The paper “Power Output and Downstream Wake Modifications of Turbines Mounted on Tension-Leg Platforms Subjected to Fully Developed Ocean Gravity Waves” by Restrepo et al. presents the simulation of a floating wind farm using Tension Leg Platforms in realistic atmospheric and wave conditions. The authors employ an LES approach coupled with an Actuator Disk model to represent the turbine rotor. Both a single turbine and a farm are considered in the simulations. The focus is on the mean power output of the turbines and farm under realistic and sinusoidal motion and different inflow conditions. 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. I have then provided more point by point and detailed comments in the following section.

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.

Specific comments

In addition to the general comments presented above, additional specific comments are provided below:

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).
2) Line 27, "the motions there impact on floating platform". I am not sure "impact" is the right word here, please rephrase.
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.
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.
5) Line 58: The term dissimilar feels vague, please provide some more details.
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)

7) Line 100: Typo “inWei”
8) Line 102: it is unclear wehter “this” refers to the present manuscript or to the work of Wei and Dabiri. Please clarify
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.
10) Line 156, I could not find details about wind drag in Section 4, please clarify
11) Line 167: “collection” I would modify to wind farm, to stay consisten with common terminology within wind energy community.
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.
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)
14) Line 174, I would recommend using Ct to identify the thrust coefficient.
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?
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.
17) Line 204, please specify to which dimensions the values refer to (i.e, length, height, width)
18) Line 205, What does surface mean in terms of “surface density”? Why was this value chosen?
19) Line 223, what does “sponge” mean in this context?
20) Line 240, no explanation is provided once again for the employed density value. This value does not seem representative for floating wind.
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.
22) Line 268, how is the average calculated? At what height?

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.
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,
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. Additionally, how was the grid size selected?
26) Line 278 Details about the test conditions (turbulence intensity, wind speeds) should also be moved to a different section.
27) Line 293, there is a repetition about grid size and CFL, which should anyway be reported in a different section
28) Line 303, the term dynamical core is unclear. Please provide further details, which should probably be included in a different section.
29) Line 306, the time step should also be provided in a different section, including all the relevant simulation parameters
30) Line 310, why are the details about the initialization of the boundary layer reported here and not with the description of the precursor simulation?
31) Line 321, I wonder if "floating" could be used rather than moving/ocean for simplicity
32) 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.
33) Line 337, why is the value of 19.5 used? This details should be reported in a different section
34) Line 348, barred text, typo
35) 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.
36) Line 360, maybe I am missing something, but I believe there are no panles in Figures 4,5,6.
37) 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.
38) The authors should try to explain the source of difference between the sinusoidal motion and the more realistic wave conditions

39) Line 374, this statement about the platform oscillations seems conflicting with the one at Line 342. Am I missing something? Please clarify
40) 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.
41) Line 382, how is the average speed calculated?
42) 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
43) 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.
44) 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.
45) 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
46) 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.
47) 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.
48) 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. 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?
49) 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.