We sincerely thank the Editor and both Reviewers for their careful reading of the manuscript and for the constructive and insightful comments provided. These suggestions have helped us to improve the clarity, structure, and scientific positioning of the study.In the following document, we address all comments point by point. We first respond to Reviewer 1 and subsequently to Reviewer 2.

Response to Reviewer 1: Sebastián Loyola

We thank the reviewer for the careful and constructive evaluation of our manuscript, as well as for the positive assessment of its scientific potential and relevance. We appreciate the detailed comments provided, which helped us to improve the clarity, structure, and scientific positioning of the study. Below we address each point raised.

The manuscript addresses a relevant and timely topic: the year-round characterization of coastal upwelling along the Uruguayan coast. It convincingly proposes the use of sea surface salinity as an alternative proxy to sea surface temperature outside the summer season. The ocean model appears to be well configured, the methodological basis is solid, and the analysis is extensive. Overall, the study has clear potential and could make a valuable contribution to the understanding of coastal upwelling processes in the region.

However, the manuscript presents several issues related to structure, focus, and scientific positioning that currently weaken its overall impact. In many passages, the text reads more like a technical report or a project deliverable than a scientific article oriented toward hypothesis-driven research, critical comparison, and discussion of results in the context of existing literature.

The Introduction section, in particular, is not clearly closed. It is difficult to identify where the introduction ends and where the methodology begins (around lines 75–90, when the description of CROCO and the model setup starts). The introduction would benefit from a more explicit motivation for the modeling approach adopted, a clearer justification for the use of this specific tool, and a more precise formulation of the study objectives and its novel contributions. I would expect to find a paragraph explaining why this type of model is needed, what gap in current knowledge it addresses, and what questions this study is able to resolve, followed by a clear statement of objectives. In addition, heat fluxes are discussed and analyzed in the Results section; however, the "Methodology" does not describe how these heat fluxes were calculated. The manuscript should clearly specify which heat flux formulation or parameterizations were used in CROCO.

More generally, the manuscript would benefit from a clearer and more conventional scientific structure, with well-defined sections (e.g., Introduction, Methodology, Results, Discussion, and Summary and Conclusions). At present, the boundaries between sections are not always clear, making it difficult to distinguish between background information, methodological descriptions, results, and interpretation. This also affects the readability of the manuscript and the clarity of its main messages.

We reorganized the manuscript into clearly separated sections which improved the clarity of the manuscript's structure and main messages.

The corrected version of the manuscript is organized as follows:

> Section 2 describes the methods, including the model configuration, the simulation domain, the datasets used, and the statistical approach applied to identify upwelling events.
>
> Section 3 presents the simulated fields and evaluates the model's ability to reproduce the mean state and interannual variability through comparisons with reanalyses, in situ observations, and published studies.
>
> Section 4 focuses on the analysis of upwelling processes, including the identification of upwelling dates, the composite distribution of SST and SSS anomalies, and the examination of selected spring and autumn events and their vertical structure.
>
> Finally, Section 5 summarizes the main findings and provides concluding remarks.

The reviewer notes that heat fluxes are analyzed in the Results section, but their formulation is not described in the Methodology, and requests clarification on the heat flux parameterization used in CROCO. The heat flux discussed in the Results section corresponds to the net air–sea heat flux output from the CROCO simulation. It is not externally imposed in the analysis, but rather a model variable consistently computed during the integration as with other prognostic variables (e.g., temperature and salinity). It reflects the surface energy exchange resulting from the prescribed atmospheric forcing and the model's surface flux formulation. This was clarified in the revised Methods section.

The comparison with previous studies is rather limited. While there is some discussion of the water column structure, there is little quantitative comparison with existing literature or independent datasets, particularly regarding the validation of vertical velocities and the vertical structure of temperature and salinity. As a result, it is not always clear whether the simulated values are realistic. The limited use of references to benchmark the results also makes it difficult to clearly identify the novelty of the study beyond its descriptive analysis.

The model validation presented in our study, includes comparisons with (to our knowledge) all available observational datasets in the region, including in situ temperature and salinity time series from Isla de Flores and vertical profiles from the BARDO database, as well as comparisons with GLORYS reanalysis fields. These comparisons demonstrate that the model realistically reproduces the vertical structure and variability of temperature and salinity in the coastal region. We note that observational data in this region remain spatially and temporally limited, particularly regarding the vertical structure and spatial extent of upwelling events. This limitation has been highlighted in previous studies and represents one of the main motivations for using high-resolution numerical simulations. To clarify this point, we revised the manuscript to more explicitly highlight the validation results and the novelty of the study.

The novelty of this work lies in the identification and characterization of upwelling events throughout the year for the Uruguayan region, and in demonstrating the relevance of sea surface salinity as an alternative proxy for detecting these events outside the summer season. This approach provides new insights into the seasonal variability and structure of upwelling in the region, which cannot be fully resolved using currently available observational datasets alone.

The methodology section requires more technical detail, especially for shallow bathymetric regions. For instance, the minimum and maximum depths considered in the simulations should be explicitly stated. In addition, the description of the forcing needs to be more detailed, particularly regarding freshwater inputs and tidal forcing.

The Model Setup (Section 2) was revised to explicitly state that a minimum depth of 5 m was imposed to ensure numerical stability in shallow coastal regions. The description of freshwater and tidal forcing was also expanded in the modified version to clearly specify the datasets used and their implementation in the simulations. Additional configuration details are also provided in previously published and validated studies (de Mello et al., 2022, 2023).

Several figures and figure captions also require corrections to improve clarity, consistency, and organization. Some figures show inconsistencies between what is described in the caption and what is actually displayed, while others would benefit from clearer panel labeling and ordering.

Figures and captions were updated to ensure consistency, clarity, and coherence throughout, and labels, abbreviations, and panel lettering were standardized to align with the captions and the terminology used in the main text.

Specific comments:

The paragraph around line 45 requires additional references to support the statements made. The material presented between lines 75 and 90 would be more appropriate in the Methodology section rather than in the Introduction.

In accordance with the reviewer's suggestion, we added general references supporting the underlying physical mechanisms described, as well as studies from other estuarine systems that show seasonal variations in thermal structure (Knauss, 1997; Geyer, 2010; Mahardja et al., 2022). Although limited studies in our region document the occurrence of thermal inversion conditions based on specific observational surveys, and one atlas characterizes the mean temperature and salinity conditions during the cold and warm period (Martínez & Ortega, 2015 and Guerrero et al., 2003; respectively), both references were incorporated in the Introduction when discussing stratification conditions along the Uruguayan coast. To our knowledge, there are no studies that explicitly describe the mean vertical temperature structure in the Uruguayan coastal region. If the reviewer is aware of specific studies addressing this mechanism in the region or in comparable freshwater-influenced coastal systems, we would be very grateful for any suggested references, which we would gladly incorporate to further strengthen the manuscript.

He suggests that sentences in the Introduction referred explicitly to the application of the Maximum Covariance Analysis to be removed and the paragraph reformulated to focus on the objectives and scientific motivation of the study. Following this suggestion, the methodological description of the MCA is now presented exclusively in Section 2, where the analysis is introduced in detail.

Regarding Figure 1 (lines 105–110), according to the caption, CL and TR should be indicated in the figure to identify the coastal location and the transect, respectively. However, in the figure only "IF" and "Transect" appear. This should be corrected for consistency, especially since IF (Isla de Flores) is referenced later in the text. In addition, if abbreviations are used for cities (e.g., Mvd, PdE), the corresponding abbreviation for PP should also be included.

We have revised the figure accordingly. Specifically, we added a broader regional context to clearly situate the study area within South America. The model domain is now explicitly indicated, and internal administrative boundaries have been removed to avoid visual distraction. Isobaths have been incorporated in the bathymetry panel to improve readability. In addition, the labels TR, CL, and RdP have been explicitly included in the figure, and their meaning has been clarified in the caption. The improved version of Figure 1 is provided below.

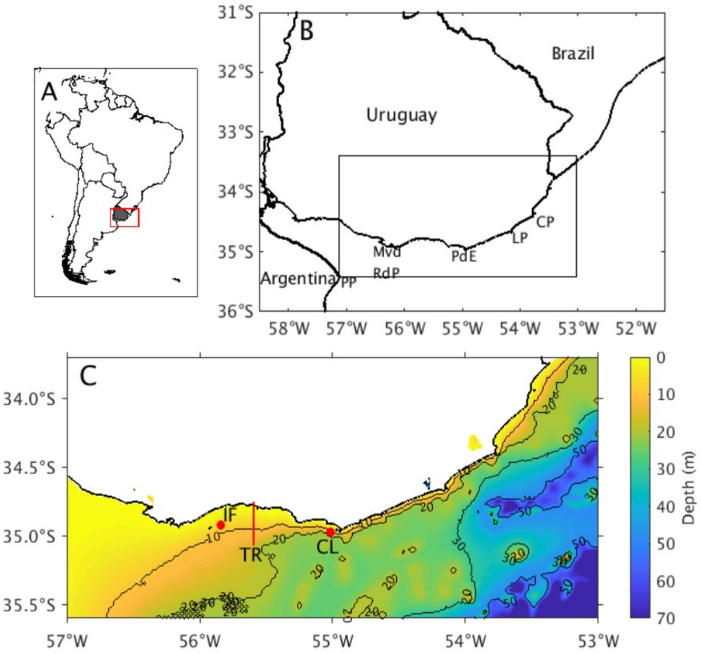

Figure 1. (A) Geographical context of the study area, showing Uruguay within South America; the red rectangle indicates the region displayed in panel B. (B) Model setup area along the Uruguayan shelf. The inner rectangle indicates the subdomain used for the analysis and identification of upwelling events. Mvd: Montevideo; PdE: Punta del Este; LP: La Paloma; CP: Cabo Polonio; PP: Punta Piedras; RdP: Río de la Plata. (C) ETOPO1 bathymetry (color scale in meters). Black contours indicate selected isobaths. IF marks Isla de Flores (location used for model–observation comparison of SST and SSS); CL indicates the coastal location used to extract vertically averaged seasonal profiles; TR denotes the transect used to analyze the vertical structure of simulated upwelling.

Around lines 115–120, it appears that only two river inputs are included. Please clarify whether these are the only freshwater sources considered in the simulation. Furthermore, the tidal forcing should be specified more clearly, including which tidal model or tidal constituents were used.

The freshwater input corresponds to the Río de la Plata discharge, computed as the combined flow of the Paraná and Uruguay rivers. The total discharge was imposed at the head of the Río de la Plata estuary and divided into two source points to ensure numerical stability and to adequately represent the estuarine inflow without generating instabilities in the model. These are therefore not two independent river systems, but a numerical implementation of the single Río de la Plata discharge.

Tidal forcing was incorporated using harmonic constituents derived from the TPXO7 global tidal model (Egbert and Erofeeva, 2002). Tidal elevation and depth-integrated velocities associated with the main tidal constituents were included directly in the CROCO tidal forcing file, following the standard CROCO tidal forcing framework. These constituents were applied dynamically at the open boundaries using Flather-type conditions for barotropic velocities, allowing realistic tidal propagation into the model domain. This implementation follows the configuration described and validated in de Mello et al. (2023). The manuscript was revised accordingly to clearly specify those details.

In Figure 2 (around line 160), the panel labels (a, b, c, etc.) should be placed outside the title, particularly for panels e and f, where the letters appear to be part of the title. It is recommended to place the panel labels as text positioned, for example, in the upper-left corner of each panel.

Figures and captions were updated in the final version of the manuscript as suggested.

In Figure 5 (around line 210), please revise the order of the panel labels so that the left panels are labeled a, b, c, d, and the right panels e, f, g, h, to improve clarity and consistency.

We thank the reviewer for this suggestion. However, the panel labeling has been intentionally kept as originally presented in order to maintain consistency with the labeling scheme used in the other multi-panel figures throughout the manuscript. In all figures, panels are labeled sequentially following a row-wise order (i.e., a–b for the first row, c–d for the second row, and so on), corresponding to panels located in row 1 column 1, row 1 column 2, row 2 column 1, row 2 column 2, respectively. For this reason, we have retained the original labeling in Figure 5.

Finally, in the caption of Figure 7 (around line 240), the word “upwelling” is repeated twice and should be corrected.

The repetition of the word “upwelling” in the caption of Figure 7 has been corrected in the revised manuscript.

For Figure 3 (around line 175), I suggest adding a vertical profile from the nearest GLORYS grid point to provide a clearer reference and vertical validation of the model results.

We extracted seasonal temperature and salinity profiles from the nearest GLORYS location to the coastal site and provided them here for comparison (Figure X, not present in the manuscript). The seasonal cycle and overall vertical structure are consistent between both datasets, with warmer and more stratified conditions in DJF and more homogeneous profiles in JJA (compared with Figure 3). GLORYS shows salinity-driven stratification in all seasons, which is also captured by the regional model, although with differences in magnitude and vertical gradients. These differences are expected given the higher horizontal resolution of our regional model (1/36°) compared with GLORYS (1/12°), which allows a better representation of sharp coastal gradients. Overall, the comparison supports the physical consistency of the simulated vertical structure.

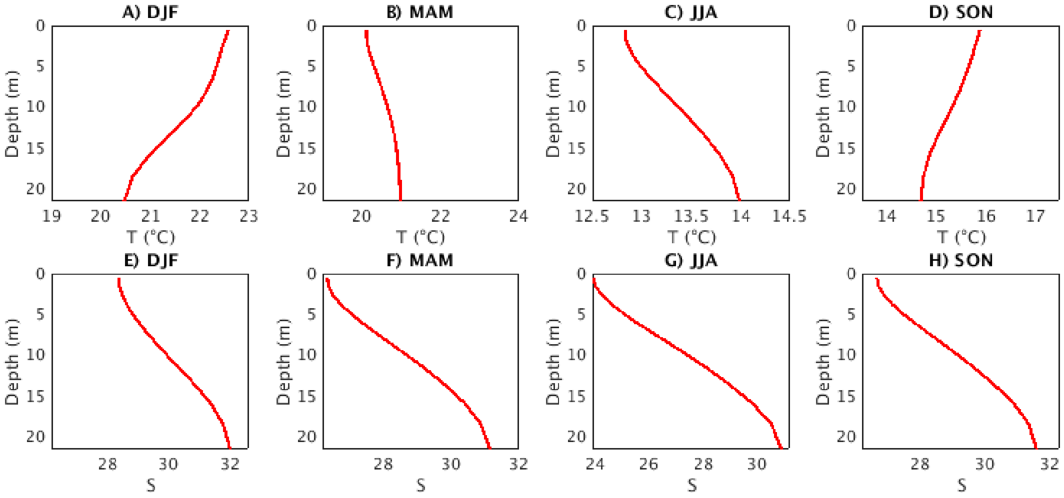

Figure X. GLORYS seasonal average vertical profiles of temperature (A-D) and salinity (E-H) at the closest GLORYS point to the coastal location. Different scales were used for temperature profiles.

In addition, the use of a high-resolution regional model aims to provide a representation of coastal processes that is closer to real ocean conditions than that of the GLORYS reanalysis (as shown in Figure X2, not added in the manuscript).

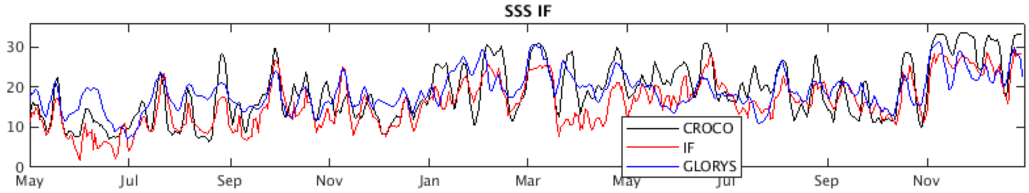

Figure X2. Evolution of daily SST (E) and SSS (F) for the period May 2019 – December 2020 simulated by CROCO (black line), measured in situ (red line) at Isla de Flores Location and represented by the GLORYS reanalysis (blue line). Correlation between GLORYS and Isla de Flores was 0.70 and between CROCO and IF 0.83.

Response to Reviewer 2 (Anonymous reviewer)

We thank the reviewer for the careful and constructive assessment of our manuscript and for recognizing the robustness of the analysis and overall clarity of the study. We have carefully addressed all comments and revised the manuscript accordingly. Below we respond point by point following the order of his/her comments.

General Comments

The manuscript by de Mello et al. uses a regional numerical simulation to investigate wind-induced coastal upwelling off the coast of Uruguay. The model performance is validated against an ocean reanalysis and in situ observations. The model appears to reproduce reasonably well the coastal observations, while compared with hydrographic data the vertical structure is somewhat more poorly represented. The authors conclude that, due to the weak thermal stratification, during spring and fall sea surface salinity is a better proxy than sea surface temperature. The manuscript is generally well written and the analysis appears to be robust. However, there are several issues that require the authors attention for it to become acceptable for publication.

Throughout the manuscript the authors refer to the coast of Uruguay, which is fine for a regional audience. However, to become more appealing to a wider audience, such as the readership of OS, I strongly suggest presenting these results as valid for many regions where the vertical stratification is modulated by the salinity stratification, such as observed in many estuaries and their region of influence around the world.

In connection with the previous issue, the Introduction starts with a brief description of the regional oceanography of the study area. I missed a more general introduction, briefly stating why is upwelling an important process and why it is important to identify effective upwelling indicators. This is partly addressed in the paragraph starting in line 50, but it should be revised and moved to the beginning of the introduction.

We thank the reviewer for this important comment. Following this suggestion, we revised the Introduction to open with a broader description of coastal upwelling and its importance in shaping marine ecosystems, biogeochemical cycles, and fisheries (Pauly and Christensen, 1995; Carr, 2001; Chavez and Messié, 2009; Belem et al., 2013; Aguiar et al., 2014; Cao et al., 2014; Wang et al., 2015). The paragraph previously located around line 50 was moved to the beginning of the Introduction and expanded, so that the general relevance of upwelling processes and the importance of identifying effective indicators are clearly established before introducing the regional context.

The inverted temperature stratification is a relevant feature and important to understand the results. However, it is only briefly mentioned in the introduction (line 47). Though the heat loss to the atmosphere must indeed play a role, it is the low salinity surface layer which prevents winter convection. Thereby, heat is lost primarily from the low salinity surface waters. Horizontal advection may also play a role, as is apparent in some of the results. In any case, the vertical temperature stratification should be described in more detail in the introduction; there is abundant literature addressing it in this region and elsewhere.

In accordance with the reviewer’s suggestion, we added general references supporting the underlying physical mechanisms described, as well as studies from other estuarine systems that show seasonal variations in thermal structure (Knauss, 1997; Geyer, 2010; Mahardja et al., 2022).

Although limited studies in our region document the occurrence of thermal inversion conditions based on specific observational surveys, and one atlas characterizes the mean temperature and salinity conditions during the cold and warm period (Martínez & Ortega, 2015 and Guerrero et al., 2003; respectively), both references were incorporated in the Introduction when discussing stratification conditions along the Uruguayan coast. To our knowledge, there are no studies that explicitly describe the mean vertical temperature structure in the Uruguayan coastal region. If the reviewer is aware of specific studies addressing this mechanism in the region or in comparable freshwater-influenced coastal systems, we would be very grateful for any suggested references, which we would gladly incorporate to further strengthen the manuscript.

Model

Model implementation

I missed a proper description of the model implementation, for instance:

> exactly what are the boundary conditions imposed? How were those implemented?
> It appears that the model is forced with daily NCEP wind stress so I wonder why do you use monthly COADS surface heat and freshwater fluxes instead of NCEP surface fluxes, which are consistent with the surface winds?
> How are tides implemented?

1. Open boundary conditions were applied at the eastern, northern, and southern boundaries using GLORYS fields. Boundary forcing was prescribed for sea surface height, barotropic and baroclinic velocities, and tracers (T/S) via boundary files. A Flather condition was used for the barotropic velocities, while Orlanski-type radiation conditions were used for three-dimensional momentum and tracers, together with sponge layers near the open boundaries. The manuscript was revised to clarify this implementation.

2. The objective of our study was to investigate wind-driven coastal upwelling, which is primarily a mechanically forced process controlled by surface wind stress. Therefore, the model was forced with interannual daily wind stress from the NCEP-DOE Reanalysis 2 to accurately reproduce the temporal variability and intensity of upwelling-favorable winds. Climatological heat and freshwater fluxes from the COADS dataset were used in combination with restoring terms toward observed sea surface temperature and salinity climatologies. This approach allowed to maintain realistic hydrographic conditions while preventing long-term model drift and minimizing uncertainties associated with surface heat flux products (and was also used at de Mello et al., 2022 and 2023). The restoring formulation allows surface heat and freshwater fluxes to adjust dynamically in response to model SST and SSS anomalies, ensuring physically consistent air–sea exchanges.

Because coastal upwelling in this region is primarily driven by wind stress, the use of climatological surface heat fluxes does not affect the occurrence or timing of upwelling events, although it may influence the magnitude of surface temperature anomalies. This configuration has been previously implemented and validated for the same regional model setup. The manuscript was revised to clarify this point.

3. Tidal forcing was incorporated using harmonic constituents derived from the TPXO7 global tidal model (Egbert and Erofeeva, 2002). The amplitudes and phases of the main tidal constituents were included directly in the CROCO forcing file, following the standard CROCO tidal forcing framework. These constituents are applied dynamically at the open boundaries using Flather-type conditions for barotropic velocities, allowing realistic tidal propagation into the model domain. This configuration follows the implementation described and validated in de Mello et al. (2023). The

manuscript was revised accordingly.

I don´t understand what´s the rationale behind comparing the model output against the ocean reanalysis instead of, for example, satellite derived SST, altimetry, etc. Likewise, how does the model compare with existing climatological SSS distributions? What features of the simulation make it "satisfactory" (l. 126)? Please explain this more clearly.

We understand the reviewer's concern regarding the rationale for comparing the model output with an ocean reanalysis. GLORYS is not used as a substitute for observations but as a complementary validation dataset. Importantly, the GLORYS reanalysis assimilates multiple observational products, including satellite-derived sea surface temperature and sea surface height, as well as in situ temperature and salinity profiles. Therefore, it provides an observationally constrained, dynamically consistent three-dimensional estimate of the ocean state. This makes it particularly useful for evaluating the model representation of the regional hydrographic structure and circulation. In addition, the use of a high-resolution regional model aims to provide a representation of coastal processes that is closer to real ocean conditions than that of the GLORYS reanalysis (as shown in Figure X, shown here for clarification but not included in the manuscript).

In this sense, our validation strategy was designed to assess the model's ability to reproduce the mean hydrographic structure and variability of multiple fields, including sea surface temperature, salinity, and circulation. Satellite observations are primarily limited to surface temperature and sea surface height and do not provide direct information on salinity or subsurface structure in this region. Moreover, there is currently no satellite-derived salinity product with sufficient spatial resolution to resolve the narrow coastal and estuarine features of the Uruguayan shelf, which limits their applicability for validating SSS distributions. Previous studies using a similar model configuration (de Mello et al., 2022, 2023) also compared model SST with satellite-derived products (MUR-GHRSST), showing good agreement in spatial patterns and variability.

In addition to the comparison with GLORYS, we validated the model against in situ observations of the BARDO Argentina database of temperature and salinity; and with SST and SSS at Isla de Flores, where the model reproduces both the climatological values and daily variability with statistically significant correlations. Furthermore, the simulated mean distributions and vertical structures are consistent with climatological patterns described in previous observational studies and regional hydrographic atlases. In addition, composites of simulated SST anomalies (SSTa) for the identified upwelling dates (Figure 7A–F) were compared with composites of observed SSTa derived from the Multi-scale Ultra-high Resolution Sea Surface Temperature dataset (MUR-GHRSST; Chin et al., 2017) for the same dates (Figure 7G–I). This event-based comparison provided an additional validation of the model's ability to accurately represent the spatial structure of SST anomalies during simulated upwelling events.

He/She asks for clarification regarding which features justify describing the model performance as "satisfactory" (l. 126). In the manuscript, this statement was intended as an introductory sentence to the detailed validation that follows, including comparisons with GLORYS reanalysis, in situ observations at Isla de Flores, correlation coefficients for daily variability, and consistency with published climatological patterns. To avoid ambiguity, we revised the wording to explicitly indicate that the assessment is based on these quantitative comparisons and documented agreements presented in the subsequent paragraphs.

Section 2 should provide a brief description of the nature of the in-situ observations, both, at Isla de Flores, and the June 2006 cruise.

The manuscript was revised to include a brief description of the in situ observations at Isla de Flores and during the June 2006 cruise as suggested by the reviewer.

The model compares very well with the in-situ temperature observations at Isla de Flores, but in the later part of the record, after January 2020, the model salinity presents large variations not apparent in the observations, and a rather gross salinity overestimation. Given the relevance of sea surface salinity to the main results of the manuscript, these differences must be addressed. Likewise, the authors suggest that some differences in the observed and modeled climatologies may be due to the fact that the observations are only available during 2018-2023, and that there was a drought leading to reduced runoff in 2020-2022. Since the model includes the observed river discharge, a model climatology matching the observational record should be constructed for comparison purposes.

We thank the reviewer for this careful and important observation and agree that this discrepancy required clarification. After revisiting the in-situ salinity dataset, we identified that the discrepancy originated from the use of raw conductivity-derived salinity values in the previous version of the manuscript. Although the conductivity sensors are cleaned approximately once per month, biofouling between maintenance periods leads to a progressive reduction in conductivity measurements, which results in an artificial underestimation of salinity in the raw time series. Therefore, the apparent model overestimation reported previously was largely influenced by this observational bias.

For the revised manuscript, we used a corrected salinity time series (which is the one present at Trinchin et al., 2021). The correction was performed by adjusting the conductivity-derived salinity values to a reference curve constructed from discrete salinity measurements collected along the coast using a calibrated YSI multiparameter probe. This procedure substantially reduces the artificial low-salinity bias in the later part of the record.

After implementing this correction, the agreement between model and observations improves considerably. The large discrepancies previously observed after January 2020 are no longer present. Remaining differences are less intense and mainly confined to specific periods (particularly during April–May 2020). These residual discrepancies may partly reflect uncertainties inherent to the correction procedure, which, although significantly improving the dataset, cannot completely eliminate observational limitations.

Additionally, the time series has been extended through December 2020, matching the period covered by the model output. The extended record shows a generally consistent evolution between model simulations and corrected observations and improves the correlation between observation and simulation (both correlations are significant, with r=0.96 for SST and r=0.83 for SSS). The corresponding panels E and F of Figure 2 from the manuscript are presented here and were updated in the revised version of the manuscript.

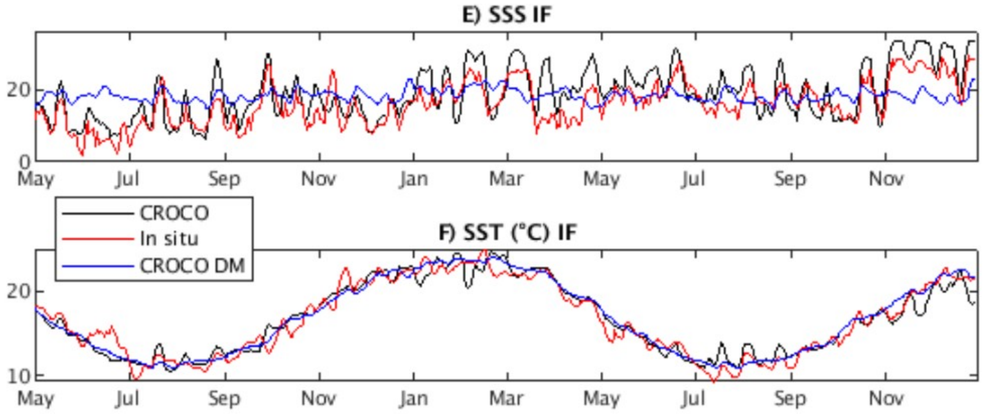

Figure 2 E and F. Evolution of daily SST (E) and SSS (F) for the period May 2019 – December 2020 simulated by CROCO (black line), and measured in situ (red line) at Isla de Flores Location. The CROCO daily mean (DM) is shown in blue line for both plots.

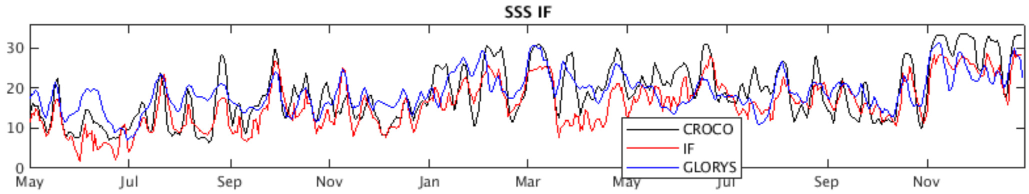

Figure X. Evolution of daily SSSfor the period May 2019 – December 2020 simulated by CROCO (black line), measured in situ (red line) at Isla de Flores Location and represented by GLORYS (blue line). Correlation between GLORYS and Isla de Flores was 0.70 and between CROCO and IF 0.83.

The reviewer also noted that differences between observed and modeled climatologies may be related to the fact that the observational record (2018–2023) includes the drought period (2020–2022), and suggested constructing a climatology matching the observational period for comparison.

We thank the reviewer for this important remark and agree that differences in temporal coverage may influence climatological comparisons. We would like to clarify that the model simulation and associated river discharge forcing extend through December 2020. Therefore, the modeled climatology only includes the initial phase of the drought event. In contrast, the Isla de Flores observational record is largely concentrated during the drought period (2020–2022), when river discharge was substantially reduced. Under these conditions, higher coastal salinity values are expected due to diminished freshwater input from the Río de la Plata.

Thus, part of the discrepancy between modeled and observed climatological salinity fields can be attributed to the fact that the observational climatology is dominated by drought conditions, whereas the modeled climatology represents a longer multi-year period (2005–2020) that includes both normal and low-discharge conditions. To clarify this point, we revised the manuscript to explicitly describe the temporal coverage of both datasets and the implications of the drought period for the interpretation of climatological differences.

I very much welcome the comparison with observations collected in June 2006. It appears that the model somewhat underestimates the vertical T and S gradients in the oceanic domain. Please address the possible impact of this difference in the Discussion.

We would like to clarify that the comparison shown in Figure 4 corresponds to observations collected at specific stations and times during the June 2006 Campaign, while the model output represents daily mean values averaged over grid cells of approximately 2.5 km resolution. Therefore, small differences in the vertical temperature and salinity gradients are expected due to spatial mismatches between station locations and model grid points, as well as temporal variability that may occur at sub-daily timescales, particularly near the surface. For this reason, this comparison was intended to evaluate whether the model reproduces the observed vertical structure and water mass characteristics, rather than to provide a point-by-point quantitative validation of gradient magnitudes. In this regard, the model successfully reproduces the key observed features, including the presence of warmer and saltier waters at depth in both estuarine and oceanic regions. To clarify this point, we revised the manuscript to explicitly state the nature and limitations of this comparison.

Fig 3 indicates that in autumn there is little thermal stratification, or slightly warmer waters below the surface layer. Yet, the upwelling composites presented in Fig. 7 show negative temperature anomalies throughout. Where would the cold surface anomaly be derived from? Does horizontal advection play a role? Analysis of the vertical stratification during the times of the composites may draw some light on this issue.

We thank the reviewer for this careful observation. We agree that distinguishing between the seasonal mean stratification and the stratification during specific upwelling events is essential for interpreting these results. Figure 3 represents the seasonal mean vertical temperature structure, averaged over the entire season and including periods both with and without upwelling. In contrast, Figure 7 presents composites of temperature anomalies during intense upwelling events only. These events occur under different background stratification regimes, which are not necessarily captured by the seasonal climatology.

As shown in the analysis of individual events (Figures 9–12), the vertical structure immediately prior to upwelling can differ substantially from the seasonal mean state (shown in Figure 3). In particular, during autumn, some upwelling events occur earlier in the season, when the water column may still retain stratification from summer conditions. In those cases, colder subsurface waters can be uplifted toward the surface, leading to negative SST anomalies. Other events occurring later in the season may involve weaker or inverted thermal stratification, resulting in different surface temperature responses. Therefore, the composite anomalies from Figure 7 represent an average response across events with distinct pre-existing vertical structures and do not necessarily correspond directly to the seasonal mean stratification shown in Figure 3.

To clarify this point, we analyzed the temporal evolution of the vertical difference in temperature and salinity between a near-surface and a deeper level at a coastal location (Figure X2 not shown in the manuscript). Salinity differences (ΔS) are predominantly negative throughout the year, indicating persistent vertical stratification associated with fresher surface waters overlying saltier subsurface waters. In contrast, temperature differences (ΔT) exhibit stronger seasonal variability. During DJF, ΔT is generally positive, reflecting thermally stratified conditions consistent with the seasonal mean structure shown in Figure 3. Under these conditions, upwelling events are expected to bring colder subsurface waters to the surface, producing negative SST anomalies.

During the transitional seasons (MAM and SON), ΔT values are smaller and frequently change sign, indicating periods of weak stratification, thermal stratification and thermal inversion. This variability suggests that upwelling events occurring within these seasons may uplift subsurface waters with differing thermal properties, depending on the pre-existing vertical structure. Consequently, the composite anomalies do not necessarily reflect the seasonal mean stratification but rather the background conditions present during each individual event.

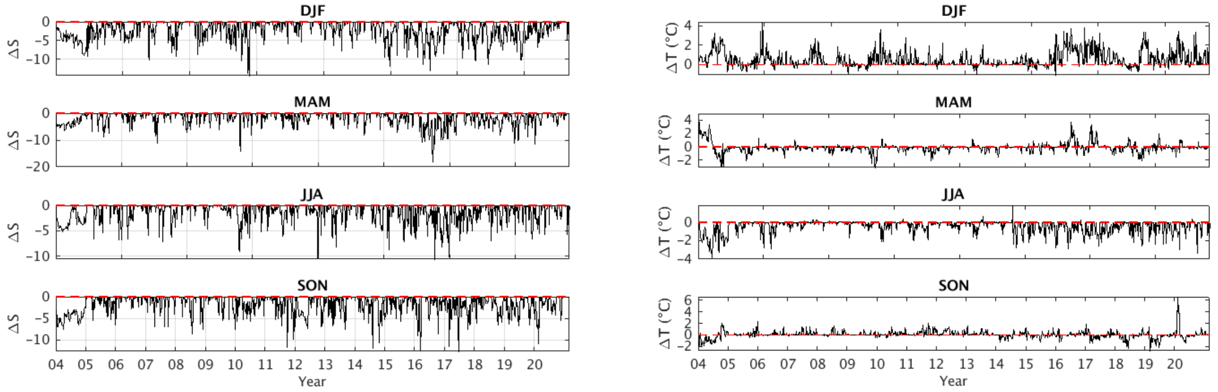

Figure X2. Seasonal evolution of vertical temperature (ΔT) and salinity (ΔS) differences between an upper (level 6) and a deeper (level 36) sigma level at a coastal location (2004–2020). The red dashed line indicates zero difference.

Regarding horizontal advection, while it is always present in the system and was also shown in uor previous studies (de Mello et al., 2022 and 2023), our interpretation emphasizes that the key factor explaining the composite signal is the pre-existing vertical structure during the selected upwelling events rather than a fundamentally different horizontal advective mechanism.

To clarify this point, we revised the manuscript to explicitly distinguish between seasonal mean stratification and event-based vertical structure, and to highlight the role of pre-upwelling conditions in determining the surface anomaly patterns.

Upwelling event selection

Six events are analyzed in section 5. The events were selected based on to their distinct SST characteristics. However, to fully understand this further information is needed. The selection appears to only include periods classified as "intense" based on the SSSa exceeding a certain threshold as described in section 4, please clarify.

The six events analyzed were selected from the set of intense upwelling events identified objectively using the SSS anomaly threshold defined in a previous Section, based on the Maximum Covariance Analysis between zonal wind and SSS anomalies. From this objectively defined set, representative events were chosen to illustrate the different thermal responses associated with upwelling during the transition seasons, including both cold-water and warm-water cases. The manuscript was revised to clarify the event selection criteria.

The Sept 2014 case presents a warm surface anomaly in the upwelling band. This is hard to understand based on the climatological profiles (Fig 3), which indicate very weak thermal stratification. The temperature anomaly and vertical velocity cross-sections draw some light on the issue. The warm upwelled waters are associated with positive subsurface anomalies farther offshore. This suggests that the surface temperature anomalies critically depend on the temperature anomalies of the subsurface layer in the offshore region, and not on the thermal stratification close to shore. These issues should be addressed.

We appreciate this thoughtful interpretation. However, as mentioned in some lines above, we would like to clarify that Figure 3 represents the seasonal mean vertical structure, which includes periods with weak, direct, and inverse stratification. The September 2014 event corresponds to a specific dynamical situation and should not be interpreted directly from the seasonal climatology. During the upwelling event, the nearshore stratification is effectively eroded by the vertical motion associated with the wind-driven circulation. The vertical velocity and temperature anomaly cross-sections allow to interpret that anomalously warm subsurface waters located offshore are advected shoreward at depth and subsequently uplifted toward the surface. Simultaneously, surface waters are transported offshore as part of the upwelling circulation. Therefore, the resulting surface temperature anomaly reflects the three-dimensional dynamical adjustment of the system, involving both cross-shore advection and vertical motion.

The May 2019 event provides the opportunity to compare the model surface temperature and salinity changes with direct observations during the upwelling setup and development. I suggest adding the evolution of the zonal wind stress during this event in Fig 13A. The model reproduces these changes quite well. However, note that the simulated T profile during the event peak (19 May, Fig 13C) is too warm (T>17.6ºC) compared with the subsurface temperature before the event (17.2ºC). In contrast, the salinity during the peak of the upwelling (17) is close to the subsurface salinity at the beginning of the event. A more in-depth analysis of the model output may draw some light on the origins of the upwelled water.

We thank the reviewer for this careful and constructive comment. We improved Figure 13 by adding the evolution of the zonal component of the NCEP2 wind during this event.

The comparison between pre-upwelling and peak-upwelling vertical profiles indeed provides valuable insight into the origin of the upwelled waters. However, it is important to note that the “pre-upwelling” temperature profile shown in Figure 13 corresponds to conditions five days prior to the event peak. During this time, the offshore thermal structure evolved due to advection and atmospheric forcing. Therefore, the subsurface temperature measured locally five days before the event does not necessarily represent the exact water that reaches the surface during the peak of upwelling. An improved version of Figure 13 is shown here, and was added to the final version of the manuscript.

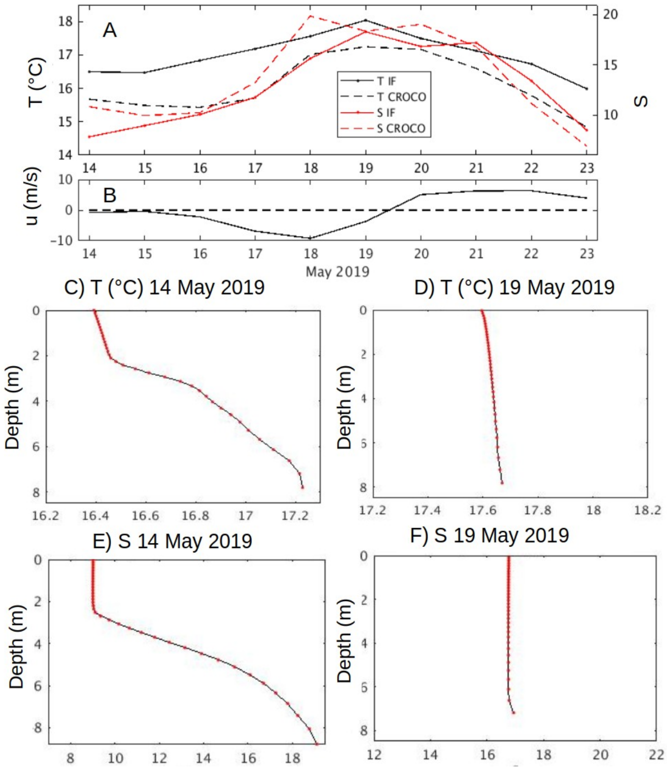

Figure 13. A- In situ (T IF and S IF) and simulated (T CROCO and S CROCO) temperature and salinity at Isla de Flores Location during the period 14-23 May 2019. NCEP2 zonal wind component for the Uruguayan coast (B). Simulated vertical profiles of salinity and temperature for 14 and 19 May 2019 at Isla de Flores location (D-F).

Other minor comments

Line 18, suggest replacing "…due to differences in thermal vertical profiles." by "…due to distinct vertical temperature stratification patterns."

Line 46, "During other seasons…" instead of "Outside of summer…" Also in line 380

We thank the reviewer for these suggestions. The text was revised accordingly.

Line 99, exactly how was the bathymetry modified (smoothed?) to minimize pressure gradient errors? This is a very important point because the inner shelf circulation is strongly influenced by bathymetric features, yet high-resolution bathymetry (e.g. multi-beam) is rarely available.

A smooth factor of r = 0.1 (r = grad(h)/h) was applied to the topography in order to prevent horizontal pressure gradient errors associated with the terrain following coordinates (Haney, 1991). The text was modified accordingly.

Line 115, Rio de la Plata must be indicated in Figure 1, as well as the location of the two sources. An explanation of why two sources are used is required.

The total freshwater discharge was introduced through two nearby source points located at the head of the estuary. This approach was adopted to distribute the large freshwater input over more than one grid cell, avoiding unrealistically large local fluxes and excessive salinity gradients, and ensuring numerical stability. This implementation follows the configuration previously validated in de Mello et al. (2022, 2023). The text was modified accordingly.

Line 149, “It is …” instead of “It's …”

Lines 174-175, “…and a pronounced inverse stratification in winter.” Is misleading, indeed you refer to the inverse thermal stratification, please clarify

Lines 197-198, I presume you are referring to anomalies of the zonal component of the wind stress. Please clarify.

We thank the reviewer for these suggestions. The text will be revised and clarified accordingly.

Line 209-210, please comment further why the leading mode of variability fails to produce the expected salinity pattern in winter.

In winter, the leading MCA mode does not exhibit a clear coastal SSS anomaly pattern. A plausible explanation is that the surface salinity response to wind forcing is weaker and less coherent in winter because the mean vertical density stratification is reduced and primarily controlled by salinity (the thermal contribution differs from summer), which may yield smaller-amplitude SSS anomalies during upwelling. In addition, upwelling-favourable winds are expected to be less frequent and/or less persistent in winter, reducing the robustness of the coupled wind–SSS signal. We added a short paragraph in the manuscript to clarify these points and note that a dedicated winter-focused analysis would be required to fully resolve the underlying mechanisms.

Line 263, Figure 11 cited out of order

Line 310, I presume you refer to the Isla de Flores data, please clarify.

Lines 362 and 365, refer to subsurface water instead of deep water.

Most figures are too small and, in many cases, the embedded text difficult to read. I also noted that in the Figures with multiple panels the panels are not properly aligned, please check.

We thank the reviewer for these suggestions. The text and figures were revised and improved accordingly.

Figure 1: Please expand the map in panel a and indicate the model domain within it. Also the lines on land in both panels are distracting, please remove them. Panel b should include some isobaths for improved readability.

CL is not indicated in panel b and Tr is presumably indicated as "Transect"

Please add some isobaths in panel b

We thank the reviewer for these comments, which will help improve Figure 1. Specifically, we added a broader regional context to clearly situate the study area within South America. The model domain is now explicitly indicated, and internal administrative boundaries have been removed to avoid visual distraction. Isobaths have been incorporated in the bathymetry panel to improve readability. In addition, the labels TR, CL, and RdP have been explicitly included in the figure, and their meaning has been clarified in the figure caption. The improved version of Figure 1 is provided below.

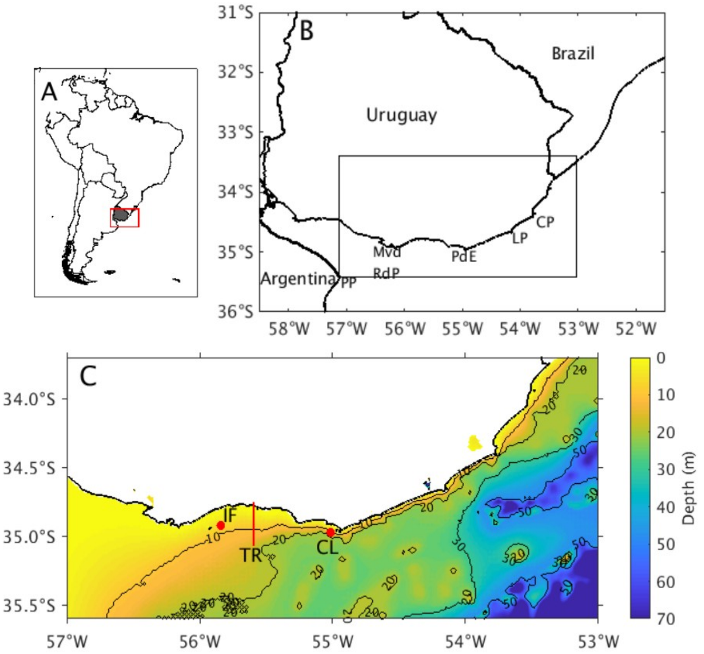

Figure 1. (A) Geographical context of the study area, showing Uruguay within South America; the red rectangle indicates the region displayed in panel B. (B) Model setup area along the Uruguayan shelf. The inner rectangle indicates the subdomain used for the analysis and identification of upwelling events. Mvd: Montevideo; PdE: Punta del Este; LP: La Paloma; CP: Cabo Polonio; PP: Punta Piedras; RdP: Río de la Plata. (C) ETOPO1 bathymetry (color scale in meters). Black contours indicate selected isobaths. IF marks Isla de Flores (location used for model–observation comparison of SST and SSS); CL indicates the coastal location used to extract vertically averaged seasonal profiles; TR denotes the transect used to analyze the vertical structure of simulated upwelling.

Figure 3: there is something wrong with the temperature labels in MAM

Figure 3 and its label were corrected accordingly.

Figure 6: please provide further details on the statistics represented by symbols, whiskers and boxes.

In the revised manuscript, we explicitly described these components in the figure caption. The box represents the interquartile range (25th–75th percentiles), the horizontal line inside the box indicates the median, the whiskers extend to 1.5 times the interquartile range, and values beyond this range are displayed individually as outliers.

Figures 7 and 9: a symmetric color palette centered at zero, similar to SSSa, should be used to display SSTa fields. Even if the anomaly is of the same sign throughout the domain such as in SON and MAM, this will help the reader to visually grasp the distributions.

We thank the reviewer for this suggestion. Our intention in using seasonally adjusted color scales was to better highlight the spatial structure of the upwelling signal in each season. Because the magnitude of SST anomalies differs substantially between seasons, using a single symmetric scale centered at zero tends to mask the coastal upwelling pattern, particularly in seasons when anomalies are relatively weak. Therefore, the color ranges were adjusted to emphasize the spatial structure of the upwelling signal in each season. We clarified this choice in the figure caption.

Figure 8 and 9: check spelling of “Apr”

We thank the reviewer for noting this. The spelling was corrected in the figures.