The manuscript presents results from downscaling four projections under SSP scenarios computed by the 1 degree GFDL earth system model to a 1/12 degree MOM6 regional model of the western North Atlantic. The authors find significant scenario dependence for SST and SSS but much less dependence for currents and sea level change. They attribute the lower sensitivity to a delayed response. Current and sea level changes are connected with the AMOC decline simulated by the parent 1 degree GFDL model. The presented findings are consistent with Saba et al. (2026) but the present study offers results for different realistic scenarios.

The inter comparison between low and high-resolution model results of the historical mean state is quite extensive given that NAW12 is not a new configuration and has been evaluated before by Ross et al. (2023/24). One could easily save some figures particularly since most of the outcome is expected, well documented progress when moving from 1 degree to 1/12 degree. Instead, the dependence of the changes on the resolution should be more the focus of the paper, to discuss in what way enhanced resolution leads to different projections. Therefore I suggest to move the evaluation into the supplement and the results from global climate model out of the supplement. In that process the discussion of the biases can be condensed and the differences of the projections enhanced.

We appreciate the reviewer for insightful and constructive comments. Here, we provide point-by-point responses to address each of the reviewer's comments and suggestions. Our replies are provided below in blue font for each of the general and specific comments. We believe that the revised manuscript is now better organized and improved.

1. Moving "3.1 Model validation for the historical period" to supplementary

As the reviewer pointed out, Ross et al. (2023) have already described the performance of MOM6-NWA12 compared to various reanalyses and observations. However, the results from Ross et al. (2023) are based on hindcast simulations driven by atmospheric reanalysis and GLORYS as surface forcing and boundary conditions during the historical period. On the other hand, our NWA12 were forced by bias-corrected atmospheric and oceanic outputs from GFDL-ESM4. Therefore, we need to confirm that NWA12, when forced by bias-corrected GFDL-ESM4 outputs, realistically captures the time-mean ocean states and circulations, which is a crucial necessary step in this study. In addition, Ross et al. (2023) did not validate the transports of the Western Boundary Current (WBC) system (i.e., the Yucatan Current, Florida Current, Antilles Current, and Deep Western Boundary Current), which are key components modulating sea level along the US South and East Coasts. Therefore, we would like to keep Section 3.1 in the main text. To address the reviewer's suggestion to consolidate figures, we combined Figs. 1 and 2 (i.e., the mean from GLORYS and biases from ESM4 and NWA12) and moved the original Fig. 2 (i.e., the mean from ESM4 and NWA12) to the supplementary material.

2. Adding the GFDL-ESM results to the main figures.

We agree with the reviewer on this comment. The addition of the future projection results derived from the parent model (i.e., GFDL-ESM4) is important for discussing the differences between the parent model and the child model (i.e., MOM6-NWA12) and improves the manuscript. Therefore, we moved Figures S4, S5, and S6 from the supplement to the main manuscript as Figures 7, 8, and 12, respectively.

Details

Line 44-45 Are these implication part of the investigations of the manuscript? I did not find much about it that would deserve being mentioned in the abstract.

We removed this part in the revised manuscript.

Line 148-150 I assume ESM4 is run under the same 4 scenarios as NWA12. Where are those runs described and how does ESM4 compare to other climate models in climate sensitivity. A littel bit more information should be provided here.

Meehl et al. (2020) noted that GFDL-ESM4 has a relatively low equilibrium climate sensitivity (ECS, 2.6 K) compared to the multi-model mean (3.7 K) of 68 participating CMIP6 models. This result is consistent with previous studies (e.g., Dunne et al., 2020; Sentman et al., 2026). We addressed this in the revised manuscript.

(L162-164) "It is noted that the equilibrium climate sensitivity (ECS) of GFDL-ESM4 is approximately 2.6 K, which is at the lower end of the sensitivity range for CMIP6 models (Dunne et al., 2020; Meehl et al., 2020; Sentman et al., 2026)."

Line 196-200 Not clear what the difference between the method here and the previous method is. For instance Liu et al. 2012 also corrected with the difference between the climatologies. Alexander et al. however used the differences between the mean. I don't understand how this would assume that the climate variability remains the same as in the historical period. Correcting with mean or climatology does not constrain the climate variability.

As the reviewer pointed out, our methods are fundamentally similar to those previous studies (Liu et al., 2012, Alexander et al., 2020, and Shin and Alexander, 2020). They all applied a climatological mean bias correction. However, Alexander et al. (2020) used interpolated daily values from the monthly outputs of a control simulation, which was forced by reanalysis during the historical period. Liu et al., (2012) used daily atmospheric forcings extracted from a reanalysis product and then added them to monthly CMIP surface forcing fields. Both methods derived high-frequency atmospheric forcings from the historical period, and then added them to monthly forcing fields to force future projection simulations with an assumption that high-frequency atmospheric variability in the future remains the same as in the observed historical period (i.e., fixed high-frequency atmospheric variability). In contrast, we use bias-corrected 3-hourly and daily atmospheric forcings derived directly from GFDL-ESM4. Therefore, the high-frequency atmospheric variability comes from GFDL-ESM4. This means that the model results in our study consider the future modeled high-frequency atmospheric variability, which is different from the previous studies.

To avoid confusion, we modified the corresponding text in the revised manuscript.

(L207-219) "It is noted that our 'Delta method' shares similarities with approaches from previous studies (Liu et al., 2012; 2015; Alexander et al., 2020; Shin and Alexander, 2020; Pozo-Buil et al., 2021), which replace model climatology with reanalysis climatology to reduce mean biases. However, our method fundamentally differs in its treatment of high-frequency atmospheric forcing. While those previous studies utilized high-frequency atmospheric forcing (i.e., daily time scales) from historical reanalysis datasets for future projections—thereby assuming that high-frequency forcing remains unchanged in the future—we retained the model-generated high-frequency atmospheric variability (e.g., 3-hourly and daily). We took this approach to ensure more consistent climate projections, recognizing that weather and climate are interdependent. Indeed, not only does weather depend strongly on low-frequency variability (e.g., weather conditions during the different phases of ENSO are substantially different), but also weather statistics can substantially change under future climate conditions (e.g., Cheng et al., 2012; Jeong and Sushama, 2019).“

Line 204-206 This argument is not convincing. Jackson and Wood do not really assess runoff uncertainty and Giuntoli argued that hydrological models add uncertainty in addition to the contribution of GCMs. Following just the latter argument you may also disregard the GCM contribution. If there is not meaningful signal and only uncertainty is added, projected runoff could be neglected. But I doubt that there is not meaningful runoff signal, since sea level projections nowadays include contributions from glacial and ice sheet melt and a clear positive trend in river discharge is visible already now (https://doi.org/10.5194/hess-28-2179-2024). So, neglecting the runoff contribution is likely to reduce realism rather than uncertainty.

Thank you for pointing this out. We agree that the Jackson and Wood study was not particularly relevant to our study. So we removed the references in the revised manuscript. Müller et al. (2024), as mentioned by the reviewer, showed that even though global river discharge driven by climate change exhibits a clear positive trend, the projected river discharge changes in our regional model domain (i.e., the southern and eastern US seaboards) are insignificant and uncertain during the first half of the 21st century (Figs. 4 and 6 in Muller et al., 2024). We believe this justifies our decision not to consider future changes in river runoff in our regional ocean model. The potential effects of a wider range of possible runoff changes towards the end of the century is a very important topic to explore but it is beyond the scope of the present study. To address this, we changed the main text in the revised manuscript.

(L178-185) Although global river discharge driven by climate change exhibits a clear positive trend, the projected changes in river discharge in our regional model domain (i.e., the southern and eastern US seaboards) are insignificant and uncertain during the first half of the 21st century (Muller et al., 2024). Therefore, we did not consider future changes in runoff in this single-model downscaling and instead applied the daily mean climatology (1993–2020) of GloFAS river runoff data for the entire simulation period (1950–2100). As a result, the potential effects of regional runoff change on nearshore salinity and sea level are not addressed in this study.

Line 216-223 You may want to mention other components that are not included in the sea level projections, particularly since you refer to this information in the conclusions.

The MOM6 cannot simulate the basin-averaged sea level changes since this model utilizes a Boussinesq approximation. Therefore, we addressed this limitation in GFDL-ESM4.1 and MOM6-NWA12 in the method section of the revised manuscript.

(L232-242)) Finally, for sea level, we note that both GFDL-ESM4.1 and MOM6-NWA12 utilize the Boussinesq approximation, which conserves ocean volume. The dynamic sea level in both models can respond to local density changes driven by local warming and freshening (e.g., Steinberg et al., 2024). However, these models cannot simulate global mean sea-level (GMSL) rise caused by thermosteric expansion or added mass from ice melt (e.g., Greatbatch, 1994; Griffies and Greatbatch, 2012; Griffies et al., 2014). Furthermore, to prevent potential drifts in the basin-integrated water volume associated with the lateral open boundary conditions, we explicitly constrain the basin-averaged SSH anomaly to be zero throughout all MOM6-NWA12 simulations. Consequently, the SSH changes derived from MOM6-NWA12 strictly represent the dynamic redistribution of water mass driven by regional ocean circulation and local steric adjustments.

Line 241-242 I think it is mostly broader, I am not sure if I see the shift away from the coast.

We agree that the Gulf Stream (GS) in GFDL-ESM4.1 is broader than in GLORYS12. However, Supplementary Fig. S1 shows the historical mean (1993–2020) position of the GS, defined by the 15°C isotherm at 200 m depth, in GLORYS12, GFDL-ESM4.1, and MOM6-NWA12. This figure shows that the GS in GFDL-ESM4.1 is shifted slightly further offshore along the South Atlantic Bight compared to its position in GLORYS12. To clarify this, we now explicitly reference Supplementary Fig. S1 in this section of the revised manuscript.

(L267-268) "In addition, ESM4.1's Gulf Stream along the South Atlantic Bight (SAB) is weaker, broader and slightly shifted away from the US East Coast compared to that in GLORYS12 (Supplementary Fig. S2)."

Line 315-319 What are "these regions" over which the temperature change was calculated?

In this context, "these regions" refers to the areas of the maximum SST warming around the MAB and the Gulf of Maine (35°N–42°N, 75°W–60°W). To clarify this, we added the longitude/latitude information to the revised manuscript.

(L340-341) "Warming in these regions around the MAB and the Gulf of Maine (35°N–42°N, 75°W–60°W) is reduced to ~3°C, ~2°C and ~1°C in SSP-370, SSP-245 and SSP-126, respectively (Fig. 5b-d)."

Line 360-363 These two statements seem to contradict each other. Fig.7 is from the end of the century and I believe to see gradual differences. I suggest to point to the gradual differences visible in Fig.7 but then also to the surprisingly similar development until 2070 seen in Fig. 8. Additionaly mention here when the scenario forcings start to differ, because later you use an argument of delayed response that requires the reader to know until when scenarios were still similar.

Thank you for pointing it out. As the reviewer suggested, we revised the sentences to point out these differences at the end of the century (Fig. 6, originally Fig.7) and highlighted the similar changes across all scenarios until around 2070 (Fig. 9, originally Fig. 8).

(L389-393) “These scenario-dependent differences in the reduction in surface current are clearly shown by the late 21st century (Fig. 6). To further explore volume transport by the WBCs system, we examine the temporal changes in the volume transport in the Florida Current, Yucatan Current, Antilles Current, and the Deep Western Boundary Current (DWBC), as shown in Fig. 9.”

(L417-429) “Similarly, the time series of volume transports in the WBCs system shows a similar rate of decline across all four SSP scenarios until approximately 2070 (Fig. 9). The insensitivity of Northwestern Atlantic WBCs to emission scenarios before 2070s is consistent with the AMOC decline in GFDL-ESM4.1, which is the major contributor to the modulation of the Atlantic WBCs system (Fig. 10). Previous studies (e.g., Weijer et al., 2020; Baker et al., 2023) found that the rate of AMOC weakening derived from most CMIP6 models shows limited sensitivity to emission scenarios prior to around 2070, consistent with GFDL-ESM4.1. It is important to note that the greenhouse gas forcings for the CMIP6 SSP scenarios begin to diverge after the historical period (~2014), with separation in their radiative forcing pathways emerging by the mid-21st century. The results that WBC volume transports and AMOC remain relatively insensitive to these diverging emissions scenarios for several decades provides critical evidence for a delayed ocean response to greenhouse gas forcing.”

Line 373-366 Is the reduction really significant given the error bars? Given the error bar it is probably more appropriate to say that the Antilles Current disappears (no significant mean transport).

Agreed. We toned down this in the revised manuscript.

(L405-406) “This suggests that the Antilles Current may disappear (nearly zero mean transport) after around 2080 (Fig. 9c)”

Line 383 Point to Fig. S6 already here

Added.

Line 368-388 Do the lateral boundary conditions make NWA12 entirely a slave of the ESM4.1 for AMOC? You may want say something about this.

As mentioned in the previous reply, we added the following sentence to address this point in the revised manuscript.

(L419-421) “The insensitivity of Northwestern Atlantic WBCs to emission scenarios before 2070s is consistent with the AMOC decline in GFDL-ESM4.1, which is the major contributor to the modulation of the Atlantic WBCs system (Fig. 10).”

Line 398-399 not shown?

This refers to Fig. 11 (originally Fig. 9).

Line 405-406 How much is AMOC vs gyre weakening? Although ultimately as Yin et al. (2010) suggested the AMOC may be the origin of the changes, it would be useful to mention changes in the barotropic circulation of the subtropical gyre (stream function). because it seems that the transport change directly associated with AMOC weakening can account only for a fraction (<50% from the numbers reported on page 18 and the AMOC decline Fig.S6) of the reduced transports.

The rate of WBC volume transport reduction in MOM6-NWA12 cannot be directly compared with the rate of AMOC weakening in GFDL-ESM4.1 because they are not two-way coupled. Additionally, due to the Orlanski's radiation open boundary conditions (Orlanski, 1976) employed in MOM6-NWA12, the volume transports across the open boundaries do not exactly match between the two models. Therefore, it is quite difficult for us to reconcile the aforementioned difference between the two models.

Similarly, since MOM6-NWA12 is a regional model with open boundaries, the stream function values in the eastern open boundary, which are required to integrate the barotropic volume transport, cannot be determined. Therefore, we are unable to compute the barotropic streamfunction for the MOM6-NWA12 domain. This limitation is briefly discussed in the revised manuscript, pointing out that the gyre circulation could potentially contribute to the changes in the WBCs volume transport.

(L442-453) "It is noted that the gyre circulation change potentially contributes to the changes in WBC volume transport. However, since MOM6-NWA12 is a regional model with open boundaries, we are unable to explore the barotropic streamfunction for the regional domain."

Line 454-455 How does geostrophic adjustment weakens upwelling. Isn't it more just the flattening of the isopynals.

This sentence is now revised.

(L517-519) A distinct decrease in density (i.e., lighter water) emerges on the western side of the Florida Current around 200 m depth (Supplementary Fig. S7). This localized density reduction reflects a relaxation, or flattening, of the upward-tilted isopycnals along the Florida coast. Consequently, this flattening of the isopycnals weakens the cross-stream horizontal density gradient, thereby reducing northward volume transport in the Florida Straits. Due to reduced bottom Ekman transport and a relaxation, or a flattening, of the upward-tilted isopycnals associated with a weakened Gulf Stream, upwelling decreases along the continental slope and shelf, limiting the supply of cold and relatively fresh subsurface water from underneath the Gulf Stream.

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