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Dear Editor,

Please find attached the revised version of our manuscript, together with our detailed response to the reviewers, for reconsideration for publication in *Seismica*.

We would like to thank you and the reviewers for the careful and constructive assessment of our manuscript. The comments have been very helpful in improving the clarity of the methodology, presentation of the results, and interpretation of the physical processes underlying the observed spatial variability.

We have carefully considered and addressed all reviewer comments in the revised manuscript. The principal revisions include:

- clarification of the ANN architecture and the multi-site, multi-output formulation;
- clarification of the model-development and independent evaluation periods and the corresponding interpretation of model generalisation;
- revision of the interpretation of spatial variations in ANN performance;
- clarification of the scope and limitations of the numerical simulations;
- expanded discussion of the ANN underestimation of the highest SWH values and its implications for extreme-wave monitoring;
- improvements to the numerical simulation figures, including additional wavefield snapshots in the Supplementary Material;
- revision of several figures to improve consistency and readability; and
- corresponding revisions throughout the Abstract, Discussion, and Conclusions to ensure that the physical interpretations are appropriately supported by the results.

A detailed point-by-point response describing how each reviewer comment has been addressed accompanies the revised manuscript. Changes have also been incorporated throughout the manuscript and Supplementary Material as appropriate.

We believe that these revisions have substantially improved the manuscript while preserving its central contribution. We thank you and the reviewers again for the time and consideration given to our manuscript, and we hope that the revised version is now suitable for publication in *Seismica*.

Yours sincerely,

Samaneh Baranbooei

(On behalf of all authors)

Response to Reviewer A Comments:

Summary

Thank you very much for taking the time to review this manuscript. Please find the detailed responses below and the corresponding revisions/corrections highlighted/in track changes in the re-submitted files.

Major Comments

1. Inconsistency in Dataset Description and Evaluation Period (2022–2023 Data)
 - Issue: In Section 2.4 (Line 118), the authors state that the integrated dataset spans 2011–2020 (~42,000 hourly samples). However, the primary evaluation time series in Figures 2, 3, 5, and 6 exclusively showcase data from July 2022 to June 2023 (or Julian days spanning 2022–2023).
 - Recommendation: The manuscript lacks an explicit description of how the 2022–2023 dataset was acquired and processed. Please clarify whether the 2022–2023 period was held out as a completely independent, unseen test set. Section 2.4 must be revised to clearly define the exact date ranges used for training, validation, and testing.

Response: Thank you for highlighting this ambiguity. We agree that the description in the original manuscript did not clearly distinguish between the dataset used for ANN model development and the later dataset used for independent evaluation.

The hourly samples from 2011–June 2022 were used for ANN model development and were partitioned into the training, validation, and internal testing subsets described in the revised manuscript. The July 2022–June 2023 dataset shown in the main time-series figures was not included in ANN training, validation, or internal testing and was used as an independent temporal evaluation dataset. Thus, the 2022–2023 results represent predictions for a later period that was completely unseen by the ANN during model development.

Section 2.4 has been revised accordingly to explicitly state the date ranges and distinguish the internal ANN data partition from the independent 2022–2023 evaluation period. We thank the reviewer for identifying this point, as the revised description makes the model evaluation procedure considerably clearer.

2. Risk of Data Leakage in Train/Test Splitting
 - Issue: In Section 2.5 (Lines 138–139), the authors mention an 80%/20% train/test split. If this split was performed by randomly shuffling individual hourly samples, the high temporal autocorrelation inherent in time-series wave/seismic data will lead to severe data leakage, resulting in overoptimistic R2 and error metrics.
 - Recommendation: The authors should explicitly clarify the splitting strategy. If a random split was used, I strongly advise re-evaluating the model using a block/chronological split (e.g., training on 2011–2020 and testing on 2021–2023 or split in months randomly) to truly demonstrate the network's predictive and generalization capabilities.

Response: Thank you for raising this important point. We agree with the reviewer and therefore revised Section 2.5 to clarify the distinction between the internal 80%/20% split used during ANN model development and the independent temporal evaluation used to assess model generalisation. The 80%/20% partition was applied to the 2011–June 2022 dataset for model training and internal testing. Importantly, the July 2022–June 2023 dataset was completely excluded from the model-development process and was subsequently used as an independent chronological evaluation dataset. Thus, the ANN predictions presented for this period were generated from data that were not encountered during training or internal testing.

We agree with the reviewer that the randomly partitioned internal test subset alone would not provide a sufficiently stringent assessment of temporal generalisation. The independent 2022–2023 evaluation addresses this concern by introducing a clear temporal separation between model development and evaluation, thereby substantially reducing the potential influence of temporal data leakage. We have revised the Methods section to make this distinction explicit and to avoid interpreting the internal 80%/20% split as the primary evidence of model generalisation.

3. Logical Ordering of Performance Metrics in Figure 4

- Issue: In Figure 4, performance metrics are sorted by the "distance to the nearest seismic station." However, the authors subsequently argue in the Discussion that performance variations are primarily governed by north-south structural scattering effects rather than simple geometric distance.
- Recommendation: Sorting Figure 4 by distance creates unnecessary confusion. I suggest reordering Figure 4 by latitude (North to South) or grouping by region (e.g., Rockall Trough vs. Porcupine Basin) to align directly with the physical wavefield scattering mechanisms discussed later in the manuscript.

Response: We thank the reviewer for this suggestion. We agree that sorting the locations by latitude provides a clearer spatial comparison and is more consistent with the geographic interpretation discussed later in the manuscript. Figure 4 has therefore been revised so that the buoy and virtual-buoy locations are ordered from north to south rather than by distance to the nearest seismic station. We also moved the previous Figure 4 (metrics sorted by the distance) to the supplementary materials.

4. Generalization problems

Even though the author tested and validated the dataset from 2021-2023 for the same region, the authors should also test their model in other regions, e.g, Northern Pacific for the generalization test.

Response: We thank the reviewer for this suggestion. We agree that evaluating the methodology in other oceanic regions would be valuable; however, we respectfully note that applying the trained ANN directly to a geographically independent region, such as the North Pacific, would represent a test of geographical model transferability rather than the generalisation investigated in the present study.

The ANN developed here is trained using seismic amplitudes recorded at a specific terrestrial seismic network, and the learned relationship between these observations and

offshore SWH inherently incorporates regional source–receiver geometry, bathymetry, propagation effects, crustal structure, and station/site characteristics. Consequently, the trained network is not expected to be directly transferable to a geographically distinct seismic network without region-specific training or calibration. Indeed, one of the findings of the present study is that ANN performance varies spatially even within the Northeast Atlantic as a consequence of differences in microseism propagation and scattering. Direct application of the same trained model to the North Pacific, where the seismic stations, source regions, bathymetry, and propagation paths are entirely different, would therefore not constitute an appropriate test of the predictive capability for which the model was designed. It should be noted that given the sensitivity to specific local microseism propagation effects, the trained ANN is likely not transferrable to another geographical location, where retraining would be required.

In the present study, generalisation is assessed in two ways: (1) temporally, by applying the trained model to the independent 2022–2023 dataset that was excluded from model development; and (2) spatially, by evaluating its performance across multiple physical and virtual offshore locations within the Northeast Atlantic study region. Testing whether the overall methodology can be reproduced in geographically independent ocean basins would require training and evaluating a new region-specific ANN using seismic and wave observations from that region. We consider such an inter-regional assessment an important direction for future work, but it is beyond the scope of the present study.

Minor Points and Figure Details

1. Time Axis Annotations in Figures:

- In Figures 2, 3, 5, and 6, the time labels mix calendar dates (2023), months (Jul, Jan), and Julian Days (2022-217, 2023-008). Please harmonize these time axes so that the exact evaluation periods across 2022 and 2023 are clear and consistent.

Response: We thank the reviewer for pointing out the inconsistency in the time-axis formatting. We have revised Figures 5 and 6 so that calendar dates are used consistently throughout.

2. Typos and Spelling Errors:

- Line 33: Change mircoseism to microseism.
- Figure 3 Caption: Correct WW3 HINCAST to WW3 HINDCAST.
- Line 191: Correct refere to refer.

Response: All the mentioned typos and spellings have been corrected, thank you!

3. Underestimation of Extreme SWH Peaks:

- Quantile-quantile (Q-Q) plots in Figures 2c and 3c show that the ANN systematically underestimates extreme ocean wave heights ($\text{SWH} > 6\text{--}8\text{ m}$). This is a common regression issue ("regression to the mean"). A brief discussion on how this limitation impacts extreme weather monitoring would add value to Section 4.

Response: We thank the reviewer for highlighting this important limitation. We have added a paragraph to Section 4 discussing this behaviour and its implications for extreme weather monitoring.

The revised Discussion clarifies that extreme SWH conditions are comparatively underrepresented in the training dataset, which can contribute to predictions being biased toward the more densely sampled range of wave conditions. Although the ANN captures the timing and overall evolution of energetic events, the magnitude of the largest predicted SWH peaks should therefore be interpreted with caution. We also note that improved representation or weighting of extreme events during training could potentially reduce this bias. Finally, we clarify that, in its present form, the seismic-based ANN is intended as a complementary offshore monitoring approach rather than a replacement for direct observations or operational forecasting systems where accurate quantification of extreme SWH is critical.

4. Ardhuin, F., Stutzmann, E., Schimmel, M. & Mangeney, A. Ocean wave sources of seismic noise” should be cited at WW3 model as well.

Response: Citation added, thank you!

Response to Reviewer B Comments:

Summary

Thank you very much for taking the time to review this manuscript. Please find the detailed responses below and the corresponding revisions/corrections highlighted/in track changes in the re-submitted files.

Main points:

The manuscript states that the main location dependence of the ANN performance is directional, i.e. different performance of modeled locations to the southwest versus those to the west and northwest. Upon comparing the results of the ANN evaluation, this does not seem obvious:

- for example, M3 and M4 perform similarly well, although one is to the northwest and one to the south
- it is quite difficult to say whether VB3 and VB4 (northwest) perform better than VB1 and VB2 (southwest)
- thus, one might also conclude that buoy / virtual buoy locations on the continental shelf are better modeled and those in the basins / troughs / deep sea less well
- distance may also play a role here, and is correlated with shelf / off-shelf (this is already mentioned and not found a satisfactory explanation)

Response: We thank the reviewer for this careful assessment of the spatial pattern in ANN performance. As the reviewer notes, the comparable performance of M3 and M4 despite their different locations, and the absence of a clear separation between the northwestern and southwestern virtual locations, indicate that the spatial pattern is more complex than a simple north/northwest versus south/southwest dependence.

We have therefore revised the manuscript to avoid interpreting direction as the dominant control on ANN performance. The revised Discussion now considers source-receiver distance and geometry, water depth and bathymetric setting, location relative to the continental shelf and shelf break, and crustal propagation effects as potentially contributing factors. Because several of these parameters are geographically correlated and only a limited number of offshore locations are considered, their individual contributions cannot be uniquely separated from the ANN performance results.

We have consequently reframed the numerical simulations section as an investigation of the possible contribution of propagation effects rather than as evidence for a uniquely directional control. The simulations demonstrate that the wavefield can be substantially modified along different source-receiver paths, including strong interactions around the continental margin and shelf break, but they do not establish that propagation direction alone explains the spatial differences in ANN performance. The corresponding statements in the Discussion, Abstract, and Conclusions have been revised accordingly.

To me it seems that with the limited number of modeled locations, it is not possible or at least not simple to favor one hypothesis clearly above the other (distance effect, effect of continental shelf, effect of particular bathymetry/crustal structure). So I propose that the

manuscript is adapted to either reinforce the argument why the hypothesis of geographic differences in wave propagation effects is favored, or to not exclude additional possible explanations so squarely. The wave propagation simulation snapshots also show a strong effect of the shelf break.

Response: We thank the reviewer for this important observation. We agree that, given the limited number of source scenarios considered in the numerical modelling, the simulations do not allow us to uniquely distinguish the relative contributions of source--receiver distance, continental-shelf and shelf-break geometry, bathymetry, and crustal structure to the observed differences in wavefield propagation and ANN performance.

We have therefore revised the Discussion to avoid favouring a single physical explanation too strongly. The revised text now interprets the simulations as demonstrating differences in source-to-receiver propagation between the northern and southern source scenarios, while explicitly acknowledging that several factors, including distance, shelf and shelf-break geometry, bathymetry, and crustal heterogeneity, may contribute to these differences and cannot be uniquely separated with the present simulations.

Accordingly, we have softened the corresponding statements in the relevant sections and no longer attribute the spatial variation in ANN performance uniquely to a particular geological structure. The simulations are instead used as supporting evidence that propagation-path effects may contribute to the observed spatial variability, while other related geographic and structural controls remain possible.

In Figure 7, the time of the snapshots should be noted. Additional snapshots in the supplementary material for different times would be great. Time-distance plots between val and IDGL for the two sources could also be considered to understand the propagation of different types of waves. All this could help to understand which waves will likely affect the SMA and how they evolve on the propagation path.

Response: We thank the reviewer for these helpful suggestions. We have revised Figure 7 so that the simulation time corresponding to the displayed wavefield snapshots is now explicitly stated in the figure caption. In addition, we have added a supplementary figure showing the evolution of the simulated wavefield at several additional time steps for both source scenarios. These additional snapshots provide a clearer view of how the wavefield develops and interacts with the continental margin and shelf break during propagation toward the Irish seismic network.

We also appreciate the suggestion to construct time-distance plots between VAL and IDGL for the two source scenarios. Although, such plots could provide further insight into the propagation of different wave types and their possible contribution to the secondary microseism amplitudes recorded at the stations, this would require a more detailed wavefield decomposition and analysis than is undertaken in the present study. We have therefore not included this additional analysis in the revised manuscript but now identify it as a useful direction for future work.

Minor points:

It is a bit confusing that in line 132 ff it is stated that the ANN has a single output neuron for SWH, but in fact, one ANN is used for all buoy and virtual buoy locations. This could be clarified.

Response: We thank the reviewer for identifying this ambiguity. The description of the ANN architecture in the original manuscript incorrectly referred to a single output neuron. The model is in fact a multi-output ANN with five seismic inputs and ten SWH outputs. At each time step, the five input neurons receive the RSMA values from the five seismic stations, while the ten output neurons simultaneously estimate SWH at the three physical buoy and seven virtual hindcast locations. The ordering of the output neurons is fixed according to the corresponding location ordering in the target matrix used during training.

We have corrected Section 2.5 to explicitly describe the five-input/ten-output architecture and have clarified the multi-site formulation later in the section. This revision does not involve any change to the ANN implementation or results; it corrects and clarifies the description of the model architecture in the manuscript.

I do not find it easy to read the histograms in Figs 2b, 3b because the bins differ between the observed and inferred data, and the histograms are shown as frequency instead of relative frequency / density. Maybe this could be slightly adjusted to make the comparison easier.

Response: We thank the reviewer for this suggestion. We have revised the histograms in Figs. 2b and 3b and also Figs. S1 to S8 in the supplementary material to use identical bin edges and a common bin width of 0.25~m for both distributions. In addition, the histograms are now presented as relative frequency (%) rather than absolute counts. These changes allow the shapes of the observed, hindcast and ANN-estimated SWH distributions to be compared directly.

Fig. 5 caption, why “e.g.” hourly measured – estimated?

Response: Deleted “e.g.” in both Figures. 5 and 6 captions, thank you!

Fig. 6 the line of VB4 is not visible

Response: We thank the reviewer for pointing this out. The VB4 curve is included in Fig. 6; however, the predicted results for VB3 and VB4 are very similar, with only negligible differences over much of the evaluated period. Consequently, the VB3 and VB4 curves largely overlap, causing the VB4 line to be obscured for substantial portions of the figure. We have clarified this in the figure caption to avoid confusion.

For the wave propagation model:

Fig. 7 what are the light blue dots?

Response: Thank you for pointing this out. The light blue dots represent additional seismic receiver locations included in the numerical simulation to record the simulated wavefield onshore. These stations were not used in the observational or ANN analysis presented in this study.

While the simulation gives us an impression of the wave propagation effects, it is for single sources and the final amplitudes will probably also depend on the superposition of multiple sources, so a word of caveat on that may help the readers.

Response: We thank the reviewer for highlighting this important limitation of the numerical simulations. We have clarified in the Numerical Simulation subsection that the single-source simulations used here are intended to isolate and compare source-to-receiver propagation characteristics for contrasting northern and southern source regions, rather than to reproduce the complete observed microseism wavefield. We have also added a caveat to the Discussion noting that superposition of multiple source contributions may further modify the observed wavefield and that the simulated amplitudes should therefore be interpreted primarily in terms of relative propagation behaviour. In addition, we have softened the corresponding interpretation in the manuscript to avoid implying that the single-source simulations uniquely explain the observed spatial variability in ANN performance.

Fig. 8, it would be helpful if the figure noted the quantity shown (displacement?), the RMS of each time series and the source-receiver distance. Also, it might be useful to show all panels with a consistent y-axis.

Response: We thank the reviewer for this suggestion. Figure 8 has been revised to explicitly identify the simulated quantity as vertical velocity, consistent with the SEISMOGRAMS_VELOCITY output used in the SPECFEM3D simulations. The RMS velocity amplitude and source–receiver distance are now reported for each synthetic trace. In addition, identical y-axis limits are used across all four panels to allow direct comparison of the simulated amplitudes between stations and source scenarios.

For readers that are not very familiar with the subject, it might be very useful to deepen the discussion of other findings and complications such as the non-linearity of microseism generation, and of how the method presented here could be utilized to study the problem of seismological ocean monitoring further.

Response: We thank the reviewer for this valuable suggestion. We have added a new subsection to the Discussion addressing these aspects. The revised text explains that secondary microseism amplitudes depend not only on SWH but also on the directional wave spectrum and the interaction of approximately opposing wave components, such that a simple relationship between SWH and recorded seismic amplitude is not necessarily expected. We further discuss how the nonlinear ANN formulation is advantageous for representing this complex relationship, while remaining subject to the physical variability associated with microseism generation and propagation.

Finally, we have expanded the discussion of how the proposed framework could be developed beyond SWH estimation, particularly through the combined analysis of ANN prediction residuals, spatial performance, numerical wavefield simulations, and directional or frequency-dependent observations to further investigate ocean--solid Earth coupling and support seismological ocean monitoring.