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Review and rebuttal of the paper

Investigation of the Wave Field Around a Monopile Due to Long Crested Irregular Waves in Moderate Steepness

Herdayanditya et al.

Editor handling the paper: Miguel Esteban

The reviewers remain anonymous.

ROUND 1

Reviewer A:

The manuscript focuses on understanding how waves behave around large monopile foundations for offshore wind turbines. It combines experimental data with theoretical models to explore the impact of wave characteristics (steepness, diffraction number) on the wave field. The findings highlight the importance of considering nonlinear effects, especially for wave crest properties, and the role of circular wave patterns (Wave Type II) in this phenomenon.

The results are very encouraging and generally eligible for publication in the journal. However, there are some minor points that the authors are advised to revise the manuscript with consideration of comments given below:

Response:
Thank you for your general remarks and advice. We will address the minor points in the corresponding answers.

Minor comments:

Rayleigh distribution (Section 2.3.1) is employed as the theoretical estimation of the wave height and crest exceedance probability. However, this distribution is often used when wave heights are generated by many small, independent wave components while Weibull distribution is highly flexible distribution that can take on various shapes depending on its parameters.

Response:
We agree that the Reyleigh starts from assumption of many small independent wave components. Our Wave Field around a Monopile (WFAM) calculation is derived with the free surface linearization (i.e. low wave steepness, thus low wave assumption). Therefore, we need to consistently use this assumption to describe the linear WFAM distribution for the irregular wave cases, using Rayleigh distribution. Nevertheless, in the revised manuscript we provide a distribution correction based on Forristall (2000) and Gramstad and Lian (2024), where the former applies the Weibull distribution for the ocean waves. L128 – L130 mention both distributions along with their equations in newly-added Appendix A. The two methods were applied and discussed for the incident waves and the WFAM in Figure 8 and L 267 – L286 along with its limitation. The correction method is also applied to correct the significant crest introduced as the practical solution (L292 – 296). It shows better results than the linear theory as shown in Figure 9 and the explanation is given in L307 – L311.

In our discussion, we also mention the possibility to apply Weibull when doing parametric study, as a way to correct the distribution more robustly. (L370 – L378).

Similarly, the choice of wave spectrum (e.g., JONSWAP) can affect the accuracy of LTF-based predictions. JONSWAP wave model can describe the frequency spectrum of ocean waves, particularly under stormy conditions. However, it is more complicated and lead to computational cost as mentioned in the introduction. Why didn't authors use other simpler model like Pierson-Moskowitz in parallel with moderate steepness condition of the wave (Section 2.2)? It is therefore necessary to have a review or explain the reason of these choices.

Response:
The main reason we selected JONSWAP than Pierson-Moskowitz is due to its possibility to account for unmature waves due to limited fetch length, via their peak parameter (γ). This is also considering that some offshore wind turbines are in the limited-fetch area (e.g. Belgian Part of the North Sea). Therefore, JONSWAP would be more suitable than the Pierson-Moskowitz. JONSWAP can also be seen to be more general than Pierson-Moskowitz because Pierson-Moskowitz can also be achieved via JONSWAP spectrum by setting $\gamma = 1.0$, describing fully developed seas, (e.g. open ocean). We elaborate this in our manuscript in L161 – L165. Concerning the computational cost, there is not much difference between computing the JONSWAP model or Pierson-Moskowitz.

L37-38 Please specify the limitations of current nonlinear models in terms of computational cost and accuracy as described in this sentence.

Response:
We provide an example of the required time for the regular wave simulations from the literature in L38 – 42.

L100 The use of acronyms and abbreviations such as FFT, should be replaced with full text for the first time (Fast Fourier Transform).

Response:
We applied the suggestion accordingly at L105

L127 There are Eqs. (15a) and (15b). Thus, Eqs. (15) is unclear.

Response:
We updated the sentence so that it becomes clearer in L135.

L149 Eqs. (7) is unclear since there are (7a), (7b) and (7c).

Response:
We specified it more clearly in L161. Furthermore, we also updated the equation referencing accordingly in L92-93.

Some figure (e.g. 8d, 9b, 9c) aren't refered in the content of the paper.

Response:
We added one more figure as suggested by the other reviewer. Therefore, the previously referred to Figure 8d, 9b, and 9c become Figure 9d, 10b, and 10c. We used these new numbers in the revised version we provide.

Figure 9d is now referred in L302

Figure 10b and 10c are now referred in L345

Typos:

Abstract: "Further, time series analyis".

Response:
This typo is now revised in the new version

L187 “The freqeuncy domain”

Response:
The typo is now revised as seen in L206 and L225

Recommendation: Revisions Required

Response:
The track-change revised version is attached.

Reviewer B:

The objectives of the study are clear and the results are logical.

Response:
Thank you for the remarks

The overall conclusions must though be made much clearer. The paper does not give clear guidance on how to assess the wave field around monopiles.

Response:
In the revised manuscript, we split the conclusion into two paragraphs to provide more clarity in the conclusion, especially on the guidance with the use of linear theory in the WFAM (L390)

What tools/methods can be used and when are they not valid anymore? How do we for example predict if there is risk of breaking waves near the monopile or not? This I assume is a key aspect of planning O&M.

Response:
We elaborated further our findings on wave height and wave crest validity corresponding to specifics marine operation cases in the paragraph 2 of the conclusion, L391 – L410. Please note that we introduce a newly added practical correction method to WFAM based on Forristal. We discover this while answering your remark about Forristall (2000) in your Figure 7 comment. Thus, we would like to thank you for igniting the idea. The correction method is demonstrated in Figure 8 and Figure 9, explained in L267 – L286 and L306 – 311, along with its observed limitations.

In the conclusion, we mentioned that linear theory is not valid to estimate the crest significant. Therefore, the limitation to use linear theory depends on the need of what evaluation is needed for a specific marine operation. We elaborated this in L400 – L406. The breaking wave aspects is also mentioned in the conclusion L407 -L410.

Below additionally some additional minor comments:

Line 42: Eq. 1 cannot correspond to Bernoulli with alpha = 1. In that case wave height should be replaced by the crest elevation.

Response:
We thank you for mentioning this mistake. It is now corrected into η_C in the revised manuscript (Eq. (1)), along with its definition in L45

Line 51: Repeated word "steepness steepness"

Response:
Indeed, that is a typo. Thanks for pointing it out. The repeated word is now removed in L55

Line 89: Typically epsilon change from 0.07 to 0.09 at the peak frequency. Why did you choose a fixed frequency?

Response:

This is again a writing error which we thank you for addressing this. We rechecked our code and the computation already applied the change of epsilon after the peak frequency (Figure B1). We updated accordingly in L93.

```
for ii in range(len(omegaNode)):
    if omegaNode[ii] <= peakOmega:
        eps_spec=0.07
    else:
        eps_spec=0.09
```

Figure B1. Code script to define the ϵ_S in JONSWAP computation

Line 105: Why you use both C and Coh. for coherence?

Response:
It should be Coh., which has been changed accordingly. We updated in L110

Table 1: w is later used for the length scale (w = 25, cf. line 142-146). Here w must mean time scale? Please be consistent.

Response:
Indeed, the Froude's length scale, defined as w, is 25 while the Froude's time scale is $\sqrt{w}$. New caption is rewritten for Table 1.

Line 135: What is meant by 'sweet spot'? You mention spurious waves, but spurious waves are typically related to the discretization of the wavemaker. As wavemakers produce head-on waves only, there should not be any significant spurious waves (only due to the active absorption correction).

You also mention diffraction effects, but are they really smallest in that position? Have you evaluated that by diffraction diagrams or other methods? What are the small guide walls you consider diffraction effects from? Can you add a description of them in Fig. 1. Have you also considered the diffraction from the north absorber beach? It seems like you might be in the region where the diffraction coefficient fluctuate between 0.88 and 1.17 depending on the wavelength. Thus, I would assume the diffraction effects would be smaller nearer to the east wavemaker. Thus the model could have been placed near the east wavemaker, but outside of the wavemakers nearfield.

Response:
The sweet spot is the location where the effect of spurious waves is minimum. We used the term spurious simply to describe unintended waves, where the causes/sources are still under investigation. Considering the confusion that might arise by using the 'spurious' terminology, we changed the term into unwanted waves (L144). We tried to minimize the unwanted waves with our best knowledge by implementing, such as:

1. Simple head-on direction (as what you mentioned)
2. Moderate wave cases (as the wave pedal can produce free waves in highly nonlinear waves)

In our incident wave analysis, we still observed such a non-uniform wave pattern along the tank, through our regular wave investigation. The regular wave analysis was part of our preliminary study of the present paper to properly design our experiment. With the regular waves, the location of the monopile is selected with design process as the following:

1. We start by dividing the wave basin into four quadrants, Q1 – Q4 as shown in Figure B2. They are the possible locations to place the monopile. The center of the basin was not possible due to practical reasons during our tests.

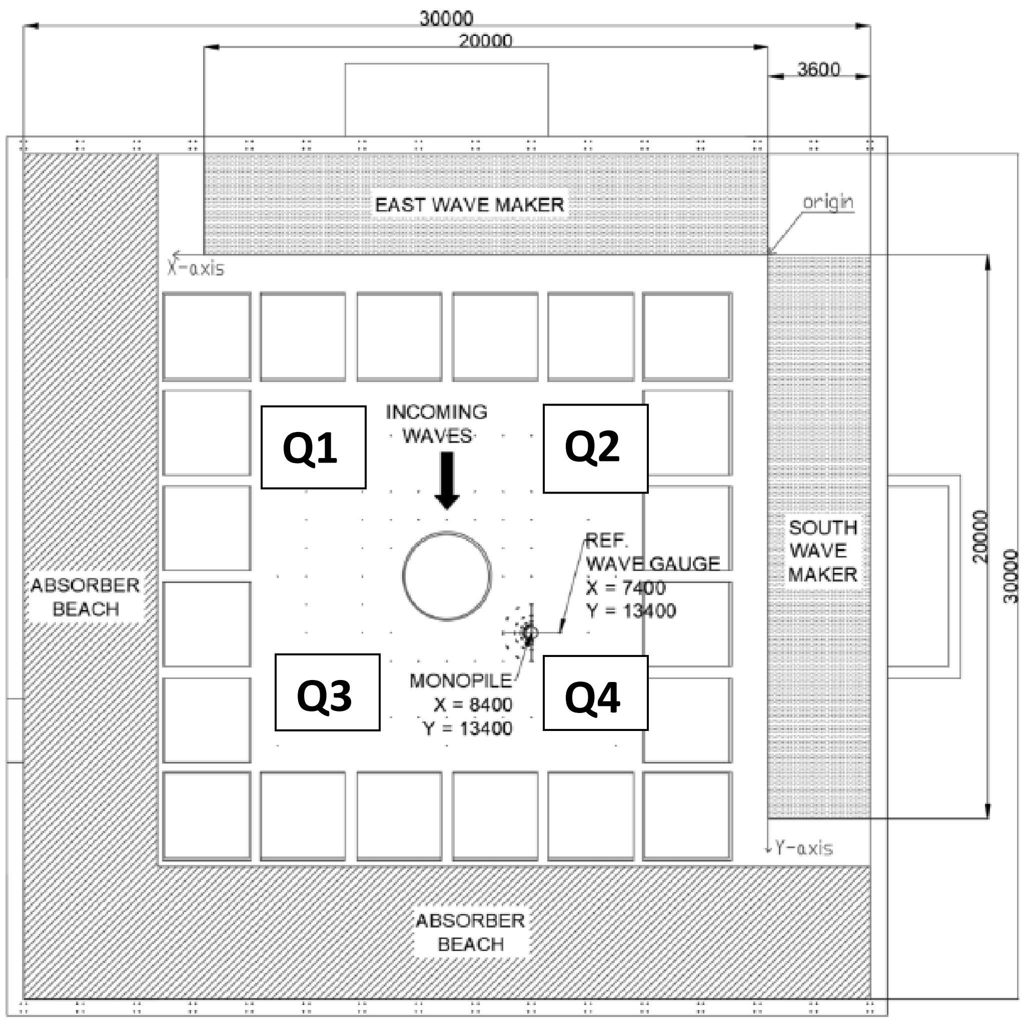


Figure B2. Quadrants division to select the most optimized spot of the COB basin.

2. We observed significant dissipation in Q1 and Q3 because of the absorbing beach on the side, causing late arrival of wave front. An example of the bended wave crest (Figure B3) Therefore, the options are left to Q2 and Q4.

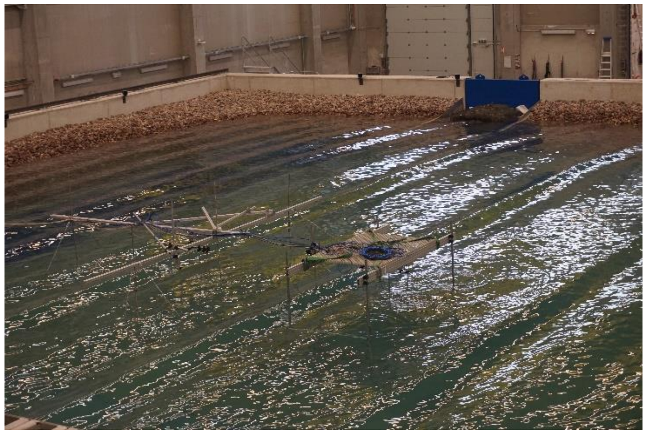


Figure B3. Bended wave crest due to the basin absorber beach.

3. We are left with Q2 and Q4 in which we performed several probe arrangements, namely Arrangement A and Arrangement B (Figure B4). We observe non-uniform regular waves in the position closer to the east-wave maker (Q2), but better uniformity in the one closer to the wave absorber (Q4), as seen in Wave Gauge (WG) 1-5 in Figure B5. Therefore, we go with the present location.

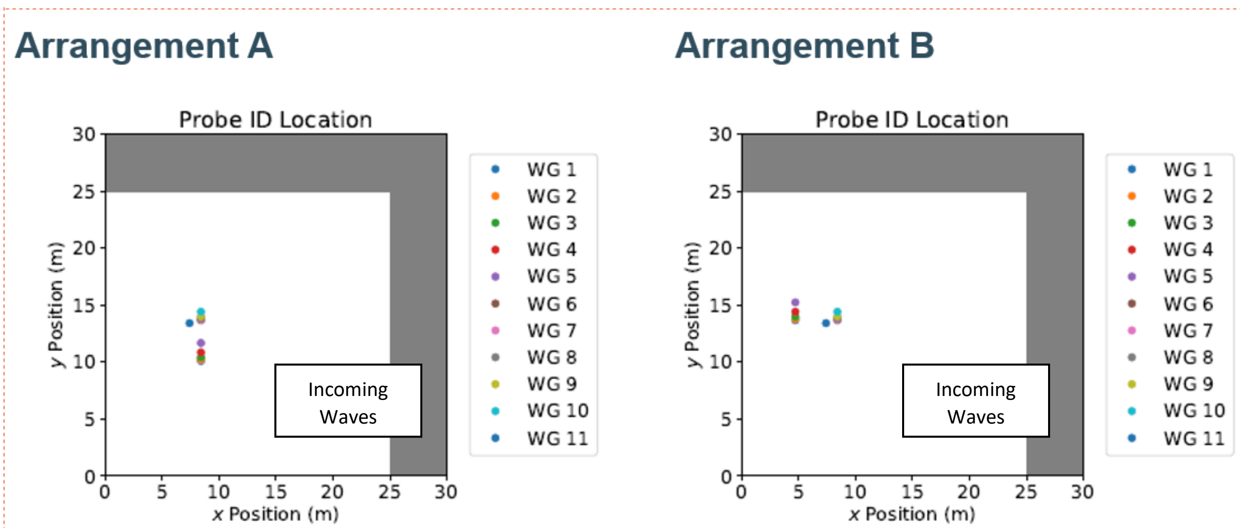


Figure B4. Two arrangements to check the Q2 and Q4

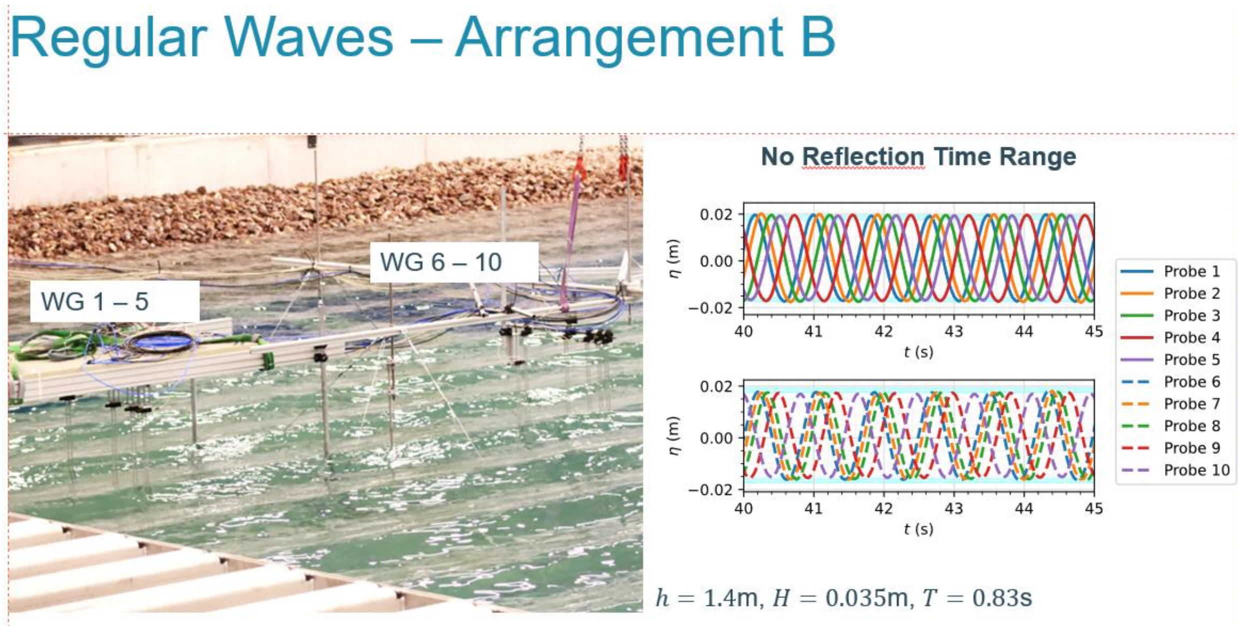
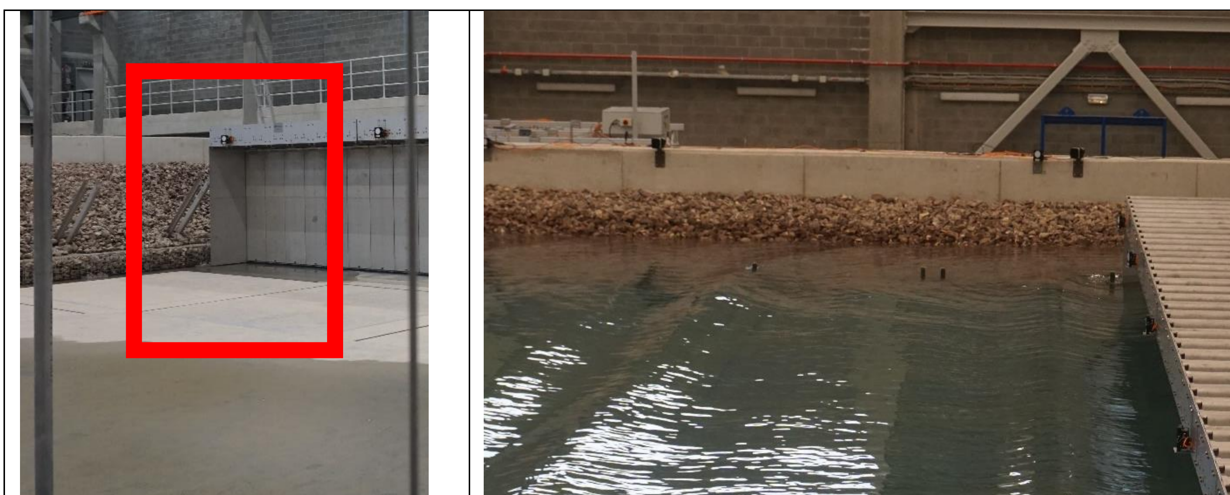


Figure B5. Wave arrangements and the measurements of the Arrangement B

From our preliminary investigation, we observed the non-uniformity might be due to the diffraction effect of the guiding wall from the wave makers, where the value becomes smaller in the further location. The picture of the guiding wall and the resulting waves are shown in Figure B6. It explains such non uniform measurements on our WG located closer to the wave maker. Thus, we mentioned the possible diffraction effect in our paper is due to the diffraction from the guiding wall. Deep analysis and understanding of the basin are currently going through another PhD research project, which is not part of this paper. Our interest is that the wave condition at the location of our monopile is good enough, as we validated in Figure 3 of the paper.



a) Guiding wall
b) Spurious waves from the corner

Figure B6. Guiding wall and the diffracted waves

We prefer not to mention this discussion because it would distract the focus of the readers and maintain the brevity of the paper. Nevertheless, we updated the guiding wall location explanation (L145 – L146) and extra justification with regard to the dissipation of the absorbing beach (L146).

Table 2: It would be great to add some nonlinearity parameters as well. This could for example be the Ursell number. Even better might be to add the test conditions in the Le Méhauté and Chakrabati diagrams. This could help a lot in understanding the obtained results.

Response:
The reason why we did not the Le Méhauté diagram in the first place was because it was more relevant to the regular wave cases. However, we added the figures but with extra description that we used T_p and H_s (L172 – 174, Figure 2)

Line 163: replace 'as' by 'with'

Response:
It is corrected accordingly (L182)

Figure 7: I would suggest to add methods that do not assume linear waves. The industry has for a long time used Forristall distribution and you mention also Fuhrman.

Response:
We added the nonlinear distribution introduced in L128 – L130 and Appendix A, applied in Figure 8 and discussed in L267 – L277. We used Forristal (2000) and Gramstad and Lian (2024). Regarding Fuhrman reference, we mistakenly refer to wrong paper, Fuhrman was about wave elevation distribution, not the crest distribution (the linear model is Gaussian). What we meant was Gramstad and Lian (2024). Both Fuhrman (2022) and Gramstad and Lian (2024) are published in the Journal of Fluid Mechanics. We would like to thank you for addressing this and apologize for our mistake.

Your comment here ignites an idea to apply certain correction as well in our WFAM (L277 – L286). Therefore, we would like to thank you for this suggestion. Although it is with limitation, it provides better correction than the linear theory (Figure 9, discussed in L306 – L311, and becomes part of our conclusion in L404 – L406).

Figure 8: Angular discretization of the theoretical calculation should be increased.

Response:
We agree that the figure of the original manuscript was not clear with respect to the angular discretization. We actually intended to really just compare the results at the corresponding angles between linear theory and experiments. We found a better way, which is now with bar chart, as in Figure 9, along with the correction we added in the revised manuscript.

Line 262 and following sections: There is referred to Wave Type 1 and Wave Type II from Swan and Sheikh. It is recommended to introduce those two types in more detail in the present paper.

Response:
We provide an explanation of the Wave Type I and Wave Type II in L320 – L342, along with additional reference needed.

Line 308: Sentence is incomplete

Response:
The sentence is completed as seen in L393

Line 308: The crest are => The crests are

Response:
It is corrected in L263.

Recommendation: Revisions Required

Response:
The track-change revised version is attached.

ROUND 2

Reviewer B

I am happy with how the authors have replied to the reviews, but a few things needs corrections before the paper is acceptable for publication.

Response:

Thank you for the remark, we will address the comments accordingly in the following. Changes in the manuscript for the second round review are in red color.

The two main issues are:

1. The explanation for the deviations between MacCamy and Fuchs LTF and experimental I find a bit confusing. Do you expect that the transfer function is significantly different for bound long waves than for free waves at the same frequency? I assume their wavelength is almost the same and thus I do not expect the transfer function to deviate so significantly.

   Response:
   If we understand the remark properly, you refer to the bound long waves as the difference frequencies waves, i.e., subharmonics with a frequency of $f^{(-)} = f_l - f_m$ where l, m are the free wave frequencies. Meanwhile, there also exist free waves at f_o where $f_o = f^{(-)}$. It is argued that because the wavelength from $f^{(-)}$, i.e., $|k_l - k_m|$ is almost the same as k_o (linear dispersion solution of f_o), the experimental LTF should also be able to extract the theoretical LTF due to $|k_l - k_m|$. Meanwhile, in our study, we show that the experimental LTF in the low frequency does not correspond to the theoretical LTF.

   Firstly, the LTF method corresponds only to the linearity of the system between the incident waves and the WFAM. On the other hand, there also exist QTFs (Quadratic Transfer Functions) that capture the nonlinearity of the system. We realized we should have explained/mentioned this in the introduction as well (added in L26—L30). Therefore, the linearity between the incident and the WFAM is captured by the LTF for free waves with f_o, k_o while the nonlinearity is captured by the QTF for waves with $f_l - f_m,\ |k_l - k_m|$, despite $f_o = f_l - f_m$. If the linear coherence is 1, then (f_o, k_o) is more significant in that frequency than $(f_l - f_m,\ |k_l - k_m|)$. Meanwhile, if the linear coherence is much lower than 1 then $(f_l - f_m,\ |k_l - k_m|)$ is more pronounced, thus the incident – WFAM is more driven by the QTF. This can be further investigated (or confirmed) via higher-order coherence study in QTF analysis. We found a good example to describe this, where runup on TLP cylinders was investigated (Sibetheros, 2005). An extract is shown in Figure A. From Figure A, they concluded the lower frequency is driven by quadratic coherence (second-order coherence). We lack a QTF computational tool, so we cannot do this study for our experiment at the moment.

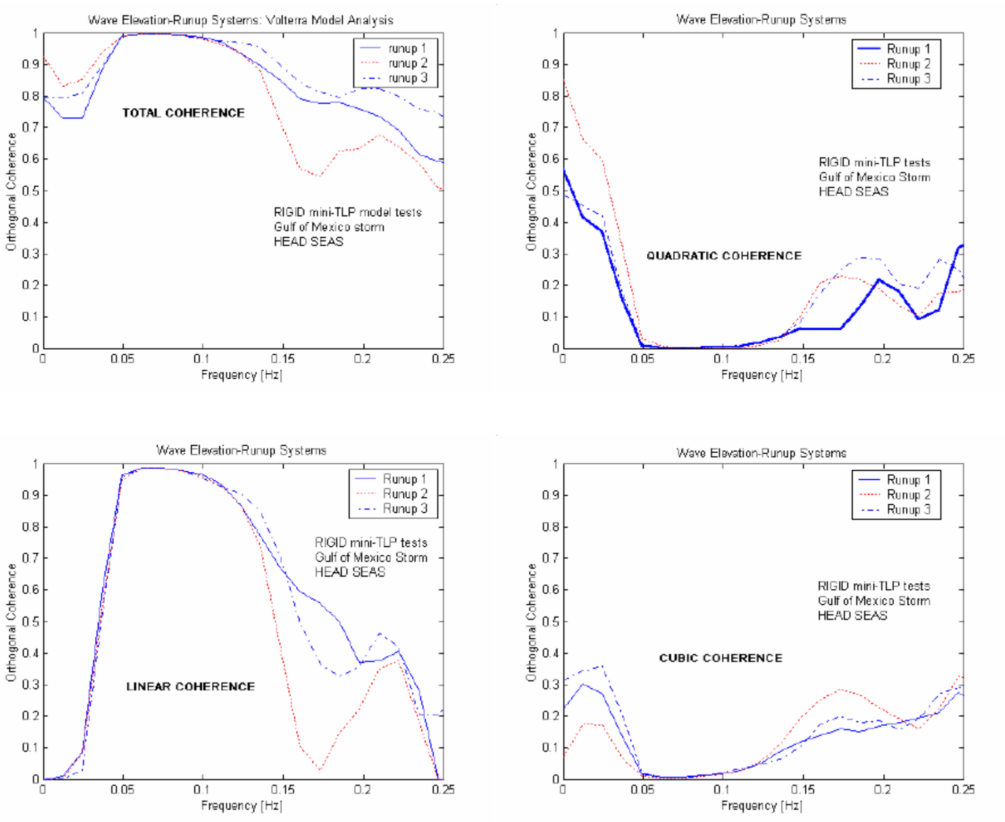


Figure 7. Linear, quadratic and cubic components for the rigid model data in head seas.

The orthogonal coherence spectra for the four cases analyzed, that is rigid model in head and quartering seas, and compliant model in head and quartering seas display several important similarities:

- The total coherence is very high in all cases and over a broad frequency band that extends beyond the wave excitation band.
- The linear component is dominant, with values exceeding 0.9 (or 90%) over the high wave energy frequency band.
- The quadratic component is significant at low frequencies and the main contributor to the high total coherence at very low (difference) frequencies.
- At high frequencies, beyond the high wave energy frequency band, both the quadratic and cubic components contribute to the relatively high total coherence.

FIGURE A. An extract from Sibetheros (2005)

Secondly, despite $|k_l - k_m| \approx k_o$ the transfer functions are different. (f_o, k_o) is due to linear boundary condition, thus their transfer function is LTF. Meanwhile $(f_l - f_m, |k_l - k_m|)$ are due to the nonlinear boundary conditions. Therefore, their transfer functions are from QTF. The analytical solution for QTF can be consulted in Taylor & Huang (1997). Future interesting studies can be performed by comparing analytical and experimental QTF as what we have done in this paper with LTF.

In conclusion for this remark, we do not agree that $(f_l - f_m, |k_l - k_m|)$ can be described with the same features as LTF of (f_o, k_o) as they are driven with different transfer functions. The difference frequency (along with the sum frequencies) must be investigated with QTFs. However, regarding the LTF deviations, …

But what is the reliability of the computation? Figure 3 shows that almost no energy is present in the incident wave for f/fp < 0.6. Maybe the wavemaker did not even introduce any energy (free or bound at these frequencies). Thus small errors may influence the calculations significantly. I for example expect that the wave reflection coefficient from the beach at these frequencies are much larger than the average provided 12%. Thus reflections will likely influence the provided experimental LTF in the low frequency range.

…, we realized from your remark that the experimental LTFs in our study deviate because the energy at (f_o, k_o) are not sufficient. Provided that the energy is sufficient the LTF will be the same. At first, we drew a strong causation that if the linear coherence is lower than 1, then the experimental LTF will not correspond to the theoretical LTF. However, LTFs and coherence are not causal. This can be elaborated by Figure 6a in wave frequency range, from which even though the coherence is low the LTFs can correspond to the theoretical LTFs. The LTFs being off in the low and high frequency is thus attributed to the numerical defect. Meanwhile, linear coherences are just a way to say that linearity is more pronounced or not, but it does not define the LTF. Thus, we added this explanation in L245—250.

For the high frequency range it is more understandable the difference you observe as here the bound and the free waves behave completely different. But you need to consider the reliability and then truncate the provided LTF in order only to show frequencies with reliable calculations, i.e. frequencies with relevant amount of energy in the incident signal.

Similarly, we follow our new argument that the experimental LTFs do not correspond to the theoretical LTFs because of a lack of energy. It seems that $0.75f_\mathrm{p} - 1.75f_\mathrm{p}$ can be seen as the regions of clean from the numerical defect. Therefore, the LTFs correspond to the theoretical LTF in this frequency range. Accordingly, we removed our L239—240. The question of how far the truncation affects the sum frequencies can be answered only by comparing the experimental and analytical QTFs.

Sibetheros, I., Niedzwecki, J. and Teigen, P. (2005). Analysis of wave run-up measurements on a mini-tlp, In: International Conference on Offshore Mechanics and Arctic Engineering, volume 41952, 881‑890.
Taylor, R.E. and Huang, J. (1997). Semi-analytical formulation for second-order diffraction by a vertical cylinder in bichromatic waves. Journal of fluids and structures, 11(5), 465‑484.

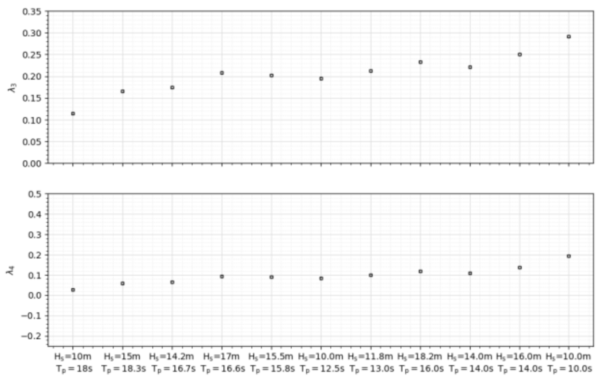
2. It should be explained in the figure caption what Corr. 1 and Corr. 2 is. Results with Corr. 2 (Gramstad and Lian) seems very strange and must be wrong. The PE curve is horizontal or even increasing around PE = 0.15. This cannot be physically correct. In lines 282-284 you comment on the results. If outside of application area then I suggest to leave this correction out as right now it leads to confusions.

Response:
The caption is updated accordingly.

Regarding the reviewer's view that our results are wrong. We verified our code by redoing Gramstad and Lian (2024). Please find it below:

A. The skewness and the kurtosis based on their Eqs. (4.4) or Eqs. (20) in our manuscript

Our Code:



Their Paper:

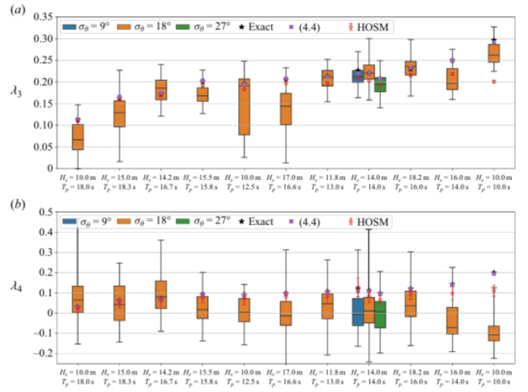
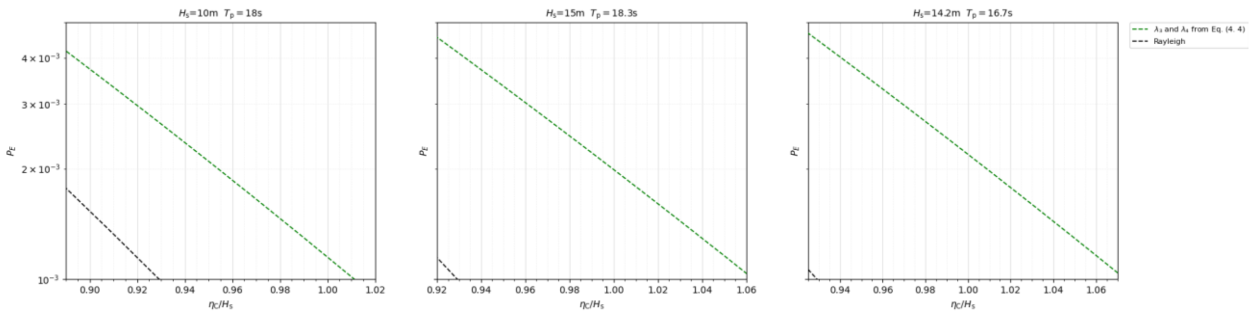


Figure 12. (a) Skewness λ_3 and (b) excess kurtosis λ_4 from model tests and HOSM simulations compared to 'exact' values as well as results from the new parametrization suggested in this paper, (4.4).

FIGURE B. Comparison between our code and Gramstad and Lian (2024) to estimate the skewness and kurtosis of ocean waves

B. The exceedance of the ocean waves in their paper, from which extracted from their Eq. (6.1) or Eq. (22) in our manuscript. There are 9 waves but only 3 are shown here. The others have been checked which also show good agreement.

Our Code:



Their Paper:

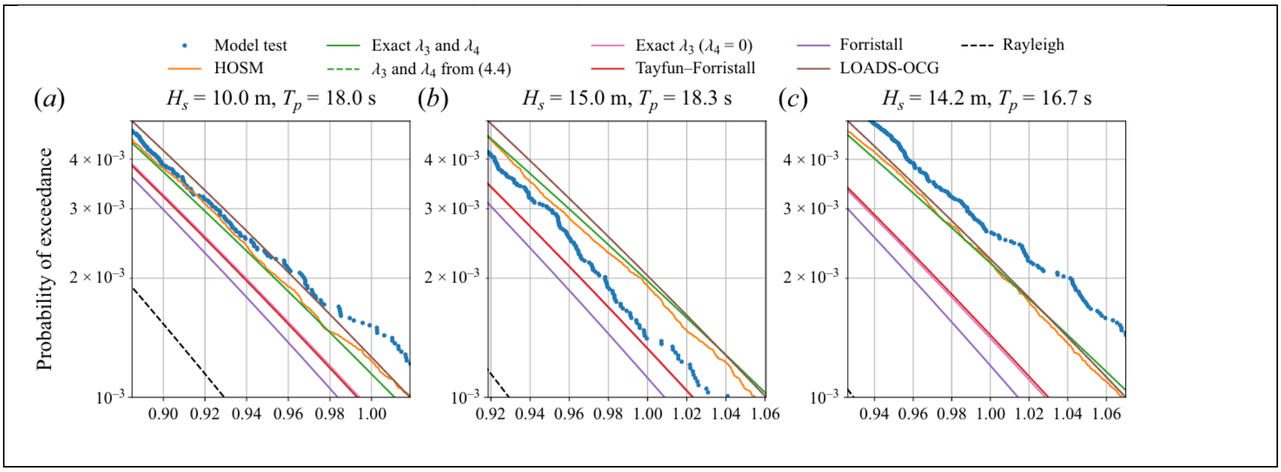


FIGURE C. Comparison between our code and Gramstad and Lian (2024) to estimate the exceedance probability

We do not think the issue is with our computation error. As already noticed by the reviewer, we think it really is because of the skewness and kurtosis input. However, since the reviewer finds it confusing, then we leave it out.

Minor comments:

1. Annoying that most figures are several pages away from the referencing text. Please put figures and text on the same page if possible.
   Response:

   The version that is being reviewed is a version with track changes. A clean version is also provided, giving closer distance between the figure and the referencing text. (submitted_round2_manuscript_clean.pdf)

2. Line 17: closed to a monopile => close to a monopile
   Response:

   Fixed accordingly in L17 of the new manuscript

3. Lines 128+129: nonlinear exceedance probability is a strange wording? I assume it is the exceedance probability using nonlinear wave theory.
   Response:

   Fixed accordingly in L137 of the new manuscript

4. Lines 198-200: What is the origin of the observed time shift? Why don't you correct so they are aligned in the gauge present in both setups using cross-correlation?

   We did try to correct it, but it does not make a significant difference in the LTF estimation. Therefore, we decided not to correct it. Below, you can observe Wave 4 – Wave 6, where the shifts are more noticeable than the others. The origin of the shift should be further investigated. This can be caused by slight changes during the installation of B01 and B02 or the wave maker itself produced slightly different time series. To confirm the latter, the record of the wave maker motions shall be checked to further find out the cause.

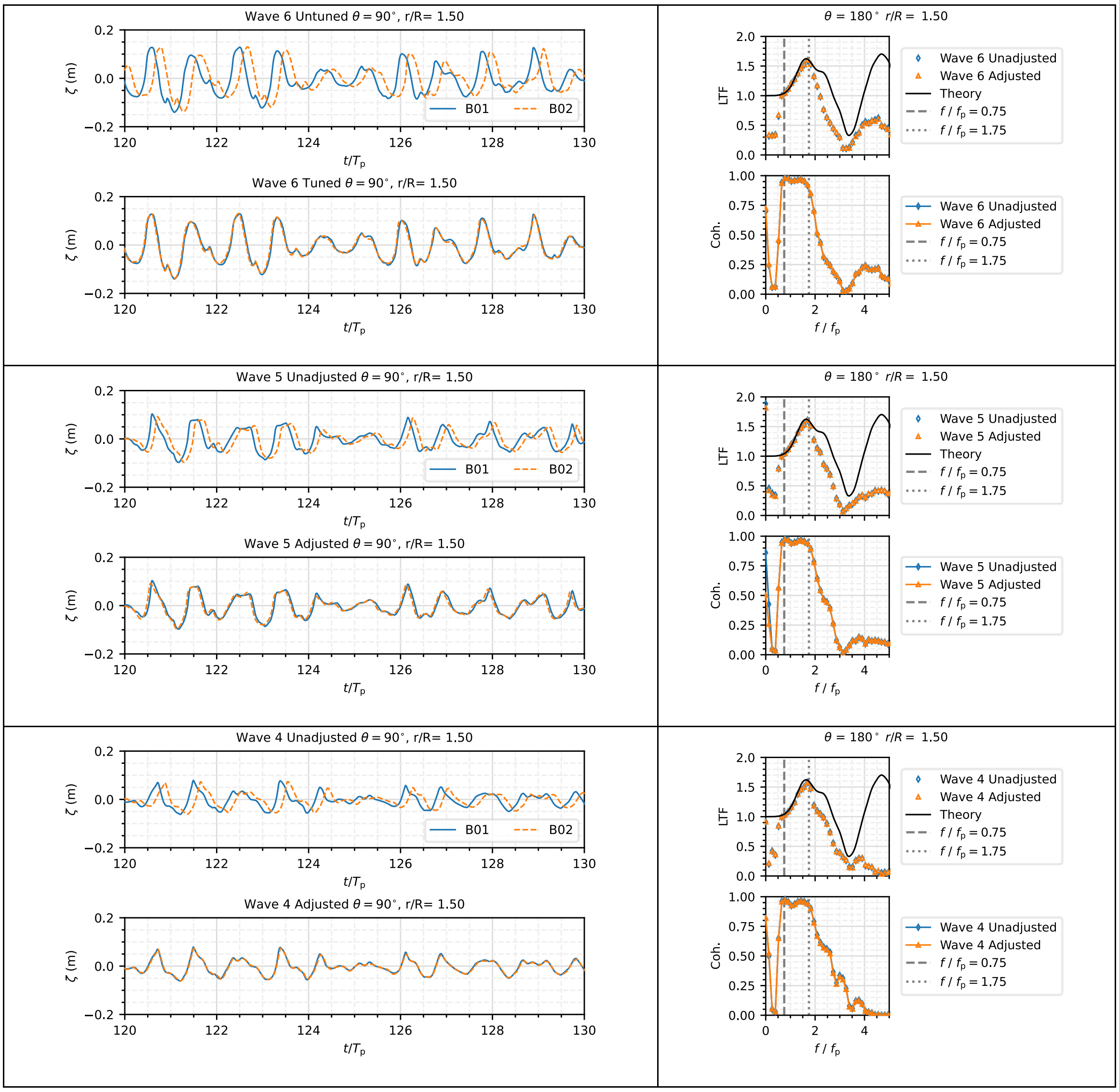


FIGURE D. Time shifting effect in the LTF estimation

5. Lines 248-250. Figure 2 indicate that Wave 3 should not be more nonlinear than Wave 6. So maybe the differences are solely caused by the selected random phases in the incident wave train? With finite number of waves you get a some uncertainty (confidence band) on the Rayleigh distribution.

   While, indeed, the phase would have imposed some uncertainty, it will not affect the coherence findings having a shorter frequency range of Wave 6 and Wave 3. Thus, we still argue that WFAM Wave 3 behaves more nonlinear than Wave 6 because the wave-monopile interactions are more nonlinear in Wave 6. Then, this creates a slight deviation on the tail of the wave height. Meanwhile, in the wave-only case, it is true that Wave 3 should not be more nonlinear than Wave 6. Nevertheless, we agree that the remark about uncertainty can be added for further study but not accounted for in this study: via bootstrap analysis or via different initial phase of the waves.