

## Human review

I appreciate the opportunity to review “*Assessing subseasonal forecast skill for use in predicting US coastal inundation risk*”. This is a well-needed study, which I think merits speedy publication. I only have minor revisions to recommend considering (\* deserving more attention), as detailed below (since the manuscript numbering is every 5<sup>th</sup> line, numbers are approximate).

Also, please accept my apology for tardiness in submitting this review. While I might be slower than the ideal reviewer, I do wonder if more attention to the availability of much speedier AI reviewers is warranted. For the authors’ and editor’s consideration, I’ve provided below my review a copy of the review I sought from ChatGPT o3.

L25 “trillions of dollars in property...” is a bit unclear.

L45 Could also cite Long et al. 2021. Specifically, the finding of no seasonal skill for the U.S. East Coast.

L70 Also consider Widlansky et al. 2017, which introduced the approach for operational seasonal sea level forecast products in the tropical Pacific islands (i.e., high-tide outlook -- = tide prediction + sea level trend + sea level anomaly forecast).

L70 “along shallower coasts” Do you mean “coasts along shallower shelves”?

L75 Abbreviate ECMWF

L75 Re: role of altimetry assimilation on forecasting skill, consider Widlansky et al. 2023.

L90 Abbreviate CNRM

L100 Feng et al. 2024 assessed a number of ocean reanalyses for U.S. Gulf and East Coast monthly sea levels.

L110 Method of separating into three components the HTF outlook was originally developed by Widlansky et al. 2017 (for tropical Pacific Islands).

L110 Clarify a bit about how the trend contribution is assessed (I found the actual assessment sufficient, just not its description).

L120 “data is scored and available” a bit unclear.

L130 IB effect in the subheading could be abbreviated.

L130 “remove static” and dynamic atmospheric pressure effects (i.e., the Dynamic Atmospheric Correction applied to altimetry measurements).

L130 The explanation why forecasts do not include the IB-effect is incomplete. The altimetry assimilation does not include the IB-effect AND models have the IB-effect turned off.

L140 Missing subscript in the density unit.

L140 Consider Feng et al. 2024 again here.

\*L150 The VLM correction aspect of this study is overly complex, and perhaps worse than taking a much simpler approach. VLM spatial variability is substantial on sub 1 deg resolution. Why not approximate VLM as the difference between the altimetry (or GLORYS12) and tide gauge observation?

L165 “respectively” unclear

L205 “near the Bahamas” IFS seems more skillful there.

\*L220 Figure 1 should also show the damped-persistence skill in map form.

L355 Consider the IB-effect assessment presented in Long et al. 2021

\*L385 Figure 10b is an example of why a better (simpler) approach to the VLM correction is warranted.

L455 Again, consider the IB-effect assessment presented in Long et al. 2021

L480 The last paragraph seems a bit incomplete.

*Prompt: Please review this manuscript.*

<https://egusphere.copernicus.org/preprints/2025/egusphere-2025-897/egusphere-2025-897.pdf>

ChatGPT o3 review

## **Overall evaluation**

This manuscript tackles a timely, clearly-defined problem: *can current subseasonal-to-seasonal (S2S) forecast systems add skill – beyond damped persistence – to operational high-tide-flood (HTF) outlooks for U.S. tide-gauge sites?* Using a 20-year hindcast set (2000–2019) from two global coupled models (ECMWF-IFS and CNRM) that assimilate altimetry, the authors compute deterministic (ACC, RMSE) and probabilistic (ROCSS, RPS) skill for non-tidal residual (NTR) anomalies at 80+ gauges, test simple post-processing steps (inverse-barometer correction and vertical-land-motion adjustment) and compare skill to damped persistence. They find that IFS beats damped persistence out to 5–6 weeks and that modest post-processing gives further gains at short leads. Results are positioned as evidence that NOAA’s monthly HTF product could incorporate model guidance today. The study is well organised, methodologically transparent, and squarely within the scope of *EGU Ocean Science*. I recommend **minor revision** before publication.

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## **1. Major comments**

#	Comment	Recommendation
M1	<p><b>Justification of model choice.</b> The manuscript asserts that IFS and CNRM are chosen because they assimilate altimetry and predict global-mean sea level, “whose absence in other systems complicates assessment” <a href="#">EGUsphere</a>. But several S2S systems (e.g., NCEP GEFSv12, JMA CPS2, BoM ACCESS-S2) now include dynamic sea-level components.</p>	<p>Add a short table (or SI) listing major S2S models and clarifying why each was or was not analysed. This will strengthen the argument that the two systems are genuinely state-of-the-art and representative.</p>
M2	<p><b>Downscaling / coastal processes.</b> Using the nearest ocean grid point ignores shelf dynamics, bathymetry, and local surge processes acknowledged by the authors (lines 60–67) <a href="#">EGUsphere</a>. While a full dynamical downscaling is beyond scope, the implications deserve deeper discussion.</p>	<p>Include a sensitivity test (perhaps for two contrastive regions) using an along-shelf average or a simple statistical bias-corrector to demonstrate robustness, or else clarify that such work is left to future studies.</p>
M3	<p><b>Vertical land motion (VLM) adjustment.</b> The method for VLM correction is briefly described but key details are missing: data sources (GPS? GIA models?), temporal resolution, and whether tectonic/anthropogenic motions are separated.</p>	<p>Provide a concise paragraph in Methods plus a reference to underlying datasets or provide the VLM time-series in the supplement.</p>
M4	<p><b>Forecast ensemble size and handling of lead-time drift.</b> The skill metrics are computed on ensemble means, but the manuscript does not state how many ensemble members each hindcast system provides and whether mean bias drifts with lead.</p>	<p>Report ensemble sizes and show lead-dependent mean bias (could be a small inset figure). Specify whether bias correction is applied before or after computing skill.</p>
M5	<p><b>Practical guidance to HTF operations.</b> The conclusions state that “current forecast systems have sufficiently high skill to be used in support of operational coastal flood guidance” <a href="#">EGUsphere</a>, yet no example decision metric is shown.</p>	<p>Add one applied example (e.g., conditional flood probability at Charleston for a selected event) to illustrate how the models would change an existing NOAA HTF outlook.</p>

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## 2. Minor / technical comments

1. **Terminology consistency** – sometimes “non-tidal residual”, elsewhere “sea-level anomaly”. Standardise to NTR (define once).
  2. **Figure 2 colour-bar** – red/blue diverging palette is illegible for colour-blind readers; consider a perceptually-uniform scheme.
  3. **Lines 115–120** – cite the version numbers of IFS (cycle 47r3?) and CNRM (CM6-1).
  4. **Equation 3** – clarify whether trend term is removed before ACC computation or only from forecasts.
  5. **ROCSS significance** – briefly state the bootstrap or Monte-Carlo procedure used to assess the 95 % confidence shown.
  6. **References** – duplicate DOI appears for Long et al. 2021 and “submitted” paper (line 590) [EGUsphere](#); check.
  7. **Typos** – “Balmeseda” → “Balmaseda” several times; “probabilistic skill scores” not “score” in abstract.
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## 3. Strengths

- Addresses an operationally important gap (2–6 week lead flood guidance).
- Uses long, consistent hindcast sets and multiple complementary skill metrics.
- Post-processing steps are simple and reproducible, thus attractive to agencies.
- Manuscript is generally well written and logically structured.

## 4. Summary recommendation

With the clarifications suggested above – particularly the added context on model selection, VLM methodology, and an applied forecast example – the paper will provide a convincing, actionable assessment of subseasonal coastal-sea-level predictability and be a valuable reference for both research and operations.