Review of manuscript: "Spatiotemporal properties of intrinsic sea level variability along the Southeast United States coastline" by Carmine Donatelli (*), Christopher M. Little, Rui M. Ponte, Stephen G. Yeager

First of all, we want to thank the Editor, and two Reviewers for their comments. We believe that the manuscript strongly benefitted from them. We addressed all points in the text, and we report a detailed response to each of them below (our response in bold).

Reviewer 1

I appreciated the opportunity to review this manuscript. The study presents an engaging analysis and contributes to the ongoing discussion on intrinsic sea level variability. The identification of a connection between along-shore variability and a region across the shelf-break is a noteworthy and potentially valuable insight. I have assigned minor review since further work is needed for the following points:

- The current structure places essential methodological details in the Appendix. I recommend integrating the Data and Methods section into the main body of the paper to enhance transparency and reproducibility. If this is not possible due to the "Letter" format, I request at least to answer to the points that I raise concerning the Appendix

Great comment. See answers from 4 to 8.

- The discussion would be more impactful if it were more explicitly connected to existing findings in the literature.

Thanks for this suggestion. See answer 15 (and also answer 18).

Please note that the questions raised below are not intended merely for a response to the reviewer, but rather highlight areas where the manuscript lacks sufficient explanation. These should be addressed directly in the main text.

Both major and minor points are described in the line-by-line review below. Note that the comments about the Appendix are anticipated, since I considered that section as a "Data&Method", which anticipate the results

Line 24:

Could you please clarify what type of predictions are being referred to here? Additionally, over what time scales are these predictions intended to apply?

1. This part now reads:

"Coastal ecosystems, communities, and economies are highly susceptible to sea level variability over a wide range of time scales (e.g., Rashid et al., 2021; NOAA, 2022).

Accurate sea level predictions are needed for mitigation and adaptation purposes and for effectively managing risks associated with sea level variability."

We specified the time scales of interest (monthly to interannual) in the next sentence (see answer 2).

Lines 25–26:

The phrase "assessment of the capabilities" is somewhat vague in scientific terms. Are you referring to a validation process? Furthermore, when you mention "ocean forecasting," could you specify the time scales involved?

2. This part now reads:

"Such predictions will benefit from: 1) improved understanding of relevant drivers of regional coastal sea level variability, and 2) assessment of the representation of monthly to interannual coastal sea level variability in dynamic models utilized for operational ocean forecasting."

Line 52:

Earlier, you defined "intrinsic" variability as being generated by the ocean itself, independent of atmospheric forcing. In this context, what exactly do you mean by "sources" of intrinsic sea level variability?

3. We rephrased this sentence as follows:

"The Southeast Coast of the United States (from now on, SEUS) has a strong intrinsic component at sub-annual to interannual timescales (e.g., Close et al., 2020; Little et al. 2024). However, previous studies do not elucidate: (i) the SEUS along-coast spatial structure, and (ii) how coastal variability relates to offshore variability over space and time."

"APPENDICES"

Why are the Data and Methods sections placed in an appendix?

Given their importance for evaluating the results, I recommend restructuring the manuscript to integrate these sections into the main body of the text.

4. OS Letters have less than 2500 words in the main text. To respect the 2500-word limit, we provided details of data and methodology in the appendix. However, we have now included more details about the methodology in the Introduction section.

In the Introduction section (lines 59-63):

“we utilize monthly sea surface height (SSH) fields from high-resolution (HR) forced ocean/sea-ice (FOSI) and repeat-year-forcing (RYF) (Stewart et al., 2020) simulations performed using the Community Earth System Model at 0.1° horizontal resolution (Chang et al., 2020; Yeager et al., 2023; Little et al., 2024). We utilized the HR FOSI simulation to evaluate whether the model faithfully represents observations, and the HR RYF simulation to cleanly estimate intrinsic sea level variability.”

Appendix A appears to be written for a highly specialized audience, which may limit accessibility. It would be helpful to include a brief explanation of the HR FOSI and HR RYF simulations. Specifically:

What processes do these simulations represent?

5. In Appendix A (lines 194-196):

“We employed an HR FOSI simulation with a spatial resolution of 0.1° (~10 km) to analyze monthly SSH fields from 1993 to 2018. The HR FOSI simulation represents the response of the ocean and sea ice to prescribed atmospheric forcing (e.g., wind, temperature).”

In Appendix A (lines 205-207):

“To cleanly quantify intrinsic variability, we use an HR RYF (repeat-year forcing) simulation. The HR RYF simulation was carried out by applying a single year of JRA55 boundary conditions from May 2003 to the end of April 2004.”

What type of ocean models are they based on?

6. The CESM1.3 uses a complex system of equations to simulate the Earth’s climate. These equations (e.g., conservation of momentum, mass, energy, equation of state) represent various physical processes within the atmosphere, ocean, land, and ice components of the Earth system.

In Appendix A (lines 196-198):

“This simulation was performed using the global Community Earth System Model version 1.3 (CESM1.3) following the Ocean Model Intercomparison Project version 2 (OMIP2) experimental protocol (Griffies et al., 2016).”

Why were these particular simulations selected over others?

7. We added the following lines in Appendix A (see lines 212-214):

“The HR RYF simulation was selected because of its unprecedentedly high resolution (0.1°), which allows us to examine the linkage between offshore and along-coast intrinsic sea level variability.”

Additionally, please clarify the rationale for applying 32 years of HR RYF data, forced with boundary conditions from a single year (2003–2004). What is the scientific justification for this approach?

8. The rationale of this approach is described in Stewart et al. (2020) (doi:10.1016/j.ocemod.2019.101557). We added the following lines (206-212) in Appendix A:

"The HR RYF simulation was carried out by applying a single year of JRA55 boundary conditions from May 2003 to the end of April 2004. The May 2003-April 2004 year is characterized by low (non-anomalous) values for major climate modes. The May-April cycle is to avoid forcing discontinuities in mid-winter. By applying the same annual forcing in each year, interannual variability in sea level can be mainly attributed to ocean intrinsic processes. Here, we analyzed the last 32 years of monthly outputs from a 70-year-long HR RYF simulation to examine the spatiotemporal properties of intrinsic sea level variability along the SEUS coastline, over the continental shelf, and adjacent deep waters."

Is monthly resolution sufficient to capture the temporal scales of intrinsic sea level variability relevant for forecasting? This question ties back to the earlier point regarding the nature of the forecasts being discussed. Why was the analysis limited to monthly resolution, especially considering the availability of high-frequency observational data such as tide gauge records (e.g., GESLA), daily altimetry grids (noting their limitations in effective temporal resolution)
Why was the Measure dataset selected for gridded sea level data? Measure uses a maximum of two altimeters. How does its spatial resolution compare to more comprehensive products, such as the one provided by Copernicus (SEALEVEL_GLO_PHY_L4_MY_008_047), which incorporates all available altimeters?

9. Great questions.

1) The primary goal of our study is to understand the linkage between offshore and along-coast intrinsic sea level variability. The monthly resolution of the numerical outputs is sufficient to capture the propagation of sea level anomalies from offshore to the coast; however, as highlighted in the Discussion section, this temporal resolution is insufficient to capture the propagation of coastal trapped waves. For this project, we do not have numerical outputs at higher temporal resolution

This limitation of our study is highlighted in the Discussion section (lines 172-173):

"Given the disparity between open-ocean and coastal wave speeds, daily outputs of SSH fields will be required to capture the along-coast propagation of sea level anomalies."

2) It is challenging to partition forced and intrinsic variability in observations. Thus, although high-frequency observational datasets are available, we can only evaluate intrinsic variability using the HR RYF simulation.

3) We only used the MEASURES dataset as a qualitative metric of the spatial structure of total sea level variability. Comparing this dataset with other altimetry products, although interesting, is beyond the scope of our study.

Line 219: What is a ball tree algorithm? A brief explanation would be helpful for readers unfamiliar with this method.

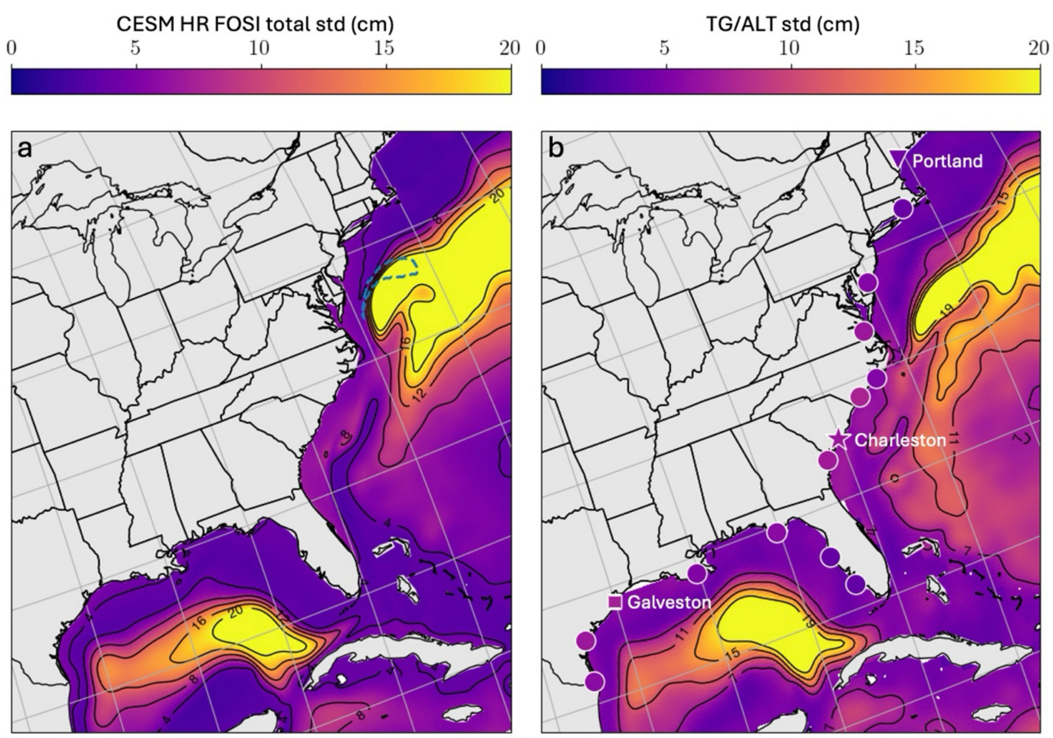
10. In Appendix B (lines 226-228):

"To compare model output and sea level observed by TGs, we extracted the SSH in the closest model grid points to each TG using a ball tree algorithm (i.e. a ball tree algorithm is an efficient means of finding model grid points closest to a list of TG locations; more details can be found https://scikit-learn.org/stable/modules/neighbors.html)."

Figure 1:

The tide gauge (TG) points are currently not visible. Could you clarify why only three tide gauges are shown in this region, which is among the most extensively monitored globally? Additionally, I suggest adding a panel that shows the difference between panels a and b. For clarity in the subsequent discussion, it would also be helpful to indicate the blue area from Figure 3 within Figure 1.

11. (i) We added more TGs in Figure 1b and made the TG points more visible.



(ii) The GS path is biased in the HR FOSI simulation (see lines 178-181). Therefore, the

difference between panels a and b would not be very informative since it would highlight large differences between the model and the altimetry product in deep waters. Instead of computing the difference between total sea level variability from the HR FOSI simulation and the altimetry product, we compared TG observations and the HR FOSI simulation to evaluate model's performance in representing coastal sea level (Table 1).

Tide gauge	*R^2*	*RMSE (cm)*
Portland (Maine)	*0.81, 0.80, 0.78, 0.8*	*2.70, 2.73, 2.88, 2.79*
New Port	*0.80, 0.78, 0.78, 0.79*	*2.52, 2.63, 2.65, 2.59*
Cape May	*0.86, 0.86, 0.85, 0.86*	*2.80, 2.85, 2.90, 2.85*
Sewells Point, Hampton Roads	*0.85, 0.85, 0.85, 0.85*	*3.51, 3.57, 3.53, 3.54*
Beaufort (North Carolina)	*0.70, 0.69, 0.73, 0.71*	*4.01, 4.11, 3.93, 3.99*
Wilmington	*0.49, 0.51, 0.54, 0.52*	*6.77, 6.72, 6.62, 6.66*
Charleston	*0.66, 0.71, 0.73, 0.66*	*5.19, 4.85, 4.79, 5.22*
Fort Pulaski	*0.64, 0.68, 0.71, 0.63*	*5.62, 5.27, 5.32, 5.75*
Naples	*0.41, 0.43, 0.51, 0.43*	*4.10, 3.92, 3.73, 3.94*
St. Petersburg	*0.48, 0.46, 0.53, 0.44*	*3.85, 3.79, 3.60, 3.93*
Apalachicola	*0.57, 0.54, 0.58, 0.52*	*4.80, 4.90, 4.73, 5.02*
Grand Isle	*0.67, 0.67, 0.72, 0.66*	*3.99, 3.95, 3.68, 4.00*
Galveston	*0.76, 0.78, 0.81, 0.75*	*4.76, 4.59, 4.37, 4.80*
Rockport	*0.73, 0.75, 0.77, 0.73*	*4.42, 4.37, 3.98, 4.65*
Port Isabel (Texas)	*0.7, 0.72, 0.75, 0.71*	*4.21, 4.05, 3.82, 4.15*

Table 1. Comparison between HR FOSI simulation and TG observations. For each ensemble member, we reported coefficient of determination (R^2) and root mean square error (RMSE).

(iii) We added the blue area from Figure 3 within Figure 1a.

Line 77:

The statement "the model generally underestimating..." may be influenced by the temporal resolution of the data. This result could differ significantly if daily rather than monthly data were used. It is worth noting that satellite observations typically underestimate high-frequency variance in shelf circulation.

12. We rephrased this part as follows (lines 78-79):

"with the model generally underestimating the observed monthly total sea level standard deviation over the shelf by 10-20%"

Line 88:

Please clarify that the results discussed here pertain specifically to a region dominated by a western boundary current. This is not a general characteristic of the deep ocean, as illustrated globally in Close et al. (2020), Figure 1c.

13. This part now reads (lines 91-94):

“we found that the intrinsic standard deviation represents 10-30% of the total sea level standard deviation on the continental shelf south of Cape Hatteras and up to 100% of the total standard deviation in deep waters (Fig. 1g; note that these results pertain to a region dominated by a western boundary current).”

Section 2.3:

The propagation described in this section is intriguing and might be better resolved using higher temporal resolution data. The cited study by Close et al. (2020) used “successive 5-day averages.” Could you explain why higher temporal resolution was not used in your analysis, and why it was feasible in other studies?

14. See answer 9.

Line 158:

The finding regarding the along-coast coherence of PC1 is particularly interesting. I recommend expanding the discussion in light of existing observational studies. For example, Oelsmann et al. (2024) (https://doi.org/10.1029/2024jc021120) analyzed the along-shore coherence of monthly sea level variability using tide gauges and coastal altimetry. Their Figure 7 shows that, along the U.S. East Coast (a western boundary), the observed clusters do not strongly correlate with interannual variability from typical climate indices, unlike what is observed along eastern boundaries. Your results, consistent with previous work, suggest that these clusters are linked to intrinsic dynamics. Notably, both your model and the observations show a separation at Cape Hatteras.

15. Great suggestion. See lines 189-192 in the Discussion section:

“This study provides a better understanding of (i) the physical processes governing offshore-shelf and shelf-to-shelf communication along the SEUS coastline, and (ii) the relationship between offshore GS variations and coastal sea level (e.g., Ezer, 1995; 2013; Wu & He, 2025). It may also help with the interpretation of observational datasets (e.g., Oelsmann et al., 2024).”

Line 158 (continued):

Please specify that the “robust 2–3-month lag” refers to the offshore region highlighted in Figure 3.

16. Done.

Lines 160–166:

These statements are somewhat unclear. Figure 3c shows that the lag-correlation of PC1 peaks at around 3 months along the entire shelf. How does this reconcile with the claim that sea level anomalies (SLAs) travel much faster, at sub-monthly scales, once they pass Cape Hatteras, which are "unseen" in your experiments?

17. We revised this part. Specifically, we better explained the oceanic mechanisms controlling the propagation of sea level anomalies from the off-shelf region to the coast. Also note that Figure 3c shows the lag-correlation between the PC1 and SSH fields with a lag of 1 month. The correlation is maximized at zero lag.

In the Discussion section (see lines 162-170):

"The along-coast coherence of PC1, and the robust 2-3 month lag between off-shelf and coastal sea level variability (see offshore region in Fig. 4), inform hypotheses about the underlying oceanic mechanisms controlling the propagation of sea level anomalies from the off-shelf region to the coast (e.g., Wu & He, 2025). Our results are consistent with propagation along the continental slope via topographic Rossby waves (e.g., Wise et al., 2018; Hughes et al., 2019; Wise et al., 2020). The latter travel with a speed of a few centimeters per second at these latitudes (first baroclinic mode), roughly consistent with the time lag we quantified between the off-shelf region and the PC1 (barotropic Rossby waves may also be involved in this transfer process). Once sea level anomalies break the potential vorticity barrier and penetrate onto the shallow continental shelf (e.g., Wise et al., 2020), they are transmitted via Kelvin waves traveling at a few meters per second (first baroclinic mode); such signals can travel from Cape Hatteras to the Gulf of Mexico in less than a month."

Reviewer 2

This paper presents a novel and valuable contribution to our understanding of intrinsic sea level variability along the U.S. East Coast. The authors use high-resolution ocean model simulations to demonstrate for the first time that 10–30% of the shelf sea level standard deviation may originate from ocean-internal processes alone. They also examine lead-lag relationships to assess predictability and explore possible mechanisms underlying a major mode of variability that is dominant south of Cape Hatteras. Overall, this study represents an important step toward identifying the full range of processes contributing to monthly-to-interannual sea level variability along the Northeast U.S. coastline.

The overall presentation of the results, methods, data, and conclusions—along with the manuscript's structure, language, and conciseness—is very strong and concise. I have only a few questions that should potentially be addressed. Note that most of my questions aim to clarify which processes could be relevant in the setup where only intrinsic variability is allowed, compared to the setup that includes external variability. Some of these comments may be somewhat subjective, as I occasionally found it difficult to determine which previous studies should be referenced here, or not.

Major comments:

Previous studies on mechanisms linking open-ocean and coastal sea level changes:

There is a substantial body of literature investigating the drivers of sea level variability along the U.S. East Coast (e.g., Frederikse et al., 2017; Calafat et al., 2018; Piecuch et al., 2018; Dangendorf et al., 2021, 2023; Steinberg et al., 2024; Wang et al., 2024). However, these studies are not cited or discussed in the introduction. I think this is likely because some of the previously discussed mechanisms are not relevant (with regards to intrinsic variability), or because these works primarily analyze historical observations or model simulations that include externally forced variability, rather than isolating intrinsic (unforced) variability, as is done in this study.

That said, I wonder whether some of the physical mechanisms proposed in those earlier studies—such as the link between offshore steric height anomalies and bottom pressure fluctuations on the shelf in modulating coastal sea level (including the communication along the coast via coastally trapped waves)—might also be relevant in the context of intrinsic variability. If such mechanisms are indeed applicable regardless of the forcing source, then it would be appropriate to acknowledge relevant studies and situate your findings within the broader framework of previously proposed mechanisms.

In the conclusions, you note: “the ocean mechanisms involved in the communication of off-shelf anomalies to the coast (and from Cape Hatteras to the Gulf of Mexico) are likely the same as those regulating the transfer of other sea-level signals, regardless of their forced or intrinsic nature.” This statement suggests that some mechanisms identified in earlier studies (e.g., Calafat et al., 2018; Dangendorf et al., 2021; 2023; Steinberg et al., 2024) could in fact be relevant to your results, particularly given your finding of significant coherence between the leading coastal principal component (PC1) and off-shelf sea level, especially at periods longer than one year. Overall, it may be helpful for the reader to explain (maybe in introduction), which of the previously discussed forcings (e.g., wind forcing) are not relevant for your study focusing on intrinsic variability (just to provide a little bit more context).

18. Great comment.

1) We cited the suggested works in the Introduction and Discussion sections. All these studies, as pointed out by the Reviewer, do not focus on intrinsic sea level variability, but similar oceanic mechanisms to those proposed in our study are described.

In the Introduction section (lines 48-52):

"This paper focuses on characterizing the spatiotemporal properties of intrinsic sea level variability along the Southeast Coast of the United States (including the Gulf of Mexico), where societal vulnerability to sea level variability is high and increasing (e.g., Thatcher et al., 2013). The majority of the studies in this area focus on atmospherically-forced variability (Frederikse et al., 2017; Calafat et al., 2018; Piecuch et al., 2018; Wang et al., 2024) and less is known about the influence of oceanic intrinsic processes on coastal sea level."

In the Discussion section (lines 183-187):

"Although the origin of atmospherically-forced and intrinsic sea level variations is different, our study reveals that the oceanic mechanisms involved in the communication of off-shelf anomalies to the coast (and from Cape Hatteras to the Gulf of Mexico) are similar to those regulating the transfer of some previously described forced sea-level signals (e.g., Calafat et al., 2018; Dangendorf et al., 2021; 2023; Steinberg et al., 2024)."

In the Discussion section, we also provided a clearer explanation of how sea level anomalies propagate from the GS detachment region to the coast (see lines 162-170). Specifically, we referred to Wu & He (2025):

"The along-coast coherence of PC1, and the robust 2-3 month lag between off-shelf and coastal sea level variability (see offshore region in Fig. 4), inform hypotheses about the underlying oceanic mechanisms controlling the propagation of sea level anomalies from the off-shelf region to the coast (e.g., Wu & He, 2025). Our results are consistent with propagation along the continental slope via topographic Rossby waves (e.g., Wise et al., 2018; Hughes et al., 2019; Wise et al., 2020). The latter travel with a speed of a few centimeters per second at these latitudes (first baroclinic mode), roughly consistent with the time lag we quantified between the off-shelf region and the PC1 (barotropic Rossby waves may also be involved in this transfer process). Once sea level anomalies break the potential vorticity barrier and penetrate onto the shallow continental shelf (e.g., Wise et al., 2020), they are transmitted via Kelvin waves traveling at a few meters per second (first baroclinic mode); such signals can travel from Cape Hatteras to the Gulf of Mexico in less than a month."

Guo et al. (2023) reveal that GS path oscillations excited by atmospherically-forced variability (e.g., NAO) control a significant fraction of the total SSH variance within the GS detachment region. In our study, SSH variability in this region may be linked to intrinsic movements of the GS path, as only intrinsic variability is allowed in the HR RYF simulation (see also answer 25). We highlighted this point in the Discussion section (see lines 174-178):

"The frequency band in which the along-coast intrinsic mode and the off-shelf region exhibit high coherence suggests that SSH within the off-shelf region might be influenced by variations in the GS position excited by intrinsic oceanic variability (e.g., Quattrocchi et al., 2012; Gregorio et al., 2015). More specifically, frequencies smaller than 0.9 year^{-1} seem consistent with interannual GS path oscillations, which are known to control a significant fraction of the total SSH variance within the GS detachment region (e.g., Guo et al., 2023)"

I think that, if some of the previously investigated mechanisms are relevant, the authors should try to emphasize a bit better why and how their findings, e.g., the detection of the dominating and coherent first mode south of Cape Hatteras, is different or similar to previous findings, or novel, considering the study on coherent sea level in that region by Calafat et al., 2018. That 'This coastal mode is coherent with sea level along the Gulf Stream axis after detachment from Cape Hatteras' is a central point of this paper (in the abstract), and therefore it's novelty needs to better clarified in light of previous research. You address several of these points in the discussion section (citing Wise et al., 2018; Hughes et al., 2019; Wise et al., 2020), but it may strengthen the manuscript to introduce some of these mechanisms earlier, in the introduction.

19. We stressed the novelty of our results in the revised manuscript. Specifically, our study demonstrates, for the first time, that 10–30% of the shelf sea level standard deviation originates from ocean-internal processes alone and is linked to an off-shelf region along the GS axis after detachment from Cape Hatteras. We also reveal that intrinsic variability, although chaotic, presents (i) a large-scale pathway connecting deep waters and the SEUS coastline, and (ii) a predictable lag between offshore and along-coast sea level.

In the Discussion section (see lines 155-159):

"Our analyses revealed that, at monthly to interannual timescales south of Cape Hatteras, intrinsic processes meaningfully contribute to sea level variability, reaching up to 30% of the total monthly sea level standard deviation on the continental shelf. A common intrinsic sea level mode, largest between Charleston and the Florida Straits, but coherent around the Gulf of Mexico, is correlated with sea level variability in the detached GS through a large-scale pathway connecting deep and shelf waters."

In the Discussion section (see lines 182-183):

"CESM simulations show that intrinsic sea level variability is smaller (in a time-aggregated sense) than forced sea level variability; however, it is not negligible and, as noted, may have inherent predictability."

Another key finding of the study is that "Intrinsic sea level variability in the detached Gulf Stream leads the coastal mode by 2–3 months, suggesting that intrinsic coastal sea level variability may exhibit predictability." I agree that this is an intriguing result from a mechanistic standpoint. However, I wonder how relevant this is in practical terms—specifically, what implications it holds for the predictability of sea level in the real world.

If I understand the results correctly, intrinsic variability accounts for roughly 20–30% of the total standard deviation in sea level (based on monthly data), relative to the forced simulations. Assuming a maximum coherence of ~0.6 between offshore sea level and the leading coastal mode (for periods >1 year), this corresponds to roughly 36% of the variance in the coastal mode being explained by offshore signals. Even if we assume that the first mode captures all of the intrinsic sea level variability, the offshore signal would then explain about 7–11% of the total sea level variability (i.e., 0.36 × 0.2–0.3) in a realistically forced setup.

If this reasoning is approximately correct, then a key question arises: To what extent does this lead–lag relationship actually translate into meaningful predictability of sea level in the real world, where forced variability dominates? Clarifying this point would strengthen the practical interpretation of the findings and help position the study within the broader context of coastal sea level forecasting.

20. In the Discussion section (see lines 183-189):

"Although the origin of atmospherically-forced and intrinsic sea level variations is different, our study reveals that the oceanic mechanisms involved in the communication of off-shelf anomalies to the coast (and from Cape Hatteras to the Gulf of Mexico) are similar to those regulating the transfer of some previously described forced sea-level signals (e.g., Calafat et al., 2018; Dangendorf et al., 2021; 2023; Steinberg et al., 2024). As such, our findings help understand the role of GS path variations (forced and intrinsic) on coastal sea level, and we suggest that sea level forecasting efforts will benefit from further studies of intrinsic variability along the SEUS coastline and elsewhere."

Minor comments (some points are related to the major comments):

Abstract L14-17: I think it would be helpful to more clearly articulate what distinguishes your study from existing research. The separation of sea level correlation patterns around Cape Hatteras has already been documented in observational studies. Therefore, I would suggest emphasizing what makes your work novel. Is this the first study to shows that this separation also exists in intrinsic SSH variability, or is this the first study that shows this phenomenon using climate or ocean models? And/or is your key contribution the finding that this separation may be predictable? Clarifying this would strengthen the positioning of your study.

21. We explained the main novelty of our study in the main text (see answers 18-19) and specified that the coastal mode we identified is purely intrinsic in the Abstract.

L52-55: Related to the 1. major comment: Are the results of previous studies investigating the causes of sea-level variability along the U.S. East Coast (e.g., Frederikse et al., 2017; Calafat et al., 2018; Piecuch et al., 2018; Dangendorf et al., 2021; 2023; Steinberg et al., 2024; Wang et al., 2024) relevant here, or not?

22. Yes, these studies are relevant, and we now acknowledged the suggested papers in the Introduction and Discussion sections. See answers 18-19-20.

L63: Minor comment: Shouldn't the methods not be in the main text in this journal?

23. OS Letters have less than 2500 words in the main text. To respect the 2500-word limit, we provided details of data and methodology in the appendix. However, we have revised the Introduction and added a few lines regarding the methodology (lines 59-63).

Methods/Appendix:

L194: Please explain what 'four consecutive cycles' means here already.

24. Done.

L202: 'The HR RYF simulation was carried out by applying a single year of JRA55 boundary conditions (May 2003 to the end of April 2004).' What if that year has a substantially different atmospheric variability (or annual cycle amplitude) than the average of 1958-2018? Wouldn't that not bias your estimates? Would it make sense to use the 1958-2018 average as a forcing?

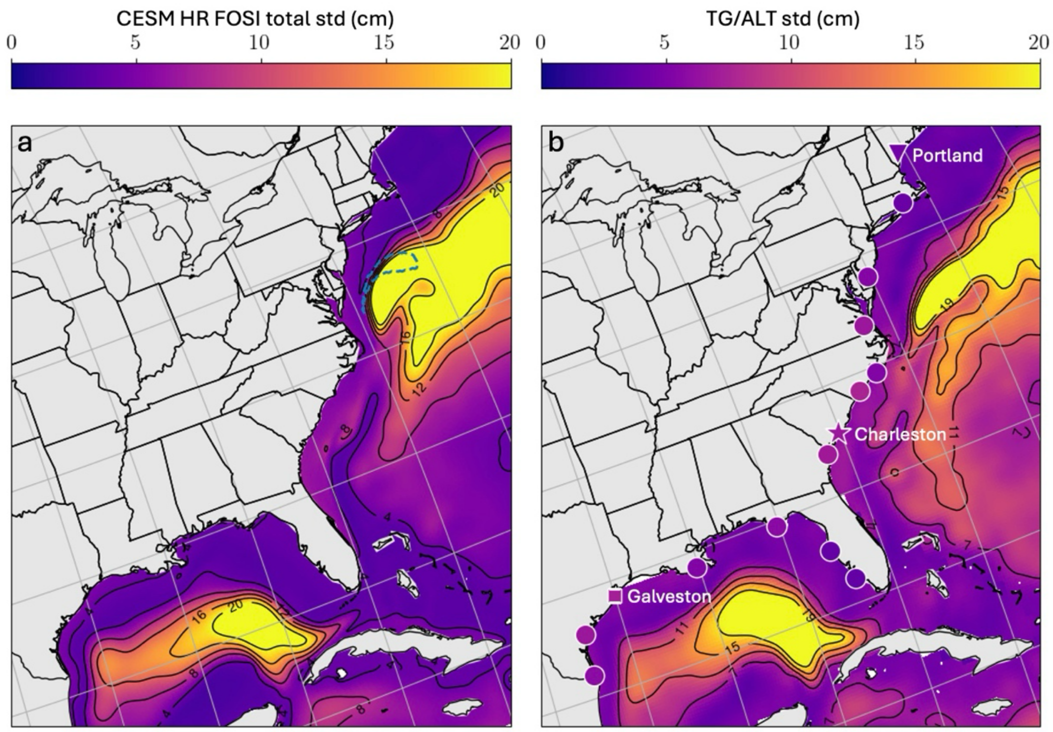
25. The rationale of this approach is described in Stewart et al. (2020) (doi:10.1016/j.ocemod.2019.101557).

We added the following lines in Appendix A (lines 206-212):

"The HR RYF simulation was carried out by applying a single year of JRA55 boundary conditions from May 2003 to the end of April 2004. The May 2003-April 2004 year is characterized by low (non-anomalous) values for major climate modes. The May-April cycle is to avoid forcing discontinuities in mid-winter. By applying the same annual forcing in each year, interannual variability in sea level can be mainly attributed to ocean intrinsic processes. Here, we analyzed the last 32 years of monthly outputs from a 70-year-long HR RYF simulation to examine the spatiotemporal properties of intrinsic sea level variability along the SEUS coastline, over the continental shelf, and adjacent deep waters."

Fig1. b) It may be helpful to improve visibility of TGs (add white marker edge color?). It would also be interesting to see some more available TGs in this region (showing STD). C-e) What are the numbers at the bottom in the time series plots? g) as you write in the text, this fraction can be as large as 1 (100%). Could it actually be larger than visible by the color scale (that is cut off at 1, or 100%)?

26. (i) We improved the visibility of TGs and added more TGs in Figure 1b.



(ii) The numbers at the bottom in the time series plots represent the total standard deviation of each time series.

(iii) the intrinsic variability computed using the HR RYF simulation represents an estimate. Therefore, the intrinsic fraction is larger than 1 over very limited portions of the domain due to several reasons (e.g., errors, interaction between forced and intrinsic variability).

Fig1. What about having a look at the differences in the power spectrum between the two experiments. At what frequencies is the variance on the shelf most strongly reduced?

27. We computed the power spectra of total and intrinsic sea level variability in Charleston. However, no significant differences were found.

L75: I assume that 'Total sea level standard deviation (mean across the FOSI members; Fig. 1a)' is the temporal STD of the ensemble mean, correct?

28. No. To calculate the total sea level standard deviation, we first computed the temporal standard deviation in each ensemble member, and then we computed the average of the four standard deviations.

L81: It may be helpful to quantify this agreement (correlation, rms), even if that's from Little et al., 2024.

29. Thanks for this suggestion. We computed the coefficient of determination (R^2) and root mean square error (RMSE) for each tide gauge (Table 1).

Tide gauge	*R^2*	*RMSE (cm)*
Portland (Maine)	*0.81, 0.80, 0.78, 0.8*	*2.70, 2.73, 2.88, 2.79*
New Port	*0.80, 0.78, 0.78, 0.79*	*2.52, 2.63, 2.65, 2.59*
Cape May	*0.86, 0.86, 0.85, 0.86*	*2.80, 2.85, 2.90, 2.85*
Sewells Point, Hampton Roads	*0.85, 0.85, 0.85, 0.85*	*3.51, 3.57, 3.53, 3.54*
Beaufort (North Carolina)	*0.70, 0.69, 0.73, 0.71*	*4.01, 4.11, 3.93, 3.99*
Wilmington	*0.49, 0.51, 0.54, 0.52*	*6.77, 6.72, 6.62, 6.66*
Charleston	*0.66, 0.71, 0.73, 0.66*	*5.19, 4.85, 4.79, 5.22*
Fort Pulaski	*0.64, 0.68, 0.71, 0.63*	*5.62, 5.27, 5.32, 5.75*
Naples	*0.41, 0.43, 0.51, 0.43*	*4.10, 3.92, 3.73, 3.94*
St. Petersburg	*0.48, 0.46, 0.53, 0.44*	*3.85, 3.79, 3.60, 3.93*
Apalachicola	*0.57, 0.54, 0.58, 0.52*	*4.80, 4.90, 4.73, 5.02*
Grand Isle	*0.67, 0.67, 0.72, 0.66*	*3.99, 3.95, 3.68, 4.00*
Galveston	*0.76, 0.78, 0.81, 0.75*	*4.76, 4.59, 4.37, 4.80*
Rockport	*0.73, 0.75, 0.77, 0.73*	*4.42, 4.37, 3.98, 4.65*
Port Isabel (Texas)	*0.7, 0.72, 0.75, 0.71*	*4.21, 4.05, 3.82, 4.15*

Table 1. Comparison between HR FOSI simulation and TG observations. For each ensemble member, we reported coefficient of determination (R^2) and root mean square error (RMSE).

L89-90. I think it would be helpful to include some interpretation of the patterns observed in the intrinsic variability from an ocean dynamics perspective. For example, what causes intrinsic variability to be smaller or larger in certain regions?

30. Great suggestion. In the Results section (lines 95-98):

"Along the GS path, we found a minimum in the offshore intrinsic fraction (Fig. 1g), which coincides with a region of low intrinsic standard deviation (Fig. 1f). The core of the GS is characterized by weaker zonal SSH gradients with respect to its margins (see SSH contours in Fig. 3). These spatial variations in SSH gradients may be responsible for the patterns observed in offshore intrinsic variability near the GS."

In the discussion section (lines 159-161), we also explained why intrinsic variability is smaller north of Cape Hatteras and larger south of Cape Hatteras over the shelf:

"The absence of intrinsic variability to the north of Cape Hatteras is consistent with the limited ability of eddies to influence sea level where the shelf is wide (e.g., Gangopadhyay et al., 2020), and the equatorward propagation of coastal sea level anomalies originating near the GS detachment."

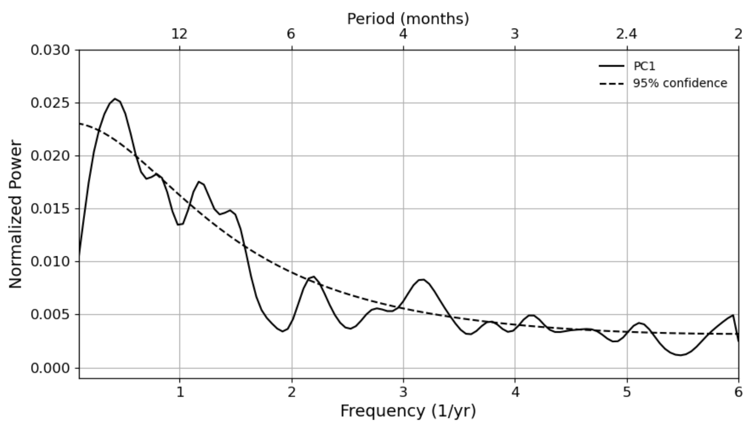
L96: It may be nice for the reader if you'd better explain why you apply the EOF analysis overall.

31. We added the following line in the Results section (line 104):

"We used EOF analysis to identify the modes that explain the largest fraction of variability along the SEUS coastline"

L105: 'Specifically, the PCs exhibit fluctuations at sub-annual to interannual time scales, including significant multi-year sea level trends (for example PC1, over a 5-year period beginning around month 90) (Penduff et al., 2019).' Is the significance based on visual inspection? A spectral analysis incl. significance test could be useful here.

32. Thanks for catching that. Yes, this description is based on visual inspection. Below, we computed the power density spectrum of the PC1:



Power density spectrum of the PC1 (continuous line). The dashed line represents the red noise spectrum with a confidence level of 0.95.

However, spectral analysis is not sufficient to evaluate the statistical significance of sea level trends. We rephrased this part as follows:

"Specifically, the PCs exhibit fluctuations at sub-annual to interannual time scales, including multi-year sea level trends (for example PC1, over a 5-year period beginning around month 90) (Penduff et al., 2019)."

Section 2.2.: The intrinsic variability north of Cape Hatteras appears to be much weaker compared to south of it, causing the EOF's to mostly pickup regions in the south. It's probably difficult to answer, but do you have any idea why the intrinsic variability North of Cape Hatteras is weaker (from an ocean dynamics perspective)?

33. See answer 30.

L120. What is the 0.3 threshold based on? Did you take into account autocorrelation in the time series?

34. This choice was subjective. However, coherence analysis is not sensitive to the precise threshold employed to identify the detachment region. We rephrased this part as follows (see lines 127-129):

"We highlight correlated off-shelf regions (blue contours in Figs. 3b, c, d) using a threshold of 0.3 (note that coherence analysis performed in the following paragraph is not sensitive to the precise threshold employed to identify the detachment region)."

L123-125: Why do you compare these correlation pattern with the minimum offshore intrinsic fraction?

35. Here, we just wanted to stress the fact that the offshore region in Figure 3 and the minimum in the intrinsic fraction found in Figure 1 do not represent the same area. We removed these lines in the revised paper.

2.3. Considering previous work, what about having a look at offshore steric and coastal SL correlations (maybe also at different frequencies). Or what about adding the steric averages in Fig. 4. computed over the same offshore-region?

36. This is a good suggestion. However, we do not think it will add much to the focus of the paper, and we plan to conduct specific analyses on this point in future works.

Fig 3. If the geographical extent shown in the figure were expanded, would similar lagged correlations emerge in other regions—for example, in the Caribbean? This could be worth exploring, particularly in light of previous findings (e.g., Dangendorf et al., 2024, Supplementary Fig. 6; Calafat et al., 2018).

37. See Figure 3a. We have done this analysis, but no other region was highlighted. This suggests that intrinsic variability at the coast does not share a common "path" with some previously described forced mechanisms (although this is not the case with all previously described mechanisms, see answers 18-20).

Discussion

L157-167 and 180 -189: You also mention that the processes communicating signals to the coast are "likely the same as those regulating the transfer of other sea-level signals, regardless of their forced or intrinsic nature." It would be useful to clearly rule out previously discussed processes (e.g., Dangendorf et al., 2021, Calafat et al., 2018) that are not possible within the framework of a setup that includes only intrinsic variability.

38. See answers 18-19-20.