Answers to Reviewer #2: Oscar Alvarez

General comment

This manuscript presents detailed observations of hydrodynamics, turbulence, and mixing processes at the estuary–shelf transition of the Patos Lagoon inlet. The manuscript is generally well structured and scientifically relevant. The observational effort is significant, and direct turbulence measurements in microtidal estuarine/plume systems remain relatively scarce, so the dataset presented here is valuable for publication. The discussion about the coexistence of strong stratification and energetic turbulence is particularly interesting and contributes to the broader understanding of estuarine mixing processes. I believe the manuscript has strong potential; however, several aspects still require clarification and further development before publication. The manuscript presents strong observational descriptions, but the physical interpretation could be developed more deeply in several sections. Some conclusions currently appear stronger than what is directly supported by the observations.

We thank the reviewer for the positive assessment of our work and for recognizing the value of the observational dataset, particularly given the scarcity of direct turbulence measurements in microtidal estuarine and plume environments. We are pleased that the reviewer found the discussion of the coexistence of strong stratification and energetic turbulence to be a meaningful contribution.

In response to the reviewer's comments, we have carefully revised the manuscript to strengthen the physical interpretation of the observations throughout the Results and Discussion sections. We have also revisited several statements in the conclusions to ensure that they remain fully supported by the observations presented. In particular, conclusions that could be interpreted as implying causal relationships or fully resolved dynamical mechanisms have been revised to emphasize the observational evidence and the corresponding level of inference. We believe these revisions improve the balance between observational description and physical interpretation and strengthen the overall manuscript.

Specific comments

General context of experiments:

The manuscript states that discharge through the estuary mouth is not directly proportional to the tributaries' discharge because wind strongly modulates the system. However, the study requires some context of the sampled conditions within the wind–discharge regime space. How representative are the observed conditions? What is the probability of occurrence of the sampled scenario? If sufficient historical data are available, a two-dimensional probability distribution of discharge and wind conditions, indicating the position of the sampled event, would greatly strengthen the interpretation.

To place our observations within the broader hydrodynamic context, we performed an additional analysis of the ebb flow regime using continuous ADCP observations from 2022, corresponding to the year of the field campaign. For each timestamp, the maximum along-channel velocity over the water column was extracted to ensure consistency with the velocity range reported in the manuscript (0.8–2.2 m s^{-1}).

Analysis of the 2022 ADCP record indicates that ebb flow conditions prevailed during 58.9% of the annual observations and during 86% of July 2022 observations (Figure R1 below, which is provided here for reference but not included in the manuscript), reflecting the well-known intensification of seaward-directed flow during winter months (Tomazelli, 1993; Möller and Castaing, 1999). This seasonal pattern is consistent with the export-dominated character of the lagoon system during high-discharge periods. To provide full support for this discussion within the paper, a new scatter plot comparing wind forcing versus estuarine flow conditions has been inserted as Figure 12 in the revised manuscript.

To assess the representativeness of the sampled current velocities, we computed the cumulative frequency distribution (CDF) of outflow intensity for both the full year and July 2022 (Figure R1). The velocities observed during the field campaign, expressed as depth-averaged along-channel velocity (0.69–1.01 m s^{-1}), correspond to conditions between the 74th and 97th percentile of the annual ebb velocity distribution, and between the 49th and 92nd percentile of the July 2022 distribution. This indicates that the field campaign encompassed moderate-to-strong ebb conditions that are representative of the typical outflow regime, rather than exceptional events.

Additionally, maximum velocities observed during the field campaign (0.8–2.2 m s^{-1}) correspond to conditions between the 56th and 100th percentile of the annual distribution, and between the

39th and 100th percentile of the July 2022 distribution, indicating that even the peak conditions do not correspond to extreme events.

The observed wind speeds during the survey (1.8–5.4 m s^{-1}) were not unusual for the region and are consistent with the regional wind climatology, characterized by synoptic-scale N–NE and S–SW winds (Tomazelli, 1993; Möller and Castaing, 1999; Machado and Calliari, 2016).

Together, these results demonstrate that the field campaign was conducted under conditions representative of the dominant outflow regime of the Patos Lagoon during winter.

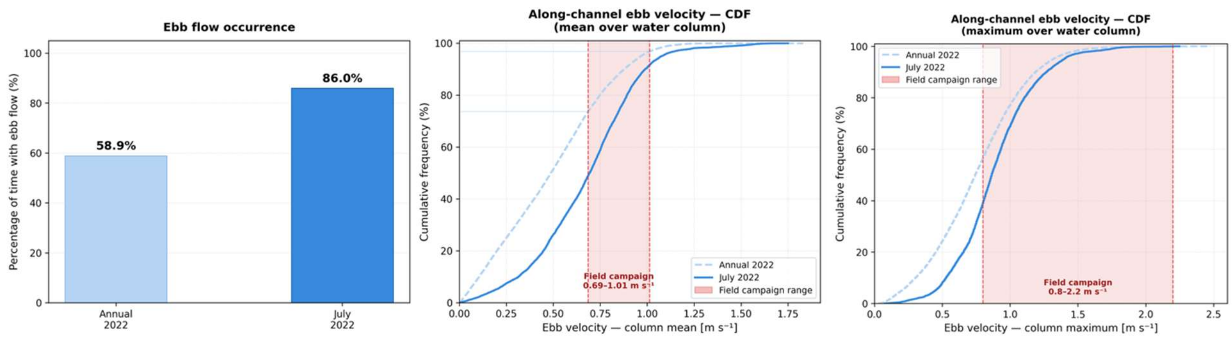

Figure R1: (Left) Outflow and inflow annual occurrences for 2022 and during July 2022. (Mid) CDF of depth-averaged along-channel ebb velocity (Right) CDF of maximum along-channel ebb velocity over the water column. These figures are only for reference and are not included in the manuscript.

The role of wind is mostly discussed in terms of inflow/outflow modulation. However, the manuscript does not sufficiently discuss the possible influence of wind on turbulence generation, vertical shear, or plume mixing. Is the wind effect totally negligible?

We agree that wind plays a fundamental role in the dynamics of the Patos Lagoon estuary and should not be regarded as negligible. In this system, persistent synoptic winds strongly influence water-level setup and set down, modulate current direction and intensity, regulate exchange through the inlet, and control the orientation, extent, and persistence of the coastal plume. Therefore, the hydrodynamic conditions sampled during our survey were themselves the result of wind forcing acting over the preceding days.

During the field campaign, however, local wind speeds were relatively weak (1.8–5.4 m s^{-1}), while strong seaward currents persisted throughout the water column. Consequently, although the large-scale circulation and plume structure were clearly conditioned by antecedent wind forcing, the direct contribution of the instantaneous local wind to turbulence generation during the measurements was likely limited.

It is also important to note that the microstructure profiler configuration used in this study does not resolve approximately the upper 2 m of the water column because the shear probes were oriented downward. As a result, the near-surface layer, where direct wind-induced turbulence would be expected to be most pronounced, was not sampled. Configuring the profiler to resolve the uppermost surface layer would necessarily compromise measurements in the near-bottom region.

We have clarified these points in the discussion of the revised manuscript (section 5.1) to emphasize that wind is a major control on the estuarine and plume dynamics and, together with river discharge, plays a key role in governing the background hydrodynamic state under which the observations were collected. Thus, we interpret wind as an essential control on the background hydrodynamic state and plume evolution, whereas the turbulence measurements presented here primarily reflect the resulting flow field rather than the instantaneous local wind forcing. Accordingly, our results should not be interpreted as indicating that wind-driven turbulence is negligible, but rather that its direct contribution could not be isolated under the sampled conditions and within the observational constraints of the present dataset.

R_i vs Re_b:

The manuscript introduces R_i and Re_b as central parameters controlling the interpretation of mixing. However, the conceptual distinction between the two parameters should be developed in more detail. At several points, Re_b appears to be treated as a superior replacement for R_i, but the underlying physical reasoning is not fully established.

Several regions of the estuary and plume exhibit R_i values indicative of stability while elevated Re_b values persist. This is one of the most interesting findings of the study and deserves a deeper physical discussion. Why does this discrepancy take place?

The manuscript argues that Re_b provides a more robust description of turbulent conditions than R_i. However, Re_b is substantially more difficult to estimate observationally because it requires direct turbulence measurements. The authors should discuss the practical implications of this result for studies lacking microstructure instrumentation.

The threshold values Re_b = 15 and Re_b = 200 play a central role in interpreting the results. However, these thresholds may be subject to limitations similar to those associated with the classical R_i = 0.25 criterion. Additional discussion of the origin, applicability, and uncertainty of these threshold values would strengthen the manuscript because several conclusions depend on them.

In the conclusions, the authors suggest that Re_b is more effective than R_i at identifying suppression of mixing under supercritical conditions. This is a very interesting conclusion, but it requires more development and support.

We agree that the conceptual distinction between the gradient Richardson number (Ri) and the buoyancy Reynolds number (Re_b) deserves a deeper and more articulated physical discussion. Our intention was not to present Re_b as a superior replacement for Ri, but rather to demonstrate how these two metrics complement each other by capturing different stages of the turbulent mixing process. To clarify this, we have revised Section 5.5 and the Conclusions.

Fundamentally, $Ri = N^2/S^2$ evaluates the instantaneous, local balance between stratification and shear, indicating whether conditions favor the initial growth of Kelvin-Helmholtz instabilities (KHI). In contrast, the dissipation rate (ε) and $Re_b = \varepsilon/(\nu N^2)$ represent the final, cumulative outcome of the energy cascade.

This conceptual difference helps explain the spatial and temporal disconnect observed in our data, where stable local Ri values ($Ri > 0.25$) coexist with elevated Re_b in energetic zones like the Patos Lagoon plume. This mismatch can be driven by three main mechanisms. First, there is an inherent time lag in the evolutionary cycle of turbulence: active mixing frequently persists in the water column long after the mean flow shear has relaxed and Ri has returned to a stable state. Second, spatial advection can be responsible for transporting turbulence generated upstream (where Ri was critically low) rapidly downstream, where microstructure profilers still record high ε and Re_b despite a locally stable density profile. Third, as recently emphasized by MacDonald and Goodman (2026), standard field instruments (such as ADCP and CTD) smooth data over vertical scales much larger than individual shear instabilities. This averaging can artificially yield stable Ri values, hiding unresolved sublayers that continue to sustain vigorous turbulence.

Regarding the practical implications for studies lacking microstructure instrumentation, we have expanded our text to highlight that Ri remains an invaluable, highly accessible proxy for standard surveys. Our findings serve as a calibration note for future work: in energetic systems, a high local Ri should not be strictly interpreted as an absolute absence of mixing, since advection and sub-grid dynamics can maintain significant mixing efficiency. Furthermore, we acknowledge the reviewer's point that the threshold values $Re_b = 15$ and $Re_b = 200$ carry statistical uncertainties and scale dependencies, much like the classical $Ri = 0.25$ criterion and we have revised the manuscript to clarify this.

Channel geometry and turbulence:

Section 5.2 and Figure 12 constitute a central component of the analysis by highlighting a potential relationship between channel morphology and TKE dissipation. However, the evidence currently presented does not appear sufficiently strong to establish a causal relationship. There is certainly a co-occurrence between the increase in cross-sectional area between km 4 and 6 and a reduction in TKE dissipation, but this alone does not demonstrate cause and effect. The strongest increase in TKE dissipation near the inlet does not appear to coincide exactly with the location of maximum lateral constriction. Dissipation remains relatively stable across the narrowest section and increases most strongly immediately downstream of the channel exit. Then, probably the observed enhancement of turbulence may be associated with plume lift-off (vertical contraction of the flow) rather than with lateral confinement imposed by the jetties. Are there additional observations supporting the influence of channel widening on the hydrodynamics? It may be useful to quantify this relationship using a correlation analysis, possibly after smoothing and normalizing both variables.

We agree that Figure 13 (formerly Figure 12 in the original manuscript) alone does not establish a strict causal relationship between channel geometry and turbulence generation. Our intention was to highlight a spatial correspondence suggesting that morphological transitions modulate the hydrodynamic response, rather than to claim that cross-sectional changes alone control TKE dissipation.

We also agree that the strongest enhancement of dissipation occurs immediately downstream of the narrowest section, at the estuary–shelf transition, where plume lift-off and rapid three-dimensional flow adjustment are expected to play a major role. We have therefore revised the discussion to clarify that the observed turbulence maximum likely results from the combined effects of channel confinement, flow acceleration, and plume adjustment processes, rather than from lateral constriction alone.

Accordingly, we have softened the causal language throughout the manuscript and now interpret the relationship between channel morphology and turbulence as one of modulation rather than as directly controlling the magnitude of TKE dissipation.

Lateral circulation:

The lateral circulation pattern is very interesting. A more detailed discussion of the mechanisms underlying this circulation and the possible implications for turbulence and mixing would improve the manuscript.

We agree that the lateral circulation deserves a more detailed discussion. In a previous study conducted in the same channel under stratified conditions (Barros et al., 2025), secondary circulation was identified as an important mechanism controlling shear production and turbulent mixing through the interaction of baroclinic pressure gradients, channel curvature, and bathymetric effects. In the present study, although stratification is largely absent within the channel, the cross-channel velocity field still exhibits coherent lateral circulation patterns (Fig. 7b). Interestingly, regions of reduced TKE dissipation between km 4 and 7 coincide not only with channel widening but also with weaker cross-channel velocity gradients, whereas the increase in ε toward the inlet occurs simultaneously with stronger lateral circulation. Therefore, we agree that the observed turbulence distribution likely reflects the combined influence of channel morphology and secondary circulation rather than geometry alone. We have expanded the discussion to acknowledge this mechanism and its potential contribution to shear production and mixing.

Technical comments

Lines 38-39: River plumes are not necessarily shallow features; this is only true for surface-advected plumes. Here, and later in Section 5.3, the manuscript would benefit from contextualizing the plume as a surface-advected plume. Relevant references include Yankovsky and Chapman (1997) and Yankovsky et al. (2022).

The introductory section describing river plume dynamics has been rewritten and streamlined in the revised manuscript to improve clarity and better frame the context of the present study.

Line 44: clarify that you are referring to the densimetric Froude number or the internal Froude number.

The text has been revised to explicitly specify that we are referring to the internal Froude number, thereby removing any ambiguity.

Lines 47–49: Here and elsewhere throughout the manuscript, several important statements are not supported by references. Additional citations to key previous studies would strengthen the scientific context.

The introduction was substantially rewritten and streamlined during the revision process, and the specific passage referred to in this comment was removed. In addition, the manuscript was carefully reviewed to ensure that key statements are appropriately supported by relevant references where necessary.

Lines 50–51: This sentence suggests that hydraulic jumps are a primary focus of the study, whereas they are only discussed as a possible interpretation. Consider rephrasing.

This portion of the introduction has been removed as part of a broader restructuring and streamlining of the manuscript. The revised introduction now focuses on the concepts most directly relevant to the objectives of the study, while expanding the discussion of key processes that required clearer contextualization.

Lines 56–57: The term "near-field" is more common in the plume literature. I recommend using it consistently throughout the manuscript.

We agree and have adopted the recommended terminology throughout the revised manuscript, using **"**near-field**"** consistently where appropriate.

Lines 65–67: River plumes are among the most extensively studied coastal phenomena. This statement should be revised. Perhaps the intended meaning is that direct turbulence measurements within river plumes remain relatively scarce.

We agree that river plumes have been extensively studied over the past decades. Our intention was to emphasize the relative scarcity of direct observational measurements of turbulence and mixing processes at high spatial and temporal resolution, particularly across the estuary–plume continuum. The text has been revised accordingly to avoid this ambiguity.

Lines 77–79: I am not entirely convinced that the manuscript explicitly addresses these research questions. Related results are presented, but the discussion does not return to these questions directly.

In response, we revised the research questions presented in the Introduction to better reflect the scope of the analyses and the principal findings discussed throughout the manuscript. The new research questions are as follows:

(1) How does channel morphology influence circulation, hydraulic adjustments (Fr_i), and the along-channel structure of velocity and density fields from the estuary to the buoyant plume?

(2) How does the interplay between shear production and buoyancy suppression modulate turbulence and mixing along the inlet and plume?

(3) What do the observed hydrodynamic and mixing characteristics reveal about the extent to which plume dynamics in microtidal systems differ from those described in mesotidal and macrotidal environments?

These revised questions are more closely aligned with the hydrodynamic interpretation developed in the Results and Discussion sections and emphasize the key contributions of the study regarding morphology, turbulence generation, and the distinct behavior of microtidal estuary–plume systems.

Lines 231–232: Why is the larger estimate removed rather than the smaller one? What percentage of the dataset was discarded under this criterion? In other words, how frequently did the two probes differ by more than two orders of magnitude?

The procedure follows the quality-control approach recommended by Stips (2005), in which, when the two independent shear probe estimates differ by more than a factor of two, the larger estimate is discarded as the more likely outlier resulting from probe contamination (one probe hitting a small piece of debris in the water, which artificially inflates the reading while the other probe records the clean background signal). We have clarified this point in the revised manuscript and noted that the threshold applied is a factor of two rather than two orders of magnitude. We also emphasize that only two complete vertical profiles (<1.3% of all casts) were discarded based on the operational quality-control criterion.

Lines 266–267: Please provide a more detailed explanation of Rf. Equations 4 and 5 would benefit from additional details regarding the physical meaning of the parameters, their typical ranges, and the interpretation of extreme values.

We agree that additional background on the flux Richardson number could be useful for a broader audience. In response to other reviewer comments, the Methods section was substantially streamlined to improve readability and avoid excessive methodological detail. However, we did add some additional context on the parameter Rf, indicating that it represents a ratio between buoyancy to production, typically bounding toward an upper limit of 0.25.

Verify whether Equation 8 uses U or ΔU. Additionally, define Δρ explicitly.

Equation 8 and the associated notation have been revised to ensure consistency with Figure 14.

Figure 5: Verify that the histogram bars correspond correctly to their associated axes. The bars appear slightly displaced relative to the data distribution in the central panel.

The histograms axis were adjusted.

Figure 6: I am not convinced that this figure provides information beyond what is already shown in Figure 4 or that it plays a significant role in the discussion. Consider removing it. The same for Figure 11.

Fig. 6 provides information that is complementary to Fig. 4 by presenting the complete evolution of the vertical density profiles along the estuary–shelf transition. This visualization is particularly important for interpreting the structural transformation of the water mass, providing the physical rationale behind the selection of the interface depth used in the two-layer decomposition. Specifically, it illustrates the transition from a well-defined, classical estuarine pycnocline within the channel (red profiles) to a structure where the main gradients migrate toward the near surface within the offshore plume region (blue profiles). This transformation directly justifies why a single mathematical criterion (such as the maximum vertical gradient) cannot be applied uniformly across the entire continuum, highlighting the necessity of the profile-shape/inflection method adopted for the shelf region. Moreover, the figure supports the discussion of the gradual transformation from the confined estuarine flow to the buoyant plume structure, which was further expanded in the revised manuscript. For these reasons, we believe that Fig. 6 contributes significantly to the physical to the interpretation of the results and should be retained.

The primary purpose of Fig. 11 is to support the arguments of Section 4.3 and provide a baseline reference for the ambient inner-shelf conditions adjacent to the plume. As stated in the manuscript, these measurements demonstrate that the comparatively weak velocities, stratification, and turbulence observed outside the plume contrast markedly with those measured within the Drift segment, thereby supporting the interpretation that the enhanced turbulent activity is an intrinsic feature of the estuary–plume system rather than a general characteristic of the surrounding shelf waters. To improve clarity, we have revised the section title and text to better emphasize this comparative purpose.

Section 4.3: The title "Jetties segment" is confusing. More generally, I don't think that this section is essential to the manuscript.

As discussed in our response regarding Fig. 11, we respectfully disagree with the reviewer. The primary purpose of this section is to provide a baseline reference for the adjacent inner shelf, allowing the plume observations to be interpreted in the context of ambient conditions. To improve clarity, we have revised the section title and text to better emphasize this comparative purpose.

Section 5.1: Some of this information appears methodological in nature and may be more appropriate in the Methods section.

Following the revision of the manuscript, this section has been refocused to emphasize the representativeness of the sampled conditions and the interpretation of the observations within the broader hydrodynamic regime of the Patos Lagoon, rather than methodological aspects of the sampling strategy. To further reflect this change, an additional figure was incorporated, and the subsection title was also revised. We therefore believe it is more appropriately presented as part of the Discussion.

Lines 587–588: better explain what is meant by the "stratification-dominated regime" described in Barros et al. (2025).

Following the substantial revision and reorganization of this section, the statement referring to a "stratification-dominated regime" has been removed. The discussion has been reformulated to more clearly emphasize the rapid transition between contrasting hydrodynamic states observed in consecutive surveys and its implications for turbulence and mixing in the Patos Lagoon estuary.

Lines 633–635: This explanation is difficult to follow. Consider rephrasing for clarity.

We agree with the reviewer that the original sentence was overly dense and difficult to follow. We have rephrased this section to decouple the physical mechanism from its consequences, improving clarity and flow.

Line 636: The concept of the near-field plume is not applicable within the laterally confined estuary.

The concept of a near-field plume is indeed strictly applicable once the flow exits the laterally confined estuary. To avoid any confusion, we have removed the explicit reference to the internal channel kilometers (km 0 to 13) and rephrased the sentence to clarify that the near-field classification refers strictly to the unconfined plume domain sampled offshore.

Lines 704–706: How do you reconcile the interpretation that mixing is partially suppressed by stratification with the statement that "mixing efficiency is maximized"? What is meant by "pycnocline erosion"?

Our intention was not to imply that turbulence is strongest within the transitional regime, but rather that turbulence remains sufficiently active to promote efficient mixing despite the presence of stratification. To avoid ambiguity, the text has been revised to clarify the distinction between turbulence intensity and mixing efficiency. We also clarified that "pycnocline erosion" refers to the progressive weakening of the density gradient through turbulent entrainment and mixing across the interface.

Lines 740–741: The statement that the spatial evolution of turbulence is consistent with the sustained supercritical regime requires further explanation. How are these processes linked?

We agree that the linkage between the spatial evolution of turbulence and the sustained supercritical regime was not sufficiently explicit. The conclusion has been revised to clarify that the persistence of supercritical flow reflects a high-momentum state that maintains elevated shear and delays hydraulic adjustment, thereby helping to sustain enhanced turbulence until progressive plume expansion and entrainment drive the transition toward subcritical conditions.