Response to Reviewer 2:

We sincerely thank Reviewer 2 for their time and effort in evaluating the manuscript. These constructive comments and suggestions are greatly appreciated and have been invaluable in improving the clarity, rigor, and overall quality of the work. We have carefully addressed each point raised and revised the manuscript accordingly, as detailed below. Reviewer comments are presented in blue italics, and my responses are shown in black.

The manuscript addresses a relevant and timely question regarding the role of the ocean salinity as water cycle indicator and as a driver of ocean circulation depending on the spatial and temporal scales. The manuscript provides some important key messages for enhancing model outcomes (the need of accurately modelling salinity dynamics) and future observation systems (why is it important to go towards 10km resolution in future Earth Observation systems). In my opinion the manuscript deserves its publication after some minor changes.

Thank you for the positive feedback. We are glad the key messages of the manuscript came through clearly.

Figure 2: *You could include the Chinese L-band mission COSM, which was launched in November 2024. Data is still not freely available, as far as this reviewer knows.*

Thank you for the suggestion. COSM has been added to Figure 2 and incorporated into related text throughout the manuscript.

Figure 3: *It is not clear which dataset is used for this figure. Could the authors include this information?. In addition, a paragraph explaining why the Arctic Ocean and the Nordic Seas are excluded from the study would be very helpful.*

As suggested, the data source used in Figure 3 has been added to the figure caption for clarity.

We also agree that the exclusion of the Arctic Ocean and the Nordic Seas should be explained more explicitly. This limitation arises from the spatial coverage of the Argo-based mixed layer depth product used in this study (Roemmich and Gilson, 2009), which is generally restricted to 60°S–65°N and provides only sparse sampling in the Nordic Seas and Arctic Ocean. Because mixed layer depth, h, is a key variable in the calculation of freshwater forcing, FWF = $S_0(E–P)/h$, our analysis is necessarily limited to regions where Argo observations provide sufficient coverage to estimate h reliably.

The following sentences are added to Section 2 (lines 164-168):

"It is also worth noting that Argo sampling is generally limited to 60°S–65°N, with sparse coverage in the Nordic Seas and Arctic Ocean. Because the mixed layer depth h derived from Argo is a key variable in computing the freshwater forcing term FWF = $S_0(E–P)/h$ (see Eq.(1)), the analyses presented in Section 3 are necessarily confined to regions where Argo provides sufficient coverage to estimate h reliably; the Arctic Ocean and Nordic Seas are therefore excluded.

L185-190: *When referring to "semi-enclosed seas," it would be useful to clearly specify which regions are included (e.g., Black Sea, Hudson Bay, Chinese seas, Mediterranean Sea). Visually, a strong correlation is not evident in some of these regions, particularly in the eastern Mediterranean. Perhaps a zoomed-in panel focusing on the semi-enclosed seas could be included to better support the discussion.*

We agree that the term "semi-enclosed basins" was too broad in this context. To make the discussion more precise, we have replaced it with the specific regions intended here: the Mediterranean Sea and the Baltic Sea. The revised sentence now reads:

"The Mediterranean Sea and Baltic Sea provide particularly clear evidence where geometric constraints can enhance forcing signals. In the Mediterranean, salinification (0.10-0.15 psu) has been linked to 10-15% evaporation increases (Skliris et al., 2018), while in the Baltic, freshening (0.10-0.20 psu) is consistent with increased precipitation and runoff (Meier et al., 2006; Lehmann et al., 2022)."

Another more general comment on this section is, that perhaps it would be nice to include in Figure 3, the correlation with the advective term (correlation of tendency minus advective term with surface forcing), in order to better understand why surface forcing does not correlate well with salinity tendencies alone and which are the places where diffusivity and entrainment have a big role.

This is a thoughtful suggestion. We agree that showing the correlation between ($\partial S/\partial t -$ advection) and E–P in Figure 3 could help clarify why surface forcing alone does not correlate strongly with salinity tendency, and identify regions where entrainment and diffusive processes are important. However, this analysis is difficult to interpret robustly when based on a monthly observationally-based mixed-layer salinity budget. The advective term derived from monthly-mean velocity fields does not include eddy contributions, and submonthly processes such as instantaneous entrainment are not resolved. Moreover, diffusive mixing and entrainment cannot be directly quantified with sufficient accuracy from available observational products, so the residual ($\partial S/\partial t -$ advection) would contain not only the signature of surface forcing but also substantial contributions from unresolved oceanic processes. Its correlation with E–P would therefore be subject to considerable uncertainty and would not provide a clean separation of the roles of surface forcing, advection, diffusivity, and entrainment. We nevertheless agree this is a valuable diagnostic and regard it as an important direction for future work using high-resolution ocean model output, where the mixed-layer salinity budget can be more fully closed and contributions from eddies, entrainment, and mixing can be more realistically represented.

L205-210*: Correlation close to the Gulf stream and Kuroshio seem to be large at seasonal scales (Figure 3a). Perhaps the red in the map is not statistically significant. It would be nice to distinguish the color among statistically significant (positive and negative) and not.*

We note that in the original submitted figure, regions with $p < 0.1$ were outlined by thin black contours, though this was inadvertently omitted from the figure caption. Following the reviewer's suggestion, panels (a) and (b) have been revised with stippling added to regions where correlations are statistically significant ($p < 0.1$), making the distinction between significant and non-significant correlations more visually clear. The color scheme has also been updated to improve visibility of the stippling and to better match the colors used in panel (c). The figure caption has been updated accordingly.

***L245-250:** The following sentence is difficult to understand (detection of SSS requires three ensemble members?) and should be rephrased for clarity: "Despite these complications, salinity exhibits higher signal-to-noise ratios than atmospheric variables, as detection requires fewer than three ensemble members for SSS versus substantially more for precipitation or E–P (Terray et al., 2012), because salinity integrates high-variance atmospheric forcing over time while spreading anomalies spatially."*

I suggest simplifying this sentence or splitting it into two or more shorter sentences.

Thank you for pointing this out. The sentence has been revised and now reads as follows:

"Despite these complications, salinity exhibits higher signal-to-noise ratios than atmospheric variables such as precipitation or E–P, because salinity integrates high-variance atmospheric forcing over time while spreading anomalies spatially. As a result, detecting anthropogenic influence in SSS requires fewer than three ensemble members for SSS versus substantially more for precipitation or E-P (Terray et al., 2012)."

***L250-255:** Sentence: "Natural decadal variability operates on timescales comparable to 10–30 year analysis periods" would be better placed at the beginning of the section, as it helps frame the discussion that follows.*

The sentence has been placed at the beginning of the section.

***L295–300**: Could you include a reference associated with this statement: "These changes exceed 2σ natural variability (>95% confidence), showing human influence on ocean salinity patterns since 1960"?*

References have been added. The sentence now reads as follows:

"These changes exceed 2σ natural variability (>95% confidence) (Zika et al. 2018), showing human influence on ocean salinity patterns since 1960 (Pierce et al., 2012; Terray et al., 2012)."

***L330–335**: In the sentence*
"Second, τvmix versus τadv controls whether subsurface waters preserve formation-era memory or respond to contemporary forcing,"
Should τadv be replaced by τf?

We thank the reviewer for this comment. However, τ_f and τ_{adv} are two distinct timescales measuring different physical processes:

- τ_f measures the persistence of freshwater forcing
- $\tau_{adv} = L/U$ measures how fast currents redistribute anomalies.

Therefore $\tau_{adv} = L/U$ is retained as originally written.

Table 1
Could the variables L and U be explained more clearly? It would also be helpful to provide a reference for each of the typical ranges listed.

This is a helpful suggestion. Table 1 has been revised to include (1) clearer definitions of the scaling variables L, U, K_h, and K_v, and (2) references for each of the typical ranges listed.

Figure 5
The datasets used should be described in more detail, particularly for panels a, b, c, and d. For panels e and f, I assume they are derived from SMOS data, but this should be explicitly stated. Additionally, are the temporal derivatives computed from monthly data and then subjected to harmonic analysis? Could you better explain how the method has been applied?

As suggested, the datasets used in Figure 5 have been added to the figure caption, along with a brief description clarifying that the salinity tendency $\partial S/\partial t$ is computed as the temporal derivative of monthly-mean SSS, and harmonic analysis is then applied to all monthly-mean fields.

L430: *In the expression L^2/Kh, what does L represent?*

Definition of *L* has been added. The sentence now reads as follows:

"Pathway coherence depends on τ_f/τ_{hmix}, where $\tau_{hmix} = L^2/K_h$, with L being the spatial scale of the plume (~1500 km for the Amazon plume) and K_h the horizontal eddy diffusivity, determines survival against lateral stirring."

L450: *It is stated that the maximum negative trends in the Pacific are located at the surface (Fig. 6a) and are weaker than the positive trends at 200 m, but this is not clearly evident from the figure. Similarly, in the Atlantic (30–10°S), Fig. 6b appears to show that the maximum trend occurs within the upper 100 m.*

We thank the reviewer for this careful reading of Section 4.2.1 and Figure 6. First, regarding the Pacific negative trends, we agree that a clear surface intensification is not evident in Fig. 6a and have revised the text to reflect this more accurately. Second, regarding the Atlantic 30–10°S band, the reviewer is correct that the maximum trend is concentrated in the upper 100 m rather than at 150–250 m as originally stated. We have revised the text accordingly, noting that the subduction signal in the Atlantic manifests as a spatial displacement of the trend maximum poleward and downward rather than as a clear subsurface intensification in depth coordinates. Both revisions bring the text into closer agreement with what Figure 6 actually shows, i.e., subsurface trends reflect subduction along isopycnal pathways rather than local forcing.

L525-530: *A brief definition on the Tuner angle and density ration will facilitate the understanding of the reader.*

Definitions of the Turner angle and density ratio have been added to the text. The paragraph now reads as follows:

"At mesoscales exceeding 100 km, density gradients in the Gulf Stream and open ocean are predominantly temperature-controlled with Turner angle $Tu > \pi/4$ and density ratio $R\rho > 1$, where the density ratio $R\rho = \alpha\Delta T/\beta\Delta S$ and Turner angle $Tu = \arctan(R\rho)$ together quantify the relative contributions of temperature and salinity to density, with ΔT and ΔS being temperature and salinity differences over equal distances of 350 m along the Saildrone track, and α and β being the thermal expansion and haline contraction coefficients, respectively."

***L794:** One reference is missing from the bibliography.*

Good catch. Li et al. 2020 was incorrectly cited as Li et al. 2019. This has been corrected.