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Review and rebuttal of the paper

Wave environment analysis at Norwegian harbours for landbased aquaculture facilities using a combined phase-averaging and phase-resolving numerical modelling approach

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Editor handling the paper: Miguel Esteban

The reviewers remain anonymous.

1 Round 1 of review

1.1 Reviewer A:

The manuscript describes the combined modeling of SWAN and REEF3D to determine the extreme wave conditions near two potential aquaculture sites in Norway. The described novelty is in the interpolation scheme of the SWAN results and the coupling with REEF3D.

General comment:

The rationale for using the combined phase-averaged (SWAN) and phase resolving model (REEF3D) is well articulated and motivated. The actual approach and the coupling is not as clear. This has partly to do with the way the modeling study is executed and the limitations in the coupling that only become apparent at the end of the manuscript, but especially at how the local design conditions have been obtained and the interpretation of the results which is currently most descriptive.

The novelty of the spline interpolation in the SWAN results is limited. A typical transformation matrix to translate offshore wave conditions to nearshore uses linear interpolation to which the spline-approach could be an improvement depending on the characteristics of the wave field, i.e. the intra-directional behavior of the SWAN computations. If the authors want to show that there indeed an improved performance the spline method should be compared with linear interpolation and the corresponding metrics should be changed. Currently the MAE, eqn 24, is used to demonstrate the skill of the spline interpolation. However, currently this metric is based on the complete wave domain whereas the interest is at the boundary of the REEF3D domain where the wave boundary condition obtained from SWAN is imposed. I suggest to limit the number of cells to the REEF3D boundary only in the MAE.

Response:

The choice of the interpolation scheme is based on the literature suggestion (line 233-235), however, the authors did not test whether the chosen method is more effective than linear interpolation, the exploration of an optimal interpolation scheme is now included as an outlook in the conclusion section.

The MAE test is primarily designed to test the interpolation scheme and not specifically for the harbour wave assessment at the harbours. The authors think that considering the whole domain is more objective for the purpose of the methodology itself, since it might be the case that MAE is low only at the phase-resolving domain boundary but higher in the entire domain.

General comment:

The offshore boundary conditions for the planned sites are obtained from the deep-water wave conditions provided by WAM10. Based on these the design conditions are derived (Table 1). Apparently, this assumes that the most extreme local conditions (at the projected aquaculture sites) are determined by these conditions. Given the sheltered location of the sites is seems that locally generated waves, or a combination thereof, may be more critical. I therefore expected the wind to play an important role (in the description of the area the strong winds are specifically mentioned). Even though all the source terms seem to be included (see the SWAN description) it is not clear whether the wind has been included in the assessment.

Response:

The reviewer raises a realistic concern, local wind could be of great interest in coastal areas. However, wind inputs were not included, both due to the objective of the study and the lack of local wind information. However, this is added for future suggestion shown in red text in the conclusion section.

General comment:

The limitations of the coupling should be explained earlier in my view. Typically you would impose the spatially-varying SWAN-derived boundary conditions along all of the phase-resolving model boundaries. If I understand it correctly, at the moment the same boundary condition is used, i.e. the same frequency-directional spectrum is used along the Northern boundary only. This is clearly inconsistent with the observed spatial variability both the wave height, direction and period in Figure 6 and 10. I suspect that the idea is that taking the most severe conditions corresponding to the N-E corner point of the domain is a constant boundary condition is a conservative approach. Given the strong variability along the Northern boundary, especially in the direction (see bottom panel of Figure 10) this is debatable. Smaller waves with a larger wave direction may have a much higher impact than the currently selected conditions that are coast-parallel. This is supported by the fact that even a small change in the mean wave angle (going from 10.7 to 12.8 degrees and a reduced wave height) leads to a significant increase in the conditions at both Fiskenes and Breivik (see table 8). Based on this I strongly doubt your conclusion that the most severe sea state is properly identified. I would also be vary cautious in stating that the Breivik proves to be a viable option. Note that to be able to assess these more oblique conditions the wave generation zone in REEF3D should also be imposed at the Easterly boundary to avoid shadow zones.

Response:

The incapability of reading in multiple spectra and directional spectrum is a limitation at the current stage, as also pointed out by the other reviewer. The authors have highlighted it in the conclusion for future implementations, shown in red text.

Due to the lack of 3D transfer from the phase-averaging model to the phase-resolving model at the current stage, it is indeed challenging to identify the most severe sea state for the harbours. The authors acknowledge this limitation and have edited the statement in the conclusion that ‘A severe sea state near the area of interest is identified from the north-east boundary and used as the input for the phase-resolving model where wave diffraction due to the coastline is significant. However, to conclude on the most severe case at the harbours, further developments to include variable boundaries in the phase-resolving model and the 3D effects from the phase-averaging wave field are needed’, as shown in red text.

The current study focus more on providing a procedure and framework than providing conclusive recommendations for design basis. Thus, the authors have removed the statement on ‘Breivik proves to be a viable option’, but replace with ‘Breivik has generally smaller waves in the presented scenario.’ shown in red text in the conclusion.

General comment:

Finally, there are no observations to constrain any of the modeling efforts, which makes it impossible to judge whether the approach of using a combination of phase-resolving an phase-averaged modeling is an improvement or not.

Response:

Unfortunately, due to the lack of measurement data at the sites, no validation against the field data is performed. The current study focuses more on providing a methodology than providing conclusive recommendations. The major advantage is the speed boost for large-scale cases. Thus, the authors have rephrased and removed a few statements on design basis so that the focus is clearer. For example, the authors have removed the last sentence at the bottom of page 17.

Specific comments

Figure 1 (needs higher resolution)

Response: The image has been enlarged and larger texts for the harbours of interest have been added to the image.

Line 22 food

Response: The mistake is corrected in the revised manuscript.

Line 24 winds

Response: The mistake is corrected in the revised manuscript.

Line 36 SWAN does have the ability to cope with local maxima in wave height due to the interactions with bathymetric variability and/or currents (see the work by Akrish et al., 2020, JFM) but at computational cost

Response: The authors thank the reviewer for pointing out and sharing the reference. This statement on local maxima has been removed in the revised manuscript.

Line 72 do you imply that the dispersion relation is affected by the amplitude of the waves?

Response: The authors do not imply that dispersion is affected by the amplitude – the statement is referring to nonlinear wave transformations that might be limited in phase-averaging models.

Figure 3: the title states that we are looking at swell conditions only, is that correct? How is the direction defined? Is this a mean swell direction? How was this figure obtained, i.e. what are the statistical methods that have been used?

Response: Yes, the figure shows swell condition only. The swell waves are considered as unidirectional irregular waves and thus has only one direction for each scenario. The graph is a direct interpretation of the WAM10 data at the chosen location without extra statistical analysis.

Table 1: How about wind?

Response: The current work focuses on the swell waves, wind waves are not included in the study, thus not included in the Table. However, as the reviewers pointed out, local winds could be of great importance, which the authors have included in the planned future work in conclusion section.

Eq 2: generation/dissipation

Response: It's dissipation. The equation has been corrected.

Line 107: coastal waters (does not have to be shallow)

Response: It has been changed to 'coastal waters'.

Eq 4. It is probably enough to provide this equation only and subsequently describe the processes that are included and what kind of settings were used.

Response: Eq.5 is removed to only keep Eq.4. The content of Eq 5 is described following Eq.4.

Line 136 only be solved

Response: The mistake is corrected in the revised manuscript.

Line 162 resolve

Response: The mistake is corrected in the revised manuscript.

Line 219 What about the directional distribution? What is the directional spreading? If is indeed swell a Jonswap spectrum with a factor of 3.3 is much too broad.

Response: The default $cos^m(\theta)$-type directional spreading function in SWAN is used, with m=2. This information is added at line 219.

Factor 3.3 is used for the JONSWAP spectrum because it is the default value per recommendation from DNV, and there is a lack of concrete information what factor is the realistic at the site.

Line 228 do you mean every 30 deg interval?

Response: It has been changed to '30 deg' in the revised manuscript.

Figure 5: top panel: please include the nested area. Bottom panel: please include the REEF3D area.

Response: The suggested domains are included in the two figures now.

Table 3: Are these mean directions?

Response: Yes, mean direction, it has been updated in Table 3.

Eqn 24: see general comment earlier. Alternatively, you could include a spatial plot of the error at each cell to show where the methods works best.

Response: Since the MAE test is primarily designed to test the interpolation scheme and not specifically for the harbour wave assessment at the harbours, the authors think that considering the whole domain is more appropriate.

The suggestion of testing where the interpolation scheme works the best is of great interest. As the reviewer pointed out in in other comments, the 18-degree angle has the largest error, possibly due to the fact that 18 degree is furthest away from actual SWAN simulation angle, thus accumulates the largest interpolation error. Thus, an optimal interpolation scheme needs to be explored. The comparison and development for the optimal interpolation is suggested for future work now, shown in red text in the conclusion section. Detailed error analysis will be carried out both for the whole domain and the interface boundary in a dedicated study.

Table 4: How did you select the 18, 36 and 281 degrees as test cases?

Response: These directions are chosen in a random generator between 240 and 90 degrees to test the ability of the interpolation scheme.

Line 254: eta?

Response: The mistake has been corrected, it is 'data'.

Figure 9: Please use the same orientation as in the previous figures. Increased resolution would help identify the variability in the depth. The horizontal scale is missing

Response: Figure 9 has been updated with clearer depth contour lines, consistent orientation as the previous figures, highlighted coastline, and horizontal dimensions.

Line 259: lessen?

Response: The mistake corrected, it is 'seen'

Line 260: resulting significant

Response: The mistake corrected.

Line 269 are listed

Response: The mistake corrected.

Line 270: Why not the actual SWAN spectra? What about the directional spreading?

Response: SWAN produces spectra at every grid points and thus has inhomogeneous spectra along the input boundary of REEF3D. At the current status, the REEF3D model does not support the input of multiple spectra along the inlet boundary or read the directional spectrum from SWAN. Thus, a JONSWAP spectrum is used based on Hs and Tp.

As also pointed out by the other reviewer, the authors have pointed out this limitation at the conclusion remarks with red texts.

Figure 10: explain how the boundary points are defined.

Response: The authors have rewritten the explanation to provide more information on the boundary points definition: 'The resulting significant wave height, peak period, and mean wave directions at the evenly-spaced wave gauges (with the same spatial interval as the grid resolution in SWAN) located along the north, east and south boundaries of the REEF3D::FNPF domain are obtained from the phase-averaging SWAN simulations. These values are summarised in Fig.10', shown in red text in the revised manuscript.

Figure 11: The wave period interpolation is at 18 deg is affected by the spline. In my view this shows the need to evaluate the potential interpolation error at the REEF3D boundary (see earlier comments).

Response: That is a valid point. 18 degree is the furthest away from actual SWAN simulations (per 30-degree interval), and this possibly contributed to the larger error. Indeed, a further investigation on what interpolation scheme is the best is needed. These discussions have now been included in the text, shown red, after Table 4. A future suggestion on interpolation scheme optimisation for the best and most accurate method is also added for future investigation in the conclusion section.

Figure 13 is not really needed in my view.

> **Response:** Figure 13 is removed.

Table 7 The caption: which consulting firm are we talking about here? Does this mean there is data available to test the model performance?

> **Response:** The chosen harbour is based on an engineering case where a Norwegian company is involved. However, the reference here is not relevant as an academic article, the caption has now been modified.

Line 292 Fig.

> **Response:** The mistake is corrected, the first Fig. is removed.

Figure 14: Why not combine this with Figure 16? Note that the units are missing. Can you also have wave generation zone at the eastern boundary?

> **Response:** The authors keep Fig. 14 as an additional figure because it shows the NWT configuration more clearly and won't overload Fig.15 and 16 with too much information that reduce the focus of the results. Currently, wave generation is only implemented in the left-hand side not the right-hand side. Units are added in Figure 12 (old Figure 14) now.

Line 294 fast

> **Response:** It is 'far' field, the mistake is corrected.

Line 302: Because of the directionality of the waves, hence the importance of the directional spreading.

> **Response:** A sentence is added here 'This highlights the importance of directional spreading and its impact on coastal wave distributions.'

Line 322 There is no discussion about the fact the results for Sim 2 are significantly larger than for Sim1. This is where the more detailed modelling with REEF3D can really help in understanding what is going on.

> **Response:** A short discussion is added here in the paragraph: 'Though Sim1 has a larger input wave height, Sim 2 shows higher waves near the harbours. This is likely due to the different incident wave angles - the harbours are more exposed to the 30 degree angle from Sim2. Sim 3 also shows lower waves than Sim 2 near the harbour, which is mostly because the directional spreading in Sim2 propagates more wave energy around the obstacles towards the harbours. The contrast among Sim 1, Sim 2 and Sim further demonstrate the importance of directionality and directional spreading.'

Line 336: Not faster than linear interpolation. Can you show it is better?

> **Response:** The authors cannot claim that proposed approach is faster or better than linear interpolation. The statement here focuses on the advantage of using an interpolation method over not using any interpolation method.

1.2 Reviewer B:

This paper proposed a novel numerical model to assess onshore wave conditions for Norwegian coasts. The aim of the proposed model was to seek the compatibility between computational efficiency and applicability to complicated topography. The findings will be useful, especially for consultancy works. The research aims and the concept of the model development were strongly supported by the introduction section. However, there are some unclear points in the model derivation. Also, given the impression that the current title and introduction would lead, discussions about the applicability to design purposes seem a bit weak. It is suggested to add more clarification for the followings.

Main comments

(1) Regarding the SWAN modeling (section 3), the input condition of the wave source was unclear. Were there wave sources over the SWAN domain or on specific boundaries?

Response:

The input waves are imposed on both the north and west boundaries. This is now clarified in red text in section 3.

(2) The input of the wave source for the REEF3D model (section 4) was also unclear. It seemed that there were point-defined wave sources over the entire inlet boundary.

Response:

The input waves from a point source chosen from the extreme scenarios in SWAN simulations are imposed at the entire inlet boundary in the FNPF domain. This is now clarified in red text in section 4.

(3) Although being arbitrarily chosen, the offshore wave with the direction of 18 deg determined the significant scenario for the REEF3D analysis (like Figure 10). What if 18 deg had not been chosen for verification? Considering future usage for design purposes, it would be suggested to clarify how to select the most severe wave conditions for REEF3D simulations.

Response:

For demonstration, only one direction (18 degree) is chosen from a limited discrete sample. However, for design purpose, its recommended to perform a complete interpolation of all offshore wave conditions in SWAN simulations following the procedure in section 2 and compare the simulation results at the FNPF boundary to identify the most extreme wave input. The second paragraph of the conclusion briefly summarise the procedure.

(4) In the paper, waves from the NE corner are deemed to be the most severe wave forcing. I suppose that this direction would vary if the REEF3D domain is placed differently. Please clarify how to determine the size of the REEF3D domain.

Response:

The reviewer is right, the domain choice will influence input wave choice. An explanation is added in red text in section 4 for the choice of domain:

‘The domain length of the numerical wave tank (NWT) in the north-south direction is chosen so that the relevant coastlines that influence wave transformation around the two harbours are included in the domain. The west-east domain length is chosen so that the relevant bathymetric features are included in the domain, such as the deep to shallow water transition. The exact extends in both directions are also limited by the size of the higher resolution bathymetry data from Secora AS.’

(5) The applicability of the proposed model was validated qualitatively but not substantially supported quantitatively. In that sense, statements about the potential usage for design purposes were a bit weak. One suggestion would be to compare the numerical results with the design wave parameters provided by consulting firms (if available).

Response:

The commercial measurement is unfortunately not available. In this case, the authors rephrase and removed a few statements so that the current work focuses on providing a procedure and framework than providing design basis. For example, the authors have removed the last sentence at the bottom of page 17.

(6) In my view, the absence of directional spectrum in the REEF3D simulations should be stated clearly as a possible limitation of the proposed model. Or did the final sentence (L356-357) imply this point?

Response:

REEF3D is able to generate directionally spreading waves but cannot read in directional spectrum as input. The authors have now added explicit statement on this limitation and the plan for future work at the end of conclusion, see red text: ‘The capability of reading in a directional spectrum from the phase-averaging model should be implemented in the phase-resolving model.’

Minor remarks

(1) Python language was utilized to interpolate the results of SWAN simulations. Were the interpolation algorithms made from tuned libraries (e.g. Numpy or SciPy)?

Response: Yes indeed, the Python code utilises several packages, including NumPy, SciPy and Pandas for data processing, for example, the interpolation scheme is from SciPy.

(2) In my view, Figure 12 is not needed (directional spectrum was not implemented into the REEF3D simulations). It could be moved to Appendix?

Response: Figure 12 is removed in the revised manuscript, together with the associated text.

(3) In Table 4, why was the MAE of mean direction of 18 deg offshore wave exceptionally large?

Response:

A suspicion is that different wave transformations take place with waves coming from different angles, and that might lead to different fidelity level. Another speculation is that 18 degree is the furthest away from actual SWAN simulations angles (30 degree interval), and thus might have larger interpolation errors. These discussions have now been included in the text, shown red, after Table 4. A future suggestion on interpolation scheme optimisation for the best and most accurate method is also added for future investigation in the conclusion section.

Recommendation: Revisions Required

2 Review round 2:

2.1 Reviewer #1

I appreciate the effort of the authors to address my earlier comments. I am happy how these have been incorporated in the manuscript except for the one related to the use of the MAE to evaluate the interpolation method. This currently addressed by the authors as:

'The MAE test is primarily designed to test the interpolation scheme and not specifically for the harbour wave assessment at the harbours. The authors think that considering the whole domain is more objective for the purpose of the methodology itself, since it might be the case that MAE is low only at the phase-resolving domain boundary but higher in the entire domain.'

This really should be evaluated at the **boundary** of the REEF3D domain as this determines the conditions approaching the harbours and associated design conditions. The fact that there is a good overall match does not say much how well the interpolation works. So please add the MAE for the REEF3D boundary points.

Response:

The authors have adopted the reviewer's advice and evaluated the interpolation error at the REEF3D input boundary, as listed in the new Table 5. Higher errors are noticed for wave coming from the western sphere and diffraction is strong at the northern tip of the island. However, the average errors are still generally bounded within 5%.

2.2 Reviewer #2

The manuscript has been properly revised according to the reviewers' comments.

However, I have a comment on the structure of the current manuscript. The ingenious coupling method seemed to be motivated to avoid the current limitation of the REEF3D (e.g. directional spectrum), whereas the limitation was clearly stated only in the conclusion section.

I suggest that the limitation is also stated in an earlier section (better before the actual coupling method), which could be beneficial for readers to understand why the presented coupling methods are required.

Therefore, I would like to ask the authors to revise the manuscript again. I would recommend the acceptance of the paper after the revision.

Response:

Then limitations of REEF3D and/or the complimentary advantages of SWAN are discussed now in the introduction section, see Line 74.

REEF3D is able to generate short-crested directional irregular waves using a theoretical directional spectrum (e.g. JONSWAP + Mitsuyasu directional spreading function), but doesn't have the capability to read in a third-party generated directional spectrum, which is indicated as future work in the conclusion to read in SWAN-generated directional spectrum. The major limitations of

REEF3D::FNPF are the computational cost in comparison to SWAN and the lack of wave generation from wind forcing.

Minor Remarks

- double brackets on L 94, 123, 161, 166

  Response: Double brackets at the citations have been fixed.

- L 329 While -> ,while?

  Response: The sentence has been fixed.

- L 330 Fig. 16b -> 16a?

  Response: The figure number is corrected.