In this article the authors explore the use of ocean dynamic persistence for seasonal sea level forecasting on the US east coast. They show that ocean dynamic persistence, based on ECCO-based initialized ocean model forecasts forced with climatological atmospheric forcing show substantial improvements in forecasts of monthly-mean sea level anomalies beyond the first lead month over damped persistence or a full-feature coupled seasonal forecast model.

This is an excellent article that presents a novel approach to ocean forecasting that holds potential for addressing poor seasonal forecasting of sea level on the US east coast. The methods and analysis used are robust, and the results will be of wide interest and applicability. I particularly appreciated the concise presentation without unnecessary complicating detail. I have a series of minor comments that should be considered before publication.

We appreciate reviewer #1's comments, which greatly helped us improve the manuscript. Please see our point-to-point response below in blue and our revisions of the manuscript in red. The line numbers refer to those in the original manuscript.

General comments:

- Can the authors please clarify whether the monthly climatological forcing used in the ECCO dynamical persistence forecasts is based on flux-forcing (e.g. specified wind stresses and heat fluxes) or bulk-forcing (fluxes computed from specified atmospheric state; wind speed, air temperature etc.)? Frederikse et al. use flux-forcing. I presume flux-forcing is used, as bulk forcing based on monthly-averaged atmospheric state will contain significant biases due to non-linearities in the bulk formula (e.g. quadratic wind stress dependence on wind speed). This question is also relevant to whether ocean-sourced dynamical anomalies are damped by the atmospheric forcing (e.g. the statement at line 306). If bulk forcing is used then, for example, ocean SST anomalies will be overly damped toward the climatology, which would then also impact sea level anomalies. On the other hand, with flux forcing there is no damping of ocean anomalies whatsoever, which is also somewhat unrealistic.

The ECCO dynamic persistence forecast is based on a flux-forced ocean model. In this configuration, surface fluxes are calculated externally using atmospheric state variables and prescribed during the model integration. We clarified the model configuration in lines 110-111 as follows:

"The atmospheric state variables from the ECCO optimization are used to force the ocean model, with surface fluxes of heat, momentum (wind stress), and freshwater being computed externally."

Under flux forcing, initial sea level anomalies can be damped in two ways. First, open ocean sea level signals forced by atmospheric variability significantly influence coastal sea level anomalies in the study region. When external monthly to interannual atmosphere variability is removed, these forced sea level signals lose their energy source, and the amplitude tends to decay over time and distance due to friction and mixing. Second, the use of climatological atmospheric forcing tends to adjust the ocean towards a steady state, further smoothing out the anomalies. We revised the statement in lines 305-307:

"However, the dynamic-persistence forecast only captures seasonally-predicable forcings on the ocean, thereby simulating how oceanic initial conditions evolve according to a climatological atmosphere. As a result, sea level variability predicted by dynamic persistence tends to be much weaker compared to what is observed in the real ocean forced by actual atmospheric conditions. The climatological atmospheric forcing used in the dynamic-persistence forecast tends to restore the ocean toward a seasonally steady state, while friction and mixing gradually dissipate existing sea level anomalies, resulting in reduced sea level variability over time (Sérazin et al., 2014)."

- The authors emphasize that the dynamical persistence forecasts suffer from weak variability, and that if this could be fixed it would improve their skill. To me, this is not an issue with the forecast itself, but a property of the system. The weak variance of the dynamical persistence forecast suggests that the seasonally-predictable signal is a small fraction of the total signal. Unlike an ensemble prediction from a full-feature seasonal forecast model, which includes an ensemble spread as well as an ensemble mean, the ocean-dynamic persistence forecast is designed to only capture the seasonally-predictable signal. Thus this feature could be considered an advantage, not a drawback. I would suggest some reframing of the text to make this point clear (e.g. lines 276-278, 282, 337). This point is also relevant to how the forecasts can be utilized in high-tide flooding outlooks (e.g. lines 336-338). It seems clear that when including predictions for monthly-mean sea level anomalies in these high-tide flooding outlooks, the stochastic component of the monthly-mean sea level still needs to be retained for the purposes of computing, for example, probabilities of threshold exceedance.

We agree that a dynamic persistence forecast captures only the seasonally-predictable (i.e., climatological) forcing on the ocean. Accordingly, the skill of the dynamic-persistence forecast features the minimum skill that a comprehensive coupled forecast system should achieve, compared to damped persistence. The question of why some coupled-forecasting systems perform worse than ocean dynamic persistence would be interesting to investigate in a follow-on study. One possible explanation is that ensemble sizes may be too small to capture the spread of unpredictable atmospheric forcings on the ocean, however, this hypothesis remains to be tested. Regardless, the amount of predictable signal captured by ocean dynamic persistence depends on the configuration of the forecast system, particularly the initialization and realistic representation of the ocean dynamic processes. We will leave it to the reader to judge whether such characteristics are a strength or weakness of the dynamic-persistence forecast, since the reviewer is correct that this framework excludes the stochastic component of atmospheric forcing, which contributes substantially to monthly sea level variability.

We retained lines 276-278 in the results section and replaced lines 281-283 with the following statement:

"Potential causes of the weak variability noticed in dynamic-persistence forecasts are discussed in Section 4, along with a mention about how this may concern the usability of such predictions."

Following the reviewer's suggestion, we also revised lines 305-307 as follows:

"However, the dynamic-persistence forecast only captures seasonally-predicable forcings on the ocean, thereby simulating how oceanic initial conditions evolve according to a climatological atmosphere. As a result, sea level variability predicted by dynamic persistence tends to be much weaker compared to what is observed in the real ocean forced by actual atmospheric conditions. The climatological atmospheric forcing used in the dynamic-persistence forecast tends to restore the ocean toward a seasonally steady state, while friction and mixing gradually dissipate existing sea level anomalies, resulting in reduced sea level variability over time (Sérazin et al., 2014)."

Specific comments:

- Lines 33-34; check the grammar. "yet to be" -> "been".

We corrected the sentence as suggested.

- Line 41; "understanding" -> "forecast"?

We corrected the sentence as suggested.

- Line 45; suggest adding "seasonal" or "monthly" in front of sea level anomalies.

Done. The sentence now reads *"…because the statistical persistence of monthly sea level anomalies dampens to near zero by the lead-4 month…"*

- Line 58; "seems reasonable to expect the possibility of achieving" - this is somewhat convoluted. Suggest a rewrite.

We revised the sentence to *"…it may be possible to achieve more skillful seasonal sea level forecasts for the East Coast."*

- Line 60: For non-US readers, can you clarify what is meant by "Southeast coast"? I presume this is mainly referring to the east coast of Florida, not in the Gulf?

We revised line 60 to read: *"it may be possible to achieve more skillful seasonal sea level forecasts for the East Coast"*. Additionally, we added a new figure in Section 2 to clarify the geographic definition of the "Northeast Coast", "Southeast Coast", and the "Gulf Coast".

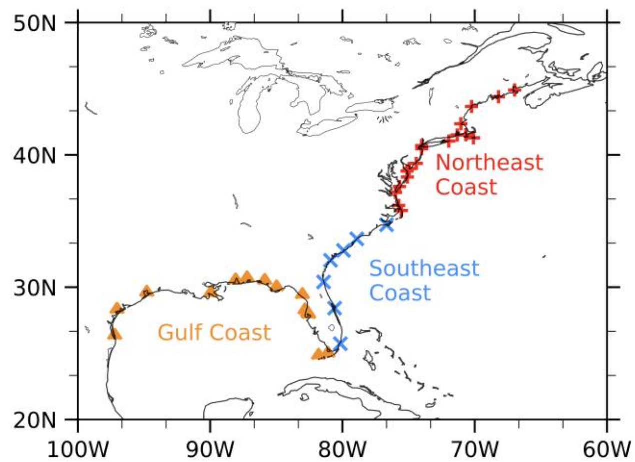

Figure 1. Study domain and water level gauge locations. The Northeast Coast extends from Eastport, ME (8410140) to Oregon Inlet Marina, NC (8652587); the Southeast Coast extends from Beaufort, NC (8656483) to Virginia Key, FL (8723214); and the Gulf Coast extends from Vaca Key, FL (8723970) to Port Isabel, TX (8779770). The 7-digit numbers following the location names denote the NOAA station IDs of the water level gauges.

- Line 61; "likely to continue to" -> again, suggest a rewrite, their influence is not trending.

We revised this to read: "…are likely to complicate efforts to skillfully forecast the sea level variability…"

- Lines 67-74; it may be worth pointing out that Frederikse et al.'s approach is distinct from the approach taken here. They use ECCO adjoint sensitivities, a linear-combinations-of-forcings etc. and thus their approach is not ocean dynamical persistence in the sense examined in this paper. Otherwise, readers may be confused that this paper is just a generalization of that study to the whole East and Gulf coasts.

In lines 67-68, we mentioned that Frederikse's work used the joint sensitivity analysis. To highlight the different approach, inline 77, we add additional explanations: "*Different from Frederikse et al. (2022), where the sea level forecasts rely on a pre-computed sea level sensitivity to atmospheric forcing to represent ocean-dynamical responses,* our investigation utilizes a set of retrospective forecasts produced with an initialized version of the ECCO model that runs forward for 12 months under climatological atmospheric conditions.”

- Lines 86-90: Please mention whether or not detiding is performed on the tide gauge data prior to the computation of monthly means (it shouldn't make much difference).

In lines 90-91, we clarified the processes to remove tide and the generation of the daily time series. *“The hourly time series are averaged to daily data after removing the tide components with the Unified Tidal Analysis and Prediction functions in MATLAB (UTide).”*

- Lines 161-167: It seems to me that this is most likely associated with the low-resolution of the ECCO model (and SEAS5). I would expect that a well-initialized (altimetry assimilating) high-resolution eddy-resolving ocean model making a forecast in dynamical-persistence mode would be able to beat simple damped persistence in the first month in strongly ocean-internal driven dynamics eddying regions. This would be worth mentioning here (i.e. it's specifically *this* ocean dynamical persistence forecast that doesn't perform well here, not all ocean dynamical persistence forecasts).

We agree with the reviewer that a dynamic persistence forecast using an eddy-resolving ocean model with more realistic initialization is presumably to outperform damped persistence in the first month. Lines 161-167 are intended to describe results supported by our current analysis and figures. We discussed the potential influence of model resolution in lines 307-309 of the Discussion section.

- Lines 183: This statement is a bit misleading, since it is difficult to see what is happening with RMSE on the coast given the weak variability and colorbar choice. There is no obvious decay in RMSE with lead time in these plots. Perhaps remove the statement? The same thing goes for the reference to Fig. 2 at line 191.

This paragraph aimed to describe the skill of the damped persistence, as evaluated by both ACC and RMSE. As the reviewer noted, there is no obvious decay in RMSE at lead-4, which is one important feature of the damped persistence and other models. Since this is the first time we discussed the RMSE distribution at the lead-4 and its evolution with lead time, we think it was important to retain this discussion rather than omit it. Including the RMSE evaluation will better explain our conclusion that the decline in forecast skill with lead time mostly refers to ACC, not RMSE.

Following the reviewer’s suggestion, we revised line 183 to: *“RMSE is also low in the coastal regions and the changes from lead-1 to lead-4 month are small”* and we removed the statement in line 191.

- Lines 220-223: There is a positive ACC difference in Fig. 4c for a few sites around Delaware, although the differences are not significant. So I'm not sure these statements are completely true.

We revised lines 220-222 to *"At the lead-4 month, there is no wide-spread evidence that the climate forecast performs better than the other models along the Northeast Coast, nor along the Gulf and Southeast Coasts (i.e., its ACC and RMSE values are similar or worse than damped persistence at almost all coastal locations considered here, although the climate forecast has slightly higher ACC at three locations in the Chesapeake Bay area to the north of Cape Hatteras."*

- Line 306-307: As mentioned above, I'm not sure I agree that the dissipation of these anomalies is due to the climatological forcing. Initial ocean perturbations will dissipate over time due to internal ocean dynamics (planetary wave generation and propagation, friction etc.). The reason that the variability is much weaker is just because you have removed a strong source of variability (the weather/non-climatological atmospheric forcing).

Please see our reply to the general comments above. We have revised and clarified the statement as below: *"However, the dynamic-persistence forecast only captures seasonally-predicable forcings on the ocean, thereby simulating how oceanic initial conditions evolve according to a climatological atmosphere. As a result, sea level variability predicted by dynamic persistence tends to be much weaker compared to what is observed in the real ocean forced by actual atmospheric conditions. The climatological atmospheric forcing used in the dynamic-persistence forecast tends to restore the ocean toward a seasonally steady state, while friction and mixing gradually dissipate existing sea level anomalies, resulting in reduced sea level variability over time (Sérazin et al., 2014)."*

- Lines 318-319: Zhu et al. 2024 (cited in the introduction) computed the contribution of remote forcings via wave propagation to sea level anomalies along the east coast, including at seasonal time-scales (e.g. see their Fig. 3). Thus, a more definite statement could likely be made here. Comparison to their results could yield some insights.

We revised line 317-319 to: *"Yet even at The Battery location on the Northeast Coast, dynamic persistence exhibits clearly higher ACC values after the lead-4 month compared to the other models (Figure 7a), which could be due to the propagation of remote signals improving the skill of dynamic persistence over time (at least compared to damped persistence)."*

- Lines 324-326: I wonder whether the relatively low number of ensemble members in the SEAS5 hindcast considered here means that stochastic, non-predictable processes still have a significant impact on the SEAS5 ensemble mean? See general comment #2 above.

Yes, the ensemble size might be too small to capture the atmosphere variability at the seasonal timescale, particularly for the U.S. East Coast wintertime.