the average calculated? At what height?

    addressed

23. Section 5, In general the beginning of Sect. 5 (until sect. 5.1) could probably be almost completely removed, as it presents information that was already stated or that should be included in the methodology, rather than in the discussion of the results.

    addressed editorially

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

25. Line 276, the description of the grid should be carried out in a different section, defining the numerical setup employed, rather than in the discussion.

    addressed editorially

26. Additionally, how was the grid size selected?

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

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

    addressed editorially

28. Line 293, there is a repetition about grid size and CFL, which should anyway be reported in a different section

    addressed

29. Line 303, the term dynamical core is unclear. Please provide further details, which should probably be included in a different section.

    this is standard language in numerical weather prediction, geophysical fluid dynamics, and high performance tools for these.

30. Line 306, the time step should also be provided in a different section, including all the relevant simulation parameters

    addressed

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?

    addressed editorially

32. Line 321, I wonder if "floating" could be used rather than moving/ocean for simplicity *we cannot change it. We want to be sure the reader understands that the difference between the ocean case and the sinusoidal forcing is that the former involves the mechanics of the platform and its interaction with the ocean, the latter is a prescribed perturbation of the flow.*

33. Line 330, this is mostly a repetition of previous sections about the implementation of motion within the AD model. Please remove or move to the section describing the AD model.

    addressed

34. Line 337, why is the value of 19.5 used? This details should be reported in a different section

    see Pearson Moskowitz section

35. Line 348, barred text, typo

    addressed

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 results.

    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

37. Line 360, maybe I am missing something, but I believe there are no panles in Figures 4,5,6.

    unclear

38. In section 5.3 I would just use sinusoidal motion for simplicity rather than sinusoidal/motion. In general, I wonder if this section needs to be separated from 5.2.

    addressed

39. The authors should try to explain the source of difference between the sinusoidal motion and the more realistic wave conditions

    the point is made in the paper that it is not the 'shape' of the motions, but rather the phase average, both being zero, that results in no net difference in power output...and keeps the platform from moving away!

40. Line 374, this statement about the platform oscillations seems conflicting with the one at Line 342. Am I missing something? Please clarify

    see above

41. Line 380, I am not sure I can identify the coherent structures in the wind farm and I am not sure that a time average of 4 hours should show something. Other post- processing techniques might provide further insights into this though, such as frequency analysis. At least the authors should indicate on the figure what they are reffering to.

    Removed the statement. We are not focusing on the wake structure, but rather on differences in power in a fixed versus moving platform turbine structures.

42. Line 382, how is the average speed calculated?

    addressed earlier

43. Figure 8, caption, I would say that turbine ID identifies the turbines in the streamwise direction or something similar, rather than from left to

right, as this can be confusing for the reader. In general, I struggled to follow the descritption of results referring to "IDs" and "Rows" , I wonder if a sentence or mark up in one of the figures could be added in the text explaining this

Clarified in captions and text what this all means

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.

    the editorial changes make this issue addressed.

45. Line 390, the time trace reported in Figure 10, at the end of the paper, to prove convergence should be moved in a different Sect or Appendix.

    Moved figure to appendix

46. Line 396, wake structure seems excessive, as the authors only looked at mean velocities. The investigation of other wake parameters would be interesting, such as TKE or statistics, leveraging LES results, but is not carried out within this work

    Used term wake deficit instead of structure

47. Line 400, in general, It is not clear to me why the three TI levels are reported in the paper, as they are not discussed in the results. For example the authors could compare the mean power outputs under different levels of TI.

    Clarified why we have different TI levels and discussed in text.

48. Line 405, the authors investigated only the mean velocities in the wake and not, for example, the formation of coherent structures. I do not think that these results support the statement that the motion affects the wake structure only minimally.

    Use term deficit instead of structure

49. Line 410, I am not sure that it makes sense having a figure in the conclusions. The figure should reported in a different section, where the amplitude of motion and turbulent oscillations is discussed.

    This is purely a fluke of latex/WES that will be addressed during final formatting.

50. The shape of the Figure 11 spectra for the u induced by the platform is also interesting. Does it makes sense that the spectrum has a significant dip at intermediate frequencies? How is this explained?

    At longer time scales, u_inflow does not vary much, but at intermediate and high, it varies much more than $u\_platform$. So, at long time scales,

u_platform dominates, and at intermediate to fine, u_inflow begins to show influence and eventually dominates. The dips in the spectrum

We elaborated on the interpretation of the figure, specifically as it concerns the power output comparison. Indeed, this is an interesting diagnostic, motivating a followup. However, it is very much floating-platform specific. A more productive and careful discussion, perhaps in comparing different platforms, could be the subject of another paper, and another team of researchers.

51. References: I believe that the format of the references is not consistent. Some authors are presented with the full first name, while for others only the initials are used. Please double-check consistency within the manuscript and with the format employed by WES.

    addressed

2 REF 2

The main objectives of the paper could be made more clear in the introduction. The authors state: Our approach distinguishes itself from the existing literature in its breadth, by the dynamic coupling of turbine/atmosphere and turbines, consideration of different atmospheric turbulent conditions, and consideration of single turbines as well as simple cluster arrangements of multiple turbines.", but what are the expected impacts and new insights from these simulations? What are the questions the authors are trying to answer? FOWTs have been studied for a few years, using numerical simulations with low, medium and high fidelity, and experiments. To the best of my knowledge, most of these studies indicate that the power production of FOWTs does not significantly change with the motion of the platform. So the analysis of the response of a single turbine does not contribute much in my point of view. I believe there are more potential contributions in the analysis of the wakes, which is very superficial in the current version of the paper, and in the analysis of the wind farm. So I suggest further analysis of the results a major re-writing of the corresponding section in order to highlight the novelties that this work might show.

Please see our response to Major Comments of REF 1. We believe we address both the referees' issues. Doing so has improved the paper.

Minor Revisions Below:

- Line 32: remove "wave" after "specific"

  addressed

- Line 59: citation format (use citep)

  addressed

- Line 60: change "often are" to "are often";citation format (use citep)

  addressed

- Line 62: citation format (use citep)

  addressed

- Line 78: citation format (use citep)

  addressed

- Line 86: delete "and their waks"

  addressed

- Line 96: "dynamic coupling of turbine/atmosphere and turbines" - this is confusing, please rephrase

  addressed

- Line 102: change "This" to "That"

  addressed

- Line 123: change "sway suge and yaw" to "sway, surge and yaw" or "sway, roll and yaw", depending on what the authors want to say

  addressed

- Line 172: I believe equation (14) is missing the area of the rotor on the right hand side

  addressed

- Line 211: remove "as well"

  addressed

- Caption of Figure 3: remove "in" (which is already struck through)

  addressed

- Line 333: "eq:uplatform"

  addressed

- Line 581: remove the degree symbol from the values of the temperature, i.e., change to 300 K, 308 K etc. This mistake is repeated in many other places in the paper, please perform a thorough check.

  addressed

- Line 584: portUrb is first mentioned here, but this is a key information of the study. Please introduce the software earlier in the paper.

  Added reference to this sooner in the paper with the proper citation now that the portUrb paper is published.

- Employ the simple past or present perfect verb tenses to describe what you did. For example, in line 293: In all simulations in this section, a CFL stability value of CFL = 0.6 was used, and a grid spacing of 10m in each direction was used. Identical turbulent precursor inflow conditions were used for all simulations of a given hub height mean wind speed to ensure the only wake and power differences between simulations with and without floating platform motions are due solely to floating platform motions.

  addressed