Response to Reviewer #1

We sincerely thank Reviewer #1 for their time and careful evaluation of the manuscript. These constructive comments are greatly appreciated and have contributed to improving the clarity, rigor, and overall quality of the work. We have addressed each point below, and the corresponding changes are reflected in the revised manuscript. Reviewer comments are presented in blue italics, and my responses are shown in black.

This is an interesting and timely paper, summarizing both the past work of Lisan Yu at larger scales, and what happens at shorter scales (~10 km) which is a challenge for future satellite missions, as well as for the observations.

Thank you for your positive feedback.

My comments are mostly minor:

Section 3: Interannual time scales. I suggest to start by indicating how interannual is defined here (detrended and deseasonalized is too vague here (it could be both subannual periods or longer periods; also for figure 3, only 10 years are considered: how stable are the statistics presented for Corr Interannual variability 'p stat' are probably not enough…)

The reviewer is correct that "detrended and deseasonalized" is not sufficiently precise. We have added a clarification in both the text in section 3 specifying that interannual variability refers to year-to-year variations on timescales of 1–7 years, obtained by removing the long-term trend and mean seasonal cycle from monthly-mean fields.

On the concern about statistical stability with only 11 years of data: this is a fair point. The short record does limit the degrees of freedom for interannual statistics, and we have noted this explicitly in the revised figure caption. The low correlation threshold of $r > 0.14$ ($p < 0.1$) is a direct consequence of this limitation. We also note that the broad spatial patterns are consistent with results based on longer records (Vinogradova and Ponte, 2017; Yu 2023), which gives us some confidence that the patterns are not an artifact of the short analysis period.

In the river plume discussion (possibly 4.1.3, it could be good to also cite Olivier et al (2023)

Olivier, L., G. Reverdin, J. Boutin, R. Laxenaire, D. Iudicone, S. Pesant, Paulo Calil, J. Horstmann, D. Couet, J. M. Erta, A. Koch-Larrouy A. Bertrand, P. Rousselot, J.-L. Vergely, S. Speich M. Araujo, 2024. Late summer northwestward Amazon plume pathway under the action of the North Brazil Current rings. Rem. Sens Res., 307, 114165, ISSN 0034-4257.

This fits well in the ' tf/tadv ~1 regime (if here one adopts tf on order of month (or months)

The reference has been added to two places in section 4.1.3. Thank you for the suggestion.

This may hint to a sight difficulty of the reading of table 2. Forcing of a river outflow might be of time scales of days, but can also be on the scale of a few weeks, even months (for the Amazone, given as an example later, for example). May be that should be mentioned beforehand. Thus near field will vary in spatial scale depending on the time scale considered. In the table, maybe use for that tf, the symbol to (tau of outflow). This would make the comparison with the previous discussion in the open ocean easier to follow. In table 4, for subsurface tvmix tadv, I consider that a particular type of subsurface signal. In the whole subduction regime, this is not so different (well, depends where, I agree).

I suspect there is an ambiguity exemplified on line 421, with mid-field tf is larger. What is meant is that the response time to plume change is on these time scales, not really the earlier river discharge time scale. The ambiguity in the whole par is that tf defined as discharge, which should have same time scale wherever one is… Same issue on line 427… One needs to redefine tf…

The reviewer raises a valid point that τ_f in Section 4.1.3 comprises two physically distinct timescales. Following the reviewer's suggestion, we have introduced τ_o as a dedicated symbol for the river outflow variability timescale and made the following changes to Section 4.1.3 and Table 2:

- In the opening paragraph, τ_o is defined and it is clarified that for large river systems such as the Amazon, τ_o can range from days (flood events) to months (seasonal discharge cycle), and that the spatial extent of the near-field will vary accordingly. All instances of τ_f/τ_{adv} have been replaced with τ_o/τ_{adv}.
- In the near-field paragraph, it is noted that τ_o ranges from days to weeks depending on the discharge event considered, while τ_{adv} remains set by boundary-current export timescales.
- In the mid-field paragraph, it is explicitly stated that τ_o remains set by discharge variability as before, but τ_{adv} has increased with distance to the point where it becomes comparable to τ_o, making clear that it is τ_{adv} that changes with distance, not τ_o.
- In the far-field paragraph, τ_f/τ_{adv} has been replaced with τ_o/τ_{adv}, and the range of τ_o is noted explicitly.
- In Table 2, the column header τ_f/τ_{adv} has been replaced with τ_o/τ_{adv}, and a footnote has been added clarifying that τ_o is independent of distance from the mouth while τ_{adv} increases with distance.

4.2.1: not so clear that there is subsurface intensification on Fig. 6 (specially in Atlantic Ocean), whereas it is shifted spatially (OK; in particular in southern Pacific). Wondering whether isopycnal representation would help (instead of Z-representation)

We thank the reviewer for this careful examination of Figure 6. The point about the Atlantic is well taken. The signal there is better described as a spatial displacement of the trend maximum poleward and downward from the surface forcing region rather than a clear vertical intensification, which is physically consistent with subduction along sloping isopycnals. We have revised the text accordingly. Regarding the suggestion to use isopycnal coordinates, we agree this would provide a more direct view of the subduction pathways. However, converting the EN4 product to isopycnal coordinates over the 1950–2019 period is not straightforward because the density field itself is evolving due to concurrent warming and salinity changes, and a simple conversion would mix salinity changes along isopycnals with isopycnal heaving. We have noted this as a direction for future work and added a brief comment in the text to this effect.

l. 512: I am not sure that this is what explains the nearly vertical trend structure in tropical regions (what is referred to there?)

The last sentence of Section 4.2.3 could indeed be clearer about what "nearly vertical trend structure" refers to and how it relates to $\tau_{vmix} \sim \tau_{adv}$. We have revised the sentence to clarify that in tropical regions, strong upwelling and enhanced vertical mixing reduce the stratification barrier between surface and subsurface waters, allowing surface freshwater anomalies to be more readily communicated downward. As a result, the tropical trend structure tends to be more vertically uniform rather than showing the clear subsurface maximum characteristic of subtropical subduction. This is distinct from the subtropical regime where $\tau_{vmix} \gg \tau_{adv}$ and subduction preserves surface anomalies at depth along isopycnal pathways.

l. 545: I don't see the transition to salinity-dominated in GS winter at the smaller scales in the figure (it tends toward pi/4)

Thanks for the comment. The text and figure caption have been revised to more accurately reflect what Figure 7 shows. The text now reads:

"Near 10 km Tu approaches or drops below $\pi/4$ and $R\rho$ approaches or falls below 1 in all three regimes, indicating a shift toward equal or salinity-dominated density gradients, though the transition is more pronounced in the open ocean and continental shelf regimes. In the Gulf Stream winter regime, Tu approaches $\pi/4$ but does not clearly cross below it, consistent with a transition toward equal T-S contribution rather than full salinity dominance (Yu 2026)."

The figure 7 caption has been revised accordingly.

l. 551: Below Rd… Horizontal flow cannot maintain geostrophic balance… This is too much a statement.The previous sentence seems to me clearer and does not require this added explanation.

The sentence has been deleted.

L 568 is awkward: 'Solar heating preferentially warms colder patches, because…'. The net feedback is not just solar heating (I guess there it was the longwave fluxes (a net cooling term; maybe use 'radiative cooling'…) that were implied), but in the other terms (sensible and latent heat), and it is not the lower ocean heat content that is in itself the cause of differential heating, but the lower surface temperature… Maybe instead of 'solar heating' use 'solar heat fluxes' (thi is clear later on in the paper)

This is a good point. The net surface heat flux damping of temperature fronts involves radiative, sensible, and latent heat components, and it is the lower SST rather than the lower heat content that drives the differential heating. We have revised the sentence accordingly.

l. 573: 'depends on atmospheric state… and on sea surface temperature, rather than on salinity itself' I would add that this can act as a positive feedback on salinity gradient, in regions where T and S are positively correlated. There, just as the latent heat flux might damp the temperature contrast, it could strengthen the salinity contrast. (one could also mention that on l.582)

In regions where T and S are positively correlated, such as where warm, salty subtropical waters meet cool, fresh subpolar waters, latent heat flux damping of the temperature contrast could indeed act to strengthen the salinity contrast, providing a positive feedback on the salinity gradient. We have added a sentence to this effect. Thanks for the suggestion.

l.578-582: however, if one starts from a 'T-dominated' front, there is a limit to this 'stronger frontogenesis', as when one gets close to pi/4, as salinity gradients increase relative to temperature gradients of these initially 'T-dominated' fronts, the horizontal surface temperature gradients tend to 0 , thus weakening the initial frontogenesis. I guess after what you refer to and describe in this paragraph is the descent towards 'salinity dominated' fronts…

The reviewer raised a subtle but important point. As salinity gradients increase relative to temperature gradients in an initially T-dominated front, the horizontal SST gradient weakens, which in turn reduces the frontogenetic forcing that was driving the initial intensification. We have revised section 4.3.2 accordingly and acknowledged this limit.

l. 584: there is a vertical extent to which the resulting vertical velocities will act as a result of the frontogenesis; It is not because they are locally on order O(10-100m/day) that the particles will be displaced vertically, and ventilate the upper pycnocline (or 'upper' hasto be carefully defined).

The sentences in question have been revised to be more precise. Thanks for the comment.

l. 588-599 seems to be another formulation of what is above. I think that these different paragraphs should be merged in a more coherent way.

We thank the reviewer for pointing this out. The two paragraphs have been merged into a single, more coherent discussion.

612 and 614. I understand the difference of what is meant here, but I believe that 'because forcing persists longer than circulation can export them…' and 'lateral transport is faster than changes in forcing' have many things in common, so one has to be a bit more careful in the separation of time-space to use the two (times and space) in a very clear fashion, which is somewhat ambiguous here.

The reviewer is correct that the two statements were indeed framed differently: one in terms of persistence in time and the other in terms of relative rates, but they were not clearly distinguished from each other. We have revised the text to make the separation between the rain-gauge and passive-tracer limits more explicit by framing both in terms of the τf/τadv ratio, and by clarifying that the passive-tracer limit can arise either from fast lateral transport or from vertical insulation of the interior.

l. 611: even the 'rain-gauge' behavior is worded in strange way. It actually works mostly if advection has much longer time scale, and does not have time to export it; If it is the opposite, advection will carry it out (except if it recirculates in the same region; maybe feedback on circulation)… Actually as is on the table for forcing mechanisms is OK. I assume thus that there is also a spatial scale involved (a gyre for example, and one has to think also of the forcing as not local, but integrated spatially over these circulation patterns (2-D or 3-D)

This is a really good point that we had not made explicit previously. The rain-gauge behavior does involve a spatial dimension, as forcing must be coherent over the circulation domain, not just persistent in time. In a subtropical gyre, water recirculates through the same evaporative region repeatedly, so the effective forcing is the spatially integrated E–P over the gyre rather than the local instantaneous flux. We have added a sentence to this effect, as it actually strengthens the physical basis of the rain-gauge framework and connects naturally to the basin-scale pattern amplification discussed in Section 3.

l. 663: formulation also strange 'Once formed, an anomaly is transformed…'. What is meant here is that feedback is not in the same nature as for temperature, but obviously if in contact with the atmosphere, it is transformed by it! I would argue that in some regions there might be some feedback of temperature anomaly on salinity (a positive T anomaly could for example lead a positive anomaly due to excess evaporation (or vice-versa if it induces excess rainfall, depending on latitude, etc…)). The reverse is a little less obvious, except in the submesoscale context…

The reviewer makes a valid point. Salinity anomalies at the surface are indeed modified by E-P fluxes when in contact with the atmosphere, and indirect T-S feedbacks through evaporation and precipitation exist, particularly in subtropical and tropical regions. We have revised the text to clarify that what distinguishes salinity from temperature is the absence of a direct negative feedback: E and P are not controlled by the salinity state itself rather than a complete absence of atmospheric interaction. We have also added a note on indirect T-S feedbacks as the reviewer suggests.

By line 674, the spatial scale context is also recognized… (which is important)

We agree with the reviewer that the spatial scale dimension is important here and have added a sentence making it more explicit. The distinction between basin-scale and regional-scale behavior is not only about how long forcing persists relative to advection, but also about whether water stays within the same forcing regime during its transit. We think this addition strengthens the connection to the timescale framework developed in Section 4.

L 694: 'mix downward… at the 10 km scale'. Another issue is also the role of these processes in intensifying surface signals (due to its overall influence on stratification)

This is an excellent point that we had not addressed explicitly. The role of submesoscale processes at 10 km is not limited to controlling vertical penetration of freshwater anomalies, ad they also influence near-surface stratification in ways that can intensify surface salinity signals. Submesoscale restratification after mixing events, for example, can trap freshwater near the surface and amplify surface anomalies. We have added a discussion to this effect, as it adds an important dimension to the argument for why O(10 km) resolution matters for satellite SSS observations.