Dear Sir or Madam,

Thank you very much for this review. I really appreciate the time you spent on the document I've written and all of the comments you have made.

Please find below my responses to your comments. First in general:

- In the new document version, we now explicitly state that models output an expected maximum envelope height (not individual crests) and we compare to buoy Hmax with this caveat, following prior works that do exactly this comparison (e.g., Barbariol et al., 2019; Janssen, 2003/2015). Including this in the new version.

- Our advances are mainly the dynamic r inverted-peak signal (drop then rebound) as a predictor which no one has ever written about besides my group. I will add a short paragraph in the Introduction making the explicit distinctions to others work. I will include this in this next revision.

  In addition to that please note the changes I've already done in this next version to correct the original paper:

  - I've reprocessed the ECMWF dataset and redid figures 2, 3 and 4. They were corrected and look very different than before.
  - I've added explicit notes in figure legends about FOWD filtering.
  - I've applied the same Hm0 > 1 m cutoff to ERA5/ECMWF.
  - I've added a section/paragraph on different Hmax definitions (zero-crossing vs envelope).

Now please find my responses to your line-specific comments:

1. Ln 8 & throughout (FOWD name). Correct—FOWD = Free Ocean Wave Dataset. I will correct the expansion everywhere and keep the note about its Hm0 > 1 m filter. I will include this in this next revision.

2. P2, 2nd paragraph(rogues not resolved; resolution claim). Agreed: spectral models do not resolve individual rogue waves. I will reword to say higher resolution can better represent spatio-temporal gradients of the envelope statistics, not "resolve" rogue waves, and keep the envelope vs crest caveat. I will include this in this next revision.

3. Ln 57 / Ln 773 (Donelan & Magnusson ref). Thanks—will fix the incomplete citation entry. I will include this in this next revision.

4. P3, Ln 77–91 (differences vs Cicon 2024). See "General points" above; I'll add a crisp comparison paragraph. I will include this in this next revision.

5. Ln 90–91 ("unproven to capture real-world extremes"). I will clarify we specifically mean rogue waves in global phase-averaged models, and acknowledge Cicon (2024) showed limited predictive skill for several traditional parameters; our contribution is the temporal r-signal. I will include this in this next revision.

6. Ln 265–267, 271 (Fig 1 seasonality). I'll re-verify the color scale/legend and align the text with the actual seasonal counts; if any plotting mistake exists, I will correct the figure and add a per-season count table in the Supplement. I will include this in this next revision.
7. Ln 281 ("unstable sea conditions"). I will define this explicitly (e.g., steepness, rapid spectral changes, multi-modal seas, shifting directionality) when the term first appears. I will include this in this next revision.
8. Ln 295–303 (Fig 2 discussion vs results). Point taken. I will revise wording to reflect the good FOWD–ERA5 agreement and note that CY47R1 shows more smoothing than expected in the plotted stats. I will include this in this next revision.
9. Ln 303 (medians 5–7 m claim). You're right—the medians are ~4 m in winter. I will correct these values in the text. I will include this in this next revision.
10. Ln 318–328 (inconsistency with Fig 2). I will update the text to match Fig 2: ERA5 winter Hmax medians are highest and close to FOWD; ERA5's Hs non-outlier range is broader than stated. I will include this in this next revision.
11. Ln 334–346 (whiskers & rogues). Agreed—whiskers alone cannot infer rogue occurrence. I will tone this down and move any rogue-frequency statements to where Hmax/Hs or explicit occurrence metrics are used. I will include this in this next revision.
12. Fig 3 top row (Hmax definitions differ). Accepted. I will add a caption line stating FOWD Hmax (zero-crossing) vs model ⟨Hmax⟩ (envelope-based expectation) and cite accordingly. I will include this in this next revision.
13. Ln 379–384 (narrow model Hmax/Hs). We agree: the narrow model Hmax/Hs reflects the envelope expectation's strong link to Hs; we use the broader FOWD ratio to highlight the missing tail. I'll add one clarifying sentence. I will include this in this next revision.
14. Fig 4 axis ranges. Good suggestion—I will tighten x-axes (e.g., 0–3 for Hmax/Hs, 0–1.5 for BFI) to make differences clearer. I will include this in this next revision.
15. Ln 419–435 (skewness & kurtosis interpretation). You're right—these are moments of the surface-elevation distribution (and the panel shows excess kurtosis). I will correct the terminology and rewrite the discussion accordingly. I will include this in this next revision.
16. Ln 468 (wording). Will change to "Hmax divided by Hs." I will include this in this next revision.
17. Ln 482 (FOWD vs raw CDIP discrepancy). Agreed this is surprising. I will re-audit buoy IDs, time windows, units, and QC alignment; if the mismatch stems from alignment/QC, we will correct Fig 5; otherwise we'll explain the cause. I will include this in this next revision.
18. Ln 490–493 (0.25–1.5 Hz vs "lower frequencies"). I'll remove the contradictory phrasing and state consistently that we observe spectral narrowing with elevated relative energy

in 0.25–1.5 Hz, indicating wind-sea injection and increased coherence around the dominant band. I will include this in this next revision.

19. Ln 515–518 (“swell reinforces background spectrum”). I will replace this with a physically precise statement about linear superposition of partitions and transient group beating; we do not imply deep-water energy transfer from low to high frequencies by nonlinear interactions here. I will include this in this next revision.

20. Ln 530–535 (Häfner threshold). Agreed—Häfner shows monotonic increase of rogue probability with r; we will avoid implying an r > 0.6 threshold and emphasize our dynamic r-transition result. I will include this in this next revision.

21. Ln 535–571 (expand dynamic r analysis). Thank you—we will expand with an additional case (or two) and add a concise schematic of the drop-and-rebound detection logic in the Supplement. I will include this in this next revision.

22. Ln 582–584 (null hypothesis / false positives). Agreed—we will quantify how often inverted-r peaks occur without a rogue wave (false-positive rate) for the same stations/period and report precision/recall. I will include this in this next revision.

23. Ln 597–598 (probability vs binary). Good point—we will rephrase and, where possible, express the dynamic r signal as an event probability (e.g., conditional frequency of rogues within a window after an inverted-r event) to compare conceptually with Cicon’s probability. I will include this in this next revision.

24. Ln 605–689 (rewrite Conclusions). Agreed—we will refocus the Conclusions on the practicality of the dynamic r-evolution signal, temper broad claims, and clearly separate what is shown by our data from what is proposed for future work. I will include this in this next revision.

25. Fig 3/4 captions and Methods cross-links. To avoid any residual ambiguity, I will add one-line cross-references in the captions pointing to the Methods paragraph where the Hmax definitions and filtering (Hm0 > 1 m) are specified. I will include this in this next revision.

Thank you very much for your help.

Best Wishes,

Laura