Authors' response to referee's and public comments.

Referee #1

The paper deals with stationery unmoored floating wind turbines. This topic is very relevant to wind turbines deployed in very deep waters far offshore where mooring costs would be high. The paper initially presents a very good overview of relevant literature. It then applies simple actuator disc theory for wind turbines to evaluate the impact of rotor design on the energy yield and cost of such turbines. The quality of the writing is overall very good and grammatically mistakes are very limited. The presentation of graphs and tables is also very clear. However the following major comments are being brought forward:

Thank you for the feedback and comments. Please find below our replies.

1. Section 2: The study ignores the hydrodynamic loads of the waves when sizing the thrusters. This assumption is unrealistic, considering that at high wind speeds the wave induced loads on the floating platform are often larger in magnitude than the wind turbine thrust.

We agree that wave induced loads can be greater than wind turbine thrust. However, we disagree that it is an unrealistic assumption to neglect wave loads when sizing the thrusters because their effect does not need to be compensated by the thrusters (line 93 of the manuscript).

(i) Indeed, the effect of first order wave loads is essentially platform motions at frequencies equal to that of the waves and - most importantly - whose time averages are zero (which is why the thrusters do not have to compensate those first order loads),

(ii) The drift forces (second order forces) are small when using a sufficiently transparent floater (e.g a semi-submersible platform with a reduced water plane area), (line 98 of the manuscript).

In practice, (i) may be achieved by filtering out wave-frequency motions in the thrusters controller. This approach was successfully implemented in [Xu et al., 2021] as can be seen in Figures 1 and 2 below. Figure 1 shows the wind turbine thrust force. Figure 2 shows the thrusters force. One can see that after the initial transient, the thrusters force is relatively stable and very close to minus the wind turbine thrust force.

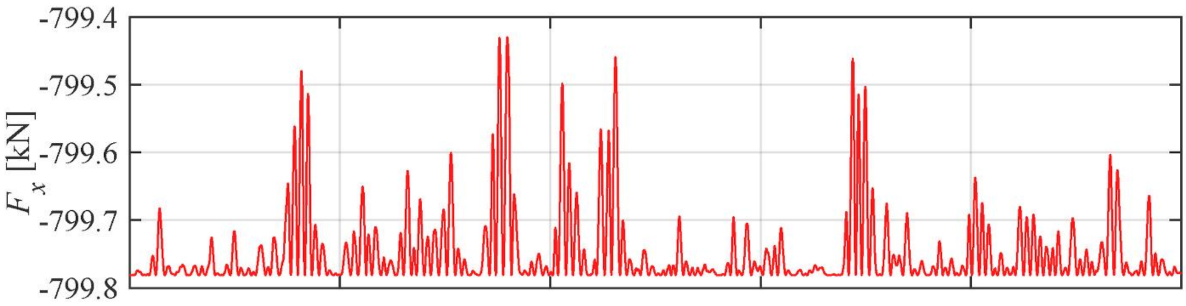

Figure 1. wind turbine thrust force as function of time. The picture is extracted from Figure 13 in [Xu et al., 2021].

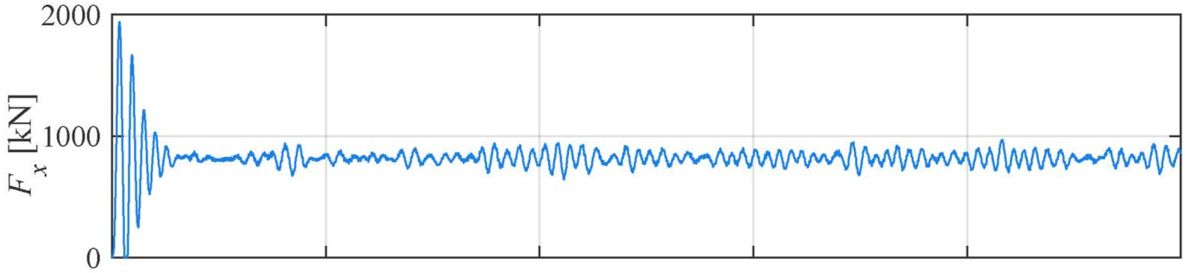

Figure 2. thrusters force as function of time. The picture is extracted from Figure 15 in [Xu et al., 2021].

Regarding (ii), we have run computations of wind loads and drift forces for the NREL 5MW wind turbine mounted on the OC4 semi-submersible platform.

- The wind loads are computed using Ct table from the NREL 5MW reference article,
- The wave loads are computed using the 2nd order QTF for the OC4 semi-submersible platform. The QTF were computed using the software Nemoh. The wave spectrum is the Pierson-Moskowitz spectrum.

Figure 3 shows the drift force (called wave load in the figure), wind loads (rotor+tower), total load and the ratio of the drift force to the total load. The results show that the wave drift force is less than 10% of the total load in most of the operational region (max 15% percent for the highest wind speed). Moreover, the OC4 semi-submersible is quite oversized for the 5MW turbine. Therefore, this ratio is expected to further reduce as the turbine diameter grows and as the floater gets thinner.

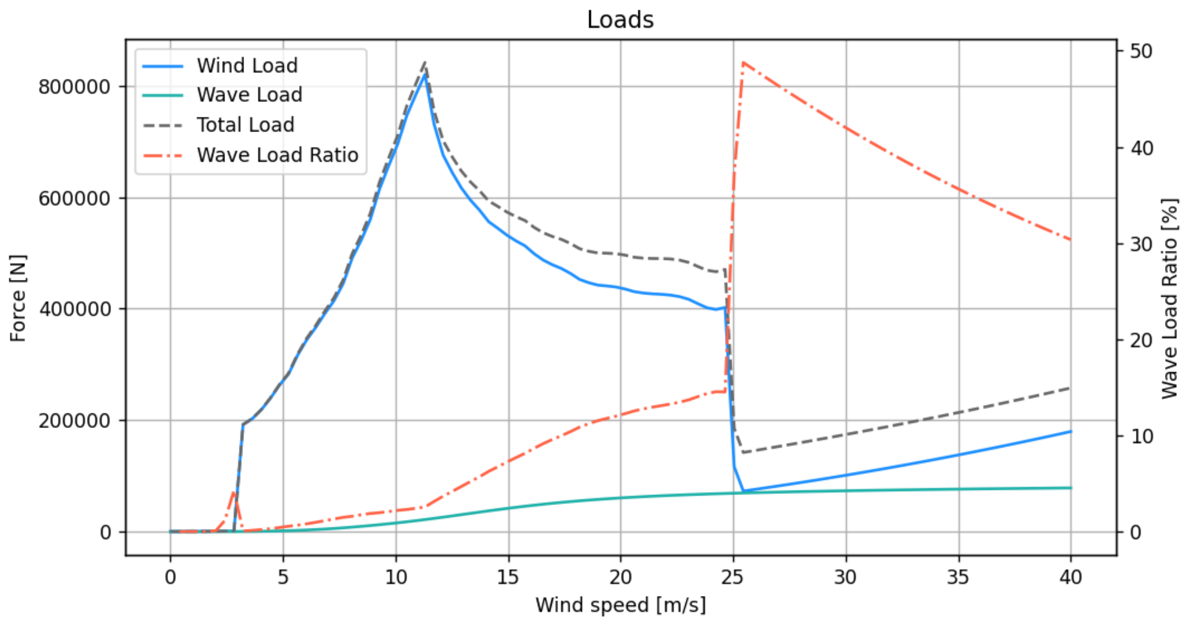

Figure 3. wind load, drift force, total force and ratio of drift force to total force as function of wind speed for the NREL 5 MW wind turbine mounted on the OC4 semi-submersible platform

Therefore, we disagree that it is an unrealistic assumption to ignore hydrodynamic loads for the sizing of the thrusters.

2. Section 2.1: The formula for the thrust coefficient CT derived from the momentum theory is only applicable for low inflow factors. For induction factors above ≈0.38, the momentum theory is invalid

and CT increases linearly with the induction factor. This fundamental limitation of the simply actuator disc theory is not factored in the study.

This limitation was not mentioned in the initial submission as the optimization led to select inductions factors lower to 1/3. Nevertheless, we agree with the reviewer that it is important. Therefore, we added the following text at the end of section 2.1 of the manuscript:

> For induction factors above ≈0.38, the actuator disc theory is invalid (the thrust coefficient increases with induction factor) (Glauert, 1926). Therefore, in the present study, induction factors are limited to the range [0, 0.4].

Figures 3 to 6 were updated accordingly (only results for induction factors between 0 and 0.4 are shown).

3. Section 3.1: The derivation of Eqt. (7) is not explained in enough detail. It is unclear how this equation for the turbine power is derived by equating the wind turbine thrust to the thrust of the dynamic positioning thrusters.

The details of the derivation are as follows:

Eq. 3: $T_{\mathrm{WT}} = \frac{1}{2}\rho_a A W^2 C_T$

Eq. 6: $P_{\mathrm{T}} = \frac{N}{D}\left(\frac{T_{\mathrm{T}}}{KN}\right)^{3/2}$

Equating the wind turbine rotor thrust (Eq. 3) and the thrusters delivered thrust in Eq. 6 (line 129 of the manuscript):

$$P_{\mathrm{T}} = \frac{N}{D}\left(\frac{\frac{1}{2}\rho_a A W^2 C_T}{KN}\right)^{3/2}$$

$$P_{\mathrm{T}} = \frac{N}{D}\frac{\frac{1}{2}\rho_a A W^2 C_T}{KN}\sqrt{\frac{\frac{1}{2}\rho_a A W^2 C_T}{KN}}$$

$$P_{\mathrm{T}} = \frac{1}{KD} \times \frac{1}{2}\rho_a A W^2 C_T \times W\sqrt{\frac{1}{2}\frac{\rho_a A C_T}{KN}}$$

$$P_{\mathrm{T}} = \frac{1}{2}\rho_a A W^3 \times C_T \times \frac{1}{KD}\sqrt{\frac{1}{2}\frac{\rho_a A}{KN}C_T}$$

$$P_{\mathrm{T}} = \frac{1}{2}\rho_a A W^3 \times C_T \times \sqrt{\frac{1}{2}\frac{\rho_a A W^2}{K^3 D^2 N}C_T}$$

Using $C_T = 4a(1-a)$:

$$P_{\mathrm{T}} = \frac{1}{2}\rho_a A W^3 \times 4a(1-a) \times \sqrt{\frac{1}{2}\frac{\rho_a A W^2}{K^3 D^2 N}4a(1-a)}$$

$$P_{\mathrm{T}} = \frac{1}{2}\rho_a A W^3 \times 4a(1-a)^2 \times \sqrt{2\frac{\rho_a A W^2}{K^3 D^2 N}\frac{a}{1-a}}$$

Which corresponds to Eq. 7

4. The impact of rotor design is solely based on the disc average axial induction factor. How this will impact the rotor solidity, mass and cost is not assessed in enough detail. Thus the cost model is not convincing.

We agree that modifications to the rotor design can be expected to impact its cost per unit of rated power, which is not taken into account in our study.

However, to our knowledge, there is no easy way to capture this effect; and it is beyond the scope of the present to develop a cost model for a rotor as function of its induction factor.

Therefore, as both you and the second referee were not convinced by the cost model, the cost of energy section has been replaced by a discussion of implications of rotor induction optimization (and peak shaving) on design and cost of energy.

Authors' response to referee's and public comments.

Referee #2

The paper presents an analysis of offshore floating turbine rotors, using an actual disk approach, for unmoored turbines that use small underwater propellers for stationkeeping. The work presents a nice literature review of relevant work so far and provides an effective first-principles analysis to prove some key points. However, the follow-on cost model and discussion is not journal-quality work, in my opinion. Additionally, I am not sure how the authors would be able to generate a more meaningful cost model using an actuator disk level of fidelity for the rotor. Maybe the NREL Cost and Scaling model could help with its regression-based approach, but those correlations are quite old and definitely not meant for exotic floating wind turbines. In the rest of the paper, while the first principles analysis does have some novelty, it probably isn't enough to hold the paper up on its own. Thus, a significant improvement in the paper must be made prior to publication.

Thank you for the feedback. As both you and reviewer #1 found the cost model not convincing, we made a major revision of this part. We replaced it by a discussion of the likely impact of changing rated rotor induction on cost of energy.

Moreover, following your comment #10, we took into account the effect of peak shaving. It adds a significant amount of new material, as for example the new Figure 8 below (in which κ corresponds to the peak shaving coefficient). The impact of peak shaving on cost of energy is also discussed in the new discussion section.

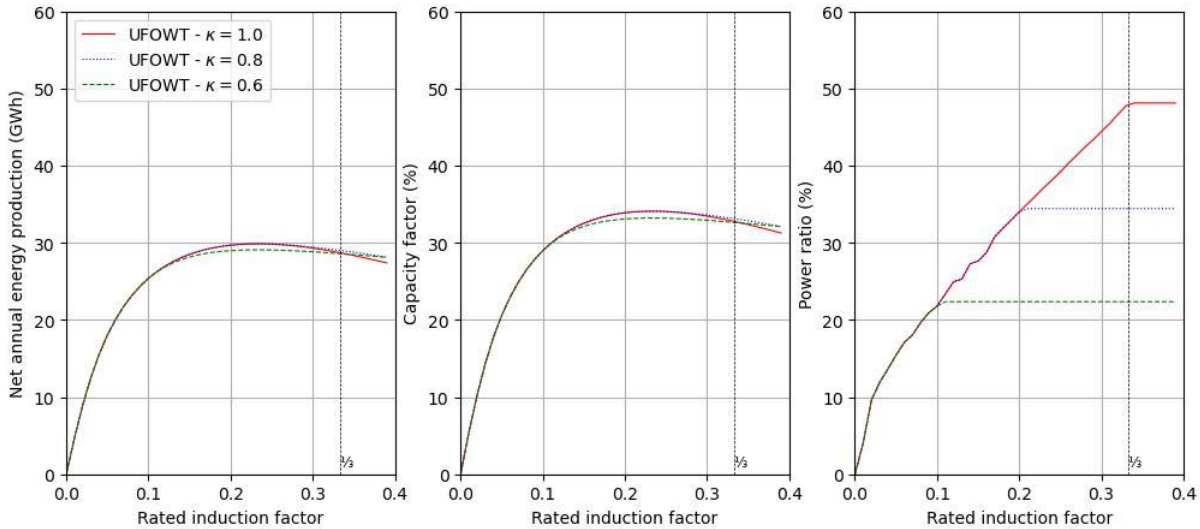

Figure 4: effect of rotor induction and peak shaving on energy production and power ratio.

We hope that those major changes will meet your expectation of significant improvement in the paper.

Moreover, please find below our replies to your detailed comments.

Detailed comments are as follows:

1. I cringe a bit at the use of "rotor design" to describe the work in this paper. Most journal papers and textbooks that deal with aerodynamic rotor design discuss chord, twist, pitch schedule, rpm, solidity, torque, etc. While those works are often grounded in BEM theory as well, this paper uses idealized actuator disk theory for optimizing overall induction targets and the size of the rotor

diameter relative to thruster diameter. A more appropriate title might be "Effect of Rotor Induction on Energy Performance and Cost of Stationary Unmoored Floating OffshoreWind Turbines", with similar find-replace changes from "rotor design" to "rotor induction".

Agree, thank you for the suggestion. “Rotor design” has been replaced by “rotor induction” wherever relevant.

2. I cringe also at some of the simplified generalizations used in the language of the paper. For instance, in the abstract "...induction factor is smaller than the usual value of 1/3". What is "usual value"? I understand that is the optimal induction from classical actuator disk theory, but I encourage the authors to be more complete and precise with their language. If I were to optimize a rotor design using a high-fidelity model that captures all of the complex aerodynamics in a realistic atmospheric inflow, I wouldn't necessarily arrive at a uniform 1/3 induction factor across the full rotor diameter. There are other instances with similar generalizations. A professional technical editor might help to call these out too, in addition to some other small grammar issues.

Agree. The necessary changes have been implemented throughout the paper.

Abstract:

~~Results show that the optimal rated induction factor is smaller than the usual value of 1/3 both from the perspective of energy performance and cost of energy. Thus, wind turbine rotors designed for SUFOWTs should be developed to optimize their cost. However, results show that the cost of energy reduction is somehow limited, of the order of 2.5 to 4.3% for the considered designs.~~ Results show that the rotor induction which maximizes net power production (which takes into account the thrusters power consumption) is smaller than the value of 1/3 which maximizes wind turbine power production. However, the increase in annual energy production or capacity factor brought by rotor induction optimization is rather small, of the order of a few percents. The effect of peak shaving was also found to be small with respect to energy production and capacity factor. Both rotor induction and peak shaving were found to be able to reduce significantly power ratio (ratio of thrusters nominal power to the wind turbine rated power), which can be expected to be beneficial for cost of energy.

Section 2.1:

It is well known that the theoretical maximum power coefficient of a wind turbine is CP,max = 16/27 (Betz’s limit). It is achieved for induction factor a = 1/3. ~~Rotors of conventional wind turbines are optimized to get as close as possible to that CP,max. However, it~~ Maximum power coefficient comes at cost of high thrust coefficient as can be seen in Figure 3. While it is not a critical challenge when station-keeping does not consume power (as for onshore, bottom-fixed or moored floating offshore wind turbines), the situation is different for UFOWTs as will be shown in what follows.

As for the help of a professional technical editor to address grammar issues, it is included in the publication workflow of Wind Energy Science (after paper acceptance though: https://www.wind-energy-science.net/about/article_processing_charges.html).

3. I do not follow the logic of the argument in the last two sentences of the abstract. Without knowledge of the work in the paper, these sentences don't make sense and the comment, "Thus, wind turbine rotors designed for SUFOWTs should be developed to optimize their cost" is probably another example of an over-simplified generalization.

Agree. This has been removed and the end of the abstract has been rewritten (see reply to previous comment).

4. Page 3, paragraph around line 50 changes from past to present tense.

Agree. The necessary changes have been implemented (also the help of a professional technical editor will make sure that the changes are correct).

5. Line 58, the second bullet, "The DP system design: it should include as many large diameter thrusters as practicable to maximize their thrust generation efficiency" seems overly broad and without context. For instance, the statement ignores cost, reliability, feasibility of incorporation on the floating platform, etc. Maybe this is included in "as practical", but another sentence would help quite a bit.

We are not sure to understand this comment. The context of the bullets is parameters that have a significant effect on **energy efficiency** (Line 57: From the previous literature, it can be concluded that the **energy efficiency of stationary unmoored floating offshore wind turbines depends significantly on:**). In this text, practical essentially refers to the feasibility of incorporation on the floating platform (cost and reliability aspects mentioned by the reviewer are not related to energy efficiency). To clarify, we added:

> *Note that practicable corresponds essentially to the feasibility of incorporation on the floating platform.*

6. On page 4, the diagram and list of forces on the UFOWT is fine, but it isn't really used at all in the paper. The following long list of assumptions on page 5 sets up the paper to ignore all but two of the terms. I do also do not agree with the assumption, "The aerodynamic drag force is neglected, which is quite acceptable in region II and III". It is quite possible that other readers will take issue with other assumptions. In the end, the point seems to be that the authors want to do an analytical actuator disk analysis comparing the platform thrusters vs the rotor thrust. The other force in the model would add enough complexity that an analytical analysis would be too difficult. However, that statement and motivation is not stated outright, and it should be. Why not just state at the beginning of Section 2 that, "We will use an actuator disk model of the wind turbine, balancing rotor thrust with the onboard propellers, to identify optimal induction factors for UFOWT configurations. Therefore, all of the assumptions of actuator disk theory apply, and we will ignore the other forces on the structure. Assumptions include... Other forces include..."

We would prefer keeping the diagram and list of forces. Indeed:

- we think that it is useful for future research on UFOWTs to have the list of forces that have to be considered,
- we think that our approximations are meaningful (not just for convenience). Regarding aerodynamic drag, it was shown in Santarromana et al. 2024 that it is negligible in Region II and III. We are not aware of any other study showing that it is not the case. (see also our answer to the comment #1 of referee #1).

7. Line 115, "Rotors of conventional wind turbines are optimized to get as close as possible to that CP,max" seems overly simple. There are many factors that go into rotor design (cost, structures, longetivity, manufacturability, etc) and some OEMs, especially those from China, prefer to minimize cost above all else and live with suboptimal power production.

Agree. This sentence has been removed (see also answer to comment #2).

8. Page 6 Figure 3. BEM theory is known to lose applicability in high thrust, high induction scenarios. I would cap your x-axis at a=0.4 as the higher thrust scenarios aren't relevant to the paper anyway. See Figure 4 from https://doi.org/10.1002/we.2688 . This same suggestion would apply to the later Figures 4 and 6.

Agree. The x-axis has been capped at *a=0.4.* (see also answer to comment #2 of referee 1).

9. Section 3 is nice work and makes convincing arguments, good work.

Thank you ☺

10. Page 9, Figure 5 and discussion. At some point in the paper, probably here, it is worth mentioning that all modern turbines use peak thrust shaving to avoid the sharp apex in thrust near rated speeds. This would noticeably reduce the burden on the station keeping propellers and perhaps increase the optimal induction in your actuator disk analysis.

Agree. Following your comment, we took into account peak shaving in our study. Indeed, it has an effect on optimal induction as can be seen in Figure 4.

Taking into account peak sheaving lead us to implement significant changes in the text in section 3.2 (power curve definition) and section 3.3 (annual energy production). It also lead to the addition of two new figures (Figure 6 and Figure 8 in the revised manuscript).

11. Page 10, Line 181 and Figure 6. Why is the chosen optimal surface ratio so small? There was a clear statement earlier that the larger thrust diameter, the better and more efficient the station keeping. What am I missing?

That surface ratio was chosen because it corresponds to the surface ratio that was found to minimize the cost of energy in [Santarromana et al., 2024] (line 183: as for the cost-optimal design found in Santarromana et al. (2024)). To clarify, we rephrased:

> *the surface ratio is taken equal to δ=0.0037 to match that of the cost-optimal design found in Santarromana et al. 2024.*

Note that even if *δ=0.0037* may seem mall, it would take four DP thrusters of 5 m diameter to be achieved. (Each DP thruster costs 1,765,500 USD).

12. Same comment on Page 11, first sentences in section 4, "it is possible for a SUFOWT to achieve power performance close to that of a conventional FOWT provided that the surface ratio is small enough. However, minimizing the surface ratio does not necessarily lead to the lowest Levelized Cost Of Energy (LCOE)". What am I missing? Should thruster diameter be big or small?

There were typos here: it should read:

> It is possible for a SUFOWT to achieve power performance close to that of a conventional FOWT provided that the surface ratio is **sufficiently large**. However, **maximizing** the surface ratio does not necessarily lead to the lowest Levelized Cost Of Energy (LCOE)

13. Page 12 Line 212, "they estimated the LCOE aboard the wind turbine to be". "aboard" is not the right word choice here. Perhaps just replace with "for".

As mentioned previously, the cost of energy part has been replaced by a discussion of the impact of rotor induction and peak shaving on cost of energy. Therefore, this comment and subsequent no longer apply.

- Cost model issues:

  + "Therefore, we used 3600 - 8000 USD/kW for CAPEXFOWT in Equation (18)." There really is no more information given than this. How is this range apportioned to the different turbine sizes? How does the cost vary by induction factor? A higher induction factor and more thrust would likely require a more sturdy floating platform, correct?

  + What is being optimized here? What are the design variables and their bounds? What are the constraints? What is the merit/objective function? Is there a mini-optimization run for every point in the induction factor sampling?

  + The results in Figure 15 have all of the turbines on top of one another for LCOE, capacity factor, etc. There is no way a 5MW turbine and a 15MW turbine should be identical in that regard with such different specific power values.

  + The thruster diameter is identical in all cases and only the number of thrusters change? No explanation as to why the model chooses this option.