

Rio Grande, July 13th, 2026

To Dr. Anne Marie Treguier

Editor of Ocean Sciences

Dear Dr. Anne,

On behalf of my co-authors, I am pleased to submit the revised version of the manuscript, "Turbulence and Mixing Along a Microtidal and Stratified Estuary–Shelf Transition," for consideration for publication in Ocean Science.

This study builds on our ongoing efforts to improve the understanding of hydrodynamics and mixing processes in highly stratified estuary–shelf transition zones through field observations. We believe that the findings provide a valuable contribution to current research in coastal physical oceanography and will be of broad interest to the readership of Ocean Science.

We sincerely appreciate the thoughtful and constructive comments provided by Prof. Daniel MacDonald and Prof. Oscar Alvarez. Their suggestions have helped us improve the quality and clarity of the manuscript. We have carefully addressed all comments, and a detailed point-by-point response is provided below.

Sincerely,

Carlos A.F. Schettini
Professor
Federal University of Rio Grande
Rio Grande, Brazil

This paper describes the results of a recent observational campaign at the mouth of the Patos Lagoon in southern Brazil, describing measurements of density, velocity and turbulence through the outlet and into the adjacent coastal shelf region. The manuscript reads very much like the combination of a thesis, in that there is sometimes too much fundamental detail, and a technical report, in that the majority of the paper is a presentation of the data. In this regard, the paper is lacking specific hypotheses or dynamical interpretation that is novel. However, it does represent a valuable contribution, and effectively illustrates that, for the most part, the outlets of microtidal systems behave similarly to those of meso- and macro-tidal environments. While this is not surprising, given that the essential structure of the outflow is driven by flow dynamics at shorter temporal scales that are relatively agnostic to the larger scale mechanisms setting up the overlying pressure gradients that drive the flow (such as tides, wind, etc.) data from a microtidal system to illustrate this is valuable.

Overall, I would recommend that the manuscript could be strengthened by focusing even more on this aspect, streamlining much of the more general introductory discussion regarding plumes and turbulence, and providing a more focused introduction on the differences between microtidal and other systems and why these systems may or may not be fundamentally different from most of the previously studied plumes. In my opinion, this is a more valuable focus than the “morphological nozzle” perspective. In this regard it is always valuable to have data from a wider variety of plume systems available in the literature, and the data collection and presentation is valuable.

We sincerely thank the reviewer for the careful assessment of our manuscript and for recognizing the value of this dataset as a contribution to the understanding of microtidal estuarine systems.

We appreciate the reviewer's observation that the manuscript, in its original form, placed considerable emphasis on the presentation of observations and could benefit from a clearer statement of its objectives, hypotheses, and dynamical interpretation. In response, we revised the Introduction and Discussion sections to more explicitly state the study's overall goals, research questions, main take-home messages, and the broader physical implications of our findings. We also streamlined portions of the Introduction to remove some unnecessary background information and provide a clearer discussion of the

similarities and differences between microtidal and previously studied meso- and macrotidal systems.

In addition, following the reviewer's recommendation to better emphasize the broader significance of the dataset, we have clarified two major observational gaps addressed by this study directly within the Introduction (Section 1, lines 52 to 70) and reinforced their implications in the Discussion (Section 5.4, lines 734 to 743).

First, as now explicitly framed in the Introduction, most historical studies quantifying turbulent kinetic energy dissipation rates (ϵ) have focused on energetic mesotidal and macrotidal systems, leaving microtidal estuaries comparatively understudied from a microstructure perspective. By providing a robust, high-resolution turbulence dataset, this study contributes a valuable benchmark for turbulence and mixing processes in low-tidal-energy environments (contextualized in Section 5.4, Table 2).

Second, existing turbulence observations are often spatially fragmented. Most studies focus either on turbulence within estuarine channels (e.g., Peters and Bokhorst, 2000; Ross et al., 2019a; Tiede et al., 2025) or on specific regions of river plumes and estuarine fronts (e.g., Kilcher et al., 2012; McPherson et al., 2019, 2020; Spicer et al., 2022). Only a limited number of studies have attempted to link estuarine mixing processes to the resulting plume structure (e.g., MacDonald and Geyer, 2004; Nash et al., 2009). Nevertheless, high-resolution observations spanning the estuarine channel, inlet, and adjacent plume as a connected system remain limited. The present study seeks to address this gap by providing a detailed characterization of the estuary–plume continuum in a microtidal system.

We are grateful for the reviewer's recognition that observations from a microtidal system provide a valuable addition to the existing body of knowledge. We agree that expanding the diversity of plume systems represented in the literature is essential for evaluating the extent to which conclusions derived from meso- and macrotidal environments can be generalized to meteorologically forced microtidal systems.

Some more specific comments are included below:

Section 1 (roughly lines 60-80): These paragraphs provide an introduction to the microtidal system, but a broader and more detailed exploration of how these systems are different from meso and macro-tidal systems would be valuable, including why we might (or might not) expect the outflow dynamics to be different? Expanding on the time scale of flow reversal in

the lagoon-channel-shelf system would be valuable (meteorological scales of days vs tidal scales of hours) and how this affects the system. Presumably, there is more time to allow for dynamics to come to a quasi steady-state. Also, the nature of the receiving waters on the shelf might change (?), particularly since the ambient is not getting refreshed by the tides on a 12 hour cycle. Focusing more on these aspects of the study may strengthen the contribution.

We have expanded both the Introduction and the Study Area sections to emphasize that, unlike strongly tidal estuaries where flow reversals occur at semidiurnal timescales, circulation in the Patos Lagoon is primarily controlled by meteorological forcing over periods of several days. This behavior is illustrated in Figure 3 and is further supported by the sea-level and current-velocity spectral analyses presented by Barros et al. (2025). However, we must clarify that this behavior is not an intrinsic feature of *all* microtidal estuaries, but rather a characteristic of discharge- and wind-dominated microtidal systems, such as Patos Lagoon. While microtidal classification strictly implies a small tidal range (e.g., Hayes, 1975), the near-complete suppression of high-frequency tidal currents in our system allows these meteorological and riverine drivers to completely dominate the hydrodynamic scales.

The vast surface area of the lagoon (~ 250 km long) acts as a low-pass filter that dampens ocean tides and river discharge pulses as well, while its alignment with the dominant synoptic wind fields (NE-SW) allows meteorological forcing to dominate the barotropic pressure gradients over multi-day periods. As a result, outflow conditions may persist for considerably longer intervals without the periodic reversals imposed by tides, allowing the system to approach quasi-steady conditions more frequently.

We have also clarified that, despite the absence of regular tidal reversals, circulation patterns can still change rapidly in response to variations in wind forcing and freshwater discharge, often on timescales of hours to days.

This characteristic reinforces the value of the Patos Lagoon as a natural laboratory for investigating plume dynamics and turbulence under sustained forcing conditions. By morphologically filtering out the complex, high-frequency tidal reversals that typically complicate fieldwork on a 12-hour cycle, this system allows us to isolate and investigate wind and river forcing under sustained, quasi-steady conditions. These aspects are now discussed more explicitly in the revised manuscript, together with their implications for plume evolution and mixing processes in microtidal environments.

Section 3 (general): This section reads much like a thesis, particularly in the data analysis section, where much of the material presented would be familiar to most readers, and definitions could be presented in a more streamlined manner.

We appreciate the reviewer's comment and agree that some methodological definitions presented in the data analysis subsection may be familiar to readers with expertise in turbulence and plume dynamics.

At the same time, because the processing and interpretation of microstructure measurements involve specialized analytical procedures and multiple methodological choices, we believe that retaining a sufficient level of detail is important to ensure transparency and reproducibility of the analyses. Our intention was also to provide a clear, step-by-step description of the workflow followed throughout the study.

Nevertheless, in response to the reviewer's suggestion, we revised Section 3 and improved its conciseness and readability where possible, while preserving the methodological information necessary for reproducibility.

Section 3.2.4 (lines ~300): Defining the interface depth for the calculation of Fr_i is a notoriously difficult process that is rarely, if ever, accomplished in an effective manner in the literature. In this case, the authors use two definitions for inside and outside of the channel, which may be warranted, but presents difficulties in interpretation. Overall, the general trends of Fr_i are probably more valuable than the actual values (discussed more below in the context of Figure 13). I would suggest providing more context to this discussion, including how much different definitions of the interface depth affect the results.

We agree that defining the interface depth for internal Froude number calculations is inherently challenging and can substantially influence the resulting values. To clarify our methodology, we expanded Section 3.2.4 and provided additional justification for the use of different interface definitions within the channel and in the offshore plume region. We believe this clarification may also benefit future studies addressing the estuary–plume continuum, where a single interface-identification criterion may not be appropriate across all environments.

Within the channel, the density structure is characterized by a well-defined pycnocline, allowing the interface depth to be objectively identified as the depth of maximum vertical

density gradient. In contrast, offshore density profiles exhibit a different structure, where the strongest density gradients often occur within the near-surface plume rather than at its base. Applying the same criterion in this region resulted in unrealistically shallow interface depths that did not represent the thickness of the buoyant plume relevant to the hydraulic analysis.

Therefore, for offshore profiles, the interface depth was defined using a profile-shape criterion based on the transition between the strongly stratified plume layer and the nearly homogeneous coastal water below. The inflection point of each density profile identifies the transition where the sharp density gradient of the surface plume gives way to a nearly uniform deeper layer, as shown by the offshore density profiles in Figure 6. This approach was adopted to consistently identify the base of the buoyant plume across the channel-to-shelf transition. We have revised the manuscript to better explain the physical rationale behind this choice and to emphasize that the interpretation of Fr_i focuses primarily on the spatial patterns and relative variability rather than on individual values.

Line 341: Please clarify if the gauged river discharge is at a section that is completely fresh.

The gauged discharge used in this study corresponds to river flow measurements obtained from upstream gauging stations located in the main tributaries of the Patos Lagoon drainage basin. Therefore, the measurements are collected in fully freshwater environments and represent freshwater inflow to the lagoon. We have revised the text to clarify this point.

Figure 2: A key aspect of the microtidal system appears to be longer time scales to allow for the flow dynamics to reach a more consistent steady state. This is shown in figure 2 to some extent, but could be emphasized more in the text, particularly the paragraph at lines 350-360. These issues are addressed somewhat later (lines 550-555) but it would be a benefit to emphasize this earlier.

We agree that one of the key characteristics of this microtidal system is the potential for sustained forcing conditions to persist over timescales longer than a tidal cycle, allowing the circulation and plume structure to approach a quasi-steady state. We have revised the text associated with Figure 2 to emphasize that the elevated river discharge, weak water-level variability, and persistent ebb-directed flow observed during the days surrounding the survey indicate a prolonged outflow-dominated condition rather than a transient event. This

clarification strengthens the connection between the environmental forcing shown in Figure 2 and the later discussion regarding the quasi-synoptic character of the observations.

Figure 4 (and related discussion): These panels illustrate the challenges of defining the interfacial layer. Clearly, in the channel, there is a weak layer separation. But both layers seen in the channel, “lift off” at the mouth. So is the channel really a two layer system, or a weakly stratified single layer?

We agree that the separation between layers within the channel is less pronounced than in the offshore plume region, where the buoyant surface layer clearly detaches from the channel and forms a distinct plume. However, we do not interpret the channel as a weakly stratified single-layer system.

Within the channel, a persistent vertical salinity and density structure is observed, with salinity varying from approximately 7 to 13 g kg⁻¹ between surface and bottom waters. This stratification is more evident in the T-S distribution (Figure 5), which reveals distinguishable upper and lower water masses with two distinguishable branches within the low-salinity estuarine waters, corresponding to surface and deeper channel waters. While the transition between these water masses is more gradual than in the offshore plume, the observed density structure is consistent with a stratified two-layer system rather than a vertically homogeneous water column.

We have revised the manuscript to clarify that the channel is characterized by persistent stratification and distinguishable upper and lower layers, whereas the interface becomes considerably sharper after plume detachment at the mouth.

Section 4: In general, the presentation of collected data is good, however, it may not be necessary to show as much detail, which might allow the section to be streamlined. For example, epsilon might be sufficient – showing eddy viscosity is somewhat redundant and does not necessarily add to the narrative. In Figure 9, it may not be necessary to show all the components of Ri independently.

We agree that streamlining the presentation is generally desirable; however, we believe that the retained variables provide complementary information that is important for the interpretation of the observed mixing dynamics.

Regarding eddy viscosity (A_z), although ε received greater emphasis in the discussion, we believe that A_z provides complementary information that warrants its inclusion. Beyond its role as a proxy for turbulent mixing, A_z exhibited spatial variability spanning approximately six orders of magnitude over less than 14 km. This remarkable variability represents a key result of the study rather than a redundant diagnostic variable. We believe these observations are particularly relevant because estuarine characterization, conceptual frameworks, and some modelling approaches often rely on representative mixing parameters to describe system-scale behavior. The variability observed here highlights the challenges associated with selecting a single characteristic A_z value to represent an entire estuary–plume system. To better reflect this point, we revised the manuscript discussion (section 5.4) to further emphasize the implications of the observed A_z variability.

Regarding the Richardson number components, we elected to retain both N^2 and S^2 because the interpretation of mixing processes in this study relies fundamentally on the balance between buoyancy suppression and shear production. While R_i provides a useful integrated measure of flow stability, its value alone does not reveal whether changes in stability arise from variations in stratification, shear, or both. Examining the individual components therefore provides direct insight into the physical mechanisms controlling the observed mixing patterns. The separate presentation of N^2 and S^2 is particularly important because the present study reveals a markedly different balance between stratification and shear than that observed by Barros et al. (2025), despite both datasets having been collected in the same estuarine system.

Section 5.3: The discussion of Froude number is biased significantly by the choice of interface depth, but the trends in the Froude number are probably more valuable than the actual values. Most theory (see Farmer and Armi 1986, Armi and Farmer 1986, and many other later papers) would suggest that $Fr \sim 1$ at the liftoff location, but this is not seen in Figure 13. Perhaps other definitions of h might recalibrate the values to be consistent with theory? Also, given the dramatic difference between the layer composition inside and outside the mouth, it is slightly misleading to plot Fr as a continuous progression. This section could be strengthened by acknowledging some of these issues. In general, the trend of Fr , particularly outside the mouth is consistent with earlier studies of near-field plumes, and also with the profile of epsilon shown in Figure 12 (perhaps figures 12 and 13 should be combined in some way).

We agree that the absolute values of Fr_i are sensitive to the adopted interface depth and have revised the manuscript to more explicitly acknowledge this limitation. The discussion

now emphasizes the spatial evolution and relative trends of Fr_i rather than its absolute values. We also agree that caution is required when interpreting Fr_i across the estuary–plume transition because different interface definitions were adopted inside and outside the mouth to account for distinct density structures. The manuscript has been revised to clarify that Fr_i is presented continuously for visualization purposes, but should not be interpreted as a strictly continuous quantity across regions characterized by different layer structures.

Regarding the expectation of $Fr_i \approx 1$ at the lift-off location, we acknowledge that idealized hydraulic-control theories frequently associate dynamical transitions with critical conditions. However, we note that in natural river-plume systems the location of plume lift-off does not necessarily coincide with the hydraulic transition. Consistent with conceptual models proposed by Hetland (2005) and Horner-Devine et al. (2015), our interpretation is that the sampled plume remained largely within the near-field region, where supercritical conditions persist and hydraulic adjustment occurs progressively offshore as entrainment, lateral spreading, and momentum dissipation reduce the internal Froude number.

Finally, we appreciate the suggestion regarding Figures 13 and 14 (formerly Figure 12 and 13 in the original manuscript). While the longitudinal evolution of Fr_i and ε exhibit notable similarities, we elected to retain the figures separately to preserve readability and avoid overloading a single figure with multiple variables. However, we revised the discussion to more explicitly highlight the spatial correspondence between elevated ε values and supercritical flow conditions, reinforcing the interpretation that intense shear-driven mixing develops within the near-field plume even as hydraulic adjustment progresses offshore.

Lines 630-635: Note that internal hydraulic jumps are rarely seen in plume observations, but Froude numbers more often reduce gradually over a significant spatial scale as the plume spreads and deepens. This is discussed in this paragraph, but the emphasis on hydraulic jumps (or their absence) may be overstated.

We agree that hydraulic adjustment in natural plume systems is often expressed as a gradual reduction in Fr_i associated with plume spreading, entrainment, and interface deepening rather than as a discrete internal hydraulic jump. In response, we revised the discussion to reduce the emphasis on hydraulic jumps and instead focus on the progressive hydraulic adjustment observed along the estuary–plume transition. This interpretation is more consistent with both the observed evolution of Fr_i and the conceptual framework of river-plume development described in previous studies.

Lines 644-648: An exact starting point for the mid-field is hard to define, but it could be argued that the mid-field may start around 11-12 km as Fr is on a downward trend and the interface depth begins to decrease, both suggesting that the intensely energetic near field/lift-off region is beginning to weaken.

We agree that the onset of the mid-field region is not sharply defined and that plume evolution is better viewed as a gradual transition rather than as a discrete boundary. While Fr_i remained above unity throughout the sampled domain, we acknowledge that the progressive decrease in Fr_i beyond approximately km 11–12, together with changes in interface depth and velocity structure, may indicate the beginning of hydraulic adjustment and the early stages of the transition toward the mid-field region. The manuscript has been revised accordingly to reflect a more gradual interpretation of plume evolution while maintaining our conclusion that fully developed subcritical conditions were not observed within the surveyed area.

Table 2: While the list of references is valuable, I am not sure that the differences in epsilon are significant enough to warrant a table (often the difference between 10^{-3} and 10^{-4} might be somewhat subjective). Note also that the reference to Spicer (2022) is more closely associated with the Merrimack than the Columbia River, as indicated in the table.

We agree that differences of one order of magnitude in reported maximum dissipation rates should be interpreted with caution, given the diversity of environmental settings, forcing conditions, observational strategies, and methodologies among studies.

The purpose of Table 2 was not to establish a strict comparison or ranking among estuarine systems, but rather to provide a broad contextual reference allowing readers to place the energetic conditions observed in the Patos Lagoon Estuary within the range of values reported in the literature. To avoid overinterpretation, we simplified the table by retaining only a reduced set of representative estuarine systems and revised the accompanying text to emphasize its illustrative character. The revised table is therefore intended solely as a qualitative benchmark showing that, despite its microtidal nature, the Patos Lagoon Estuary can exhibit dissipation rates comparable to those reported for considerably more energetic mesotidal and macrotidal environments.

We also expanded the discussion to emphasize that the value of this comparison lies not in establishing quantitative differences among systems, but in highlighting a broader physical implication of the results. Specifically, our observations suggest that microtidal estuary–

plume systems may achieve turbulence levels comparable to those reported in energetic tidal environments through fundamentally different forcing mechanisms, driven primarily by sustained river discharge, persistent outflow, and topographic constriction rather than tidal forcing.

Lines 715-720: The discussion about R_i dependence is valuable. It should be noted that the scale over which the critical value of $\frac{1}{4}$ is valid may be quite small, and below the resolution of your data set. Thus, your values of R_i may be more of a bulk Richardson number, for which turbulence can be generated and sustained at values higher than $\frac{1}{4}$. We recently published a paper on this topic which you might find interesting:

MacDonald, D.G., and L. Goodman, 2026. Defining an appropriate range of scales for application of the gradient Richardson number, with implications for observations of stratified shear turbulence at laboratory and ocean scales. *Frontiers in Marine Science*. 13:1758561. doi: 10.3389/fmars.2026.1758561

We thank the reviewer for bringing this recent study to our attention. We found the discussion particularly relevant to the interpretation of our Richardson number estimates.

Following the reviewer's suggestion, we have revised the manuscript (Section 5.5) to acknowledge that the classical critical value of $R_i = 0.25$ is strictly applicable when the Richardson number is evaluated over sufficiently small vertical scales. As discussed by MacDonald and Goodman (2026), field observations commonly rely on finite-resolution measurements that are often more representative of a bulk Richardson number than a true gradient Richardson number, such that turbulence may persist at R_i values substantially greater than 0.25 because unresolved sublayers can locally satisfy instability criteria.

In our dataset, this perspective is supported by the independent buoyancy Reynolds number (Re_b) and TKE dissipation (ϵ) estimates, which frequently indicate vigorous turbulence in regions where R_i exceeds the classical threshold. This perspective is useful for interpreting the coexistence of elevated R_i with high ϵ and Re_b values observed in the present study.

The manuscript has been revised accordingly (discussion section 5.5) and now explicitly acknowledges the scale dependence of Richardson number estimates and the limitations of applying a universal critical threshold to field observations.

Overall, I think the data set is strong and provides a valuable contribution as a reference point for micro-tidal outflows. I recommend that the manuscript be strengthened by streamlining the paper (particularly the introduction and discussion sections) and focusing primarily on the micro-tidal aspect of the results, which is the most novel.

Dan MacDonald

We thank the reviewer for this positive assessment of the manuscript and for recognizing the value of the dataset. We agree that the microtidal character of the system represents one of the most novel aspects of the study. Following this suggestion, we revised the Introduction, Discussion, and Conclusions and we believe these revisions improve the focus of the manuscript while reinforcing its contribution as a reference study for high-discharge microtidal estuary–plume systems.

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 ($1.8\text{--}5.4\text{ m s}^{-1}$) 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.

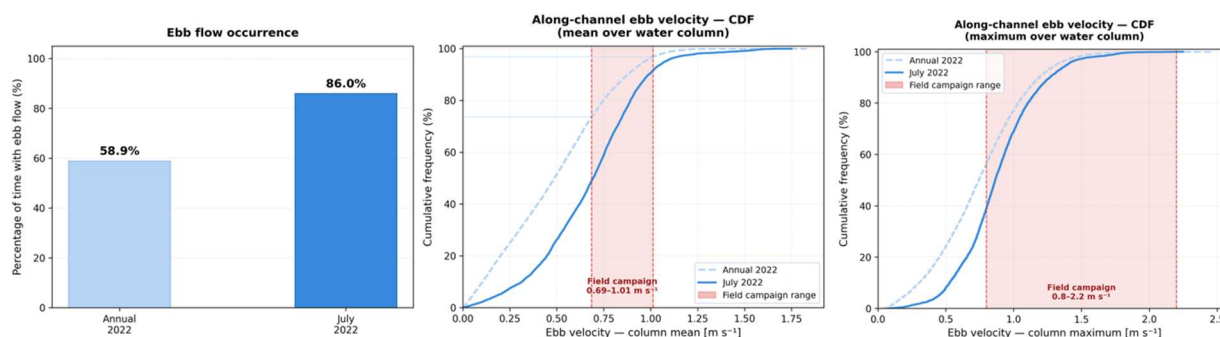


Figure R1: (Left) Outflow and inflow annual occurrences for 2022 and during July 2022. (Mid) CDF of depth-averaged along-channel ebb velocity (Right) CDF of maximum along-channel ebb velocity over the water column. These figures are only for reference and are not included in the manuscript.

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 ($1.8\text{--}5.4\text{ m s}^{-1}$), 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 $Ri = 0.25$ 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 Ri 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_l), 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 R_f . 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 R_f , 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 Δp 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.