Answers to Reviewer #1: Daniel MacDonald

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 (meterological 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 Froud 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 ~ 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 Ri dependence is valuable. It should be noted that the scale over which the critical value of ¼ is valid may be quite small, and below the resolution of your data set. Thus, your values of Ri may be more of a bulk Richardson number, for which turbulence can be generated and sustained at values higher than ¼. 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 strengthed 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.