Title: Flow Structure and Mixing Near a Small River Plume Front: Winyah Bay, SC, USA

Author(s): Papageorgiou et al.

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Once more we appreciate the time reviewer Spicer spent on this manuscript and the constructive comments provided. These have been considered and fully incorporated in the revised manuscript.

Responses to each comment are presented below (in blue fonts) while the file of the manuscript with the tracking on provides more details on where the changes have been carried out.

We hope that this revised version is acceptable for publication in EGU Oceans.

Sincerely

George Voulgaris

Authors Response to Reviewer Comments

Major comments:

1. I still struggle to follow the mixing aspect of this paper and what story is being presented there. There still does not seem to be a clear message and conflicting analyses are presented (i.e., the mixing efficiency) but never discussed or reconciled except for a few broad sentences at the very end of the conclusions (which is not a good place for it). Identifying mixing mechanisms is an objective of the paper, but I feel accomplishing this objective is muddled in the text. It is there…it just could be clearer.

We have clarified the objective and findings of the mixing efficiency story. We agree with the reviewer that the mixing data are conflicting. In the revised manuscript the conflict is reconciled through the concept of the layered turbulence in highly stratified flows as presented in the review paper of Caulfield (2021). Under highly stratified flows, like in our data, turbulence can be high in certain regions with weak stratification while other regions of the flow are characterized by low intensity turbulence but higher stratification. However, the limitation of resolving the spatial distribution of TKE dissipation rates due to the averaging methods used does not allow this layering to be identified while the high dissipation rates of the high turbulence regions dominate the recorded total signal. The concept described above explains the reduced mixing found within the plume and the higher dissipation rates. We present the results of a standard parameterization used in modeling to demonstrate the differences and this estimation has been moved after the explanation / discussion of the observed data and the purpose of these estimates is clearly stated now.

2. Tidal straining is mentioned in the introduction and conclusions, but there does not seem to be any analysis on the mechanism in the rest of the paper. Either do not mention tidal straining or add a much more detailed analysis of the process.

In the revised paper it has been clarified that tidal straining presents macro-scale conditions like what we encounter, but as per its definition the terms describes the driving force but not the mixing mechanism per se. It is argued that the mechanism is the same as explained above (layered turbulence) and straining is the driver that creates the highly stratified conditions. We have also added some text explaining the concept of tidal straining.

Minor comments (by line number):

13	In the text "…a return was flow developed…", the "was" should be removed. Done
55	It would be helpful to expand a bit on the concept of tidal straining here. Even just a few more sentences describing the basics of it. Done as suggested
142-143	Looks like a few lines repeat here. Corrected
Fig. 1	Looks much better with bathymetry We agree thanks for the suggestion
145-158	Were other MicroCTD quality control flags utilized besides the terminal velocity? How about instrument inclination? What was the range of speeds allowed around the terminal velocity? See Spicer et. al (2023) (Evolving Interior Mixing Regimes in a Tidal River Plume) Supporting Information for things I am looking for. I am sorry to nit-pick. But since individual profiles are being used and not being averaged together, data post-processing is important for this study.

The concerns expressed are reasonable, and we appreciate the desire to be clear about how the data were quality controlled. Additional information on the screening process has been provided, only one data point had to be removed due to high angle of attack or high vibration (likely due to interference with the line from the weight), and no pair of dissipation estimates from the shear probes differed by more than one order of magnitude, so no points were removed due to those criteria. The minimum speed allowed was 0.5 m/s, and maximum vertical velocities were 0.7 m/s. Given the strong density changes, some variability is to be expected.

The general agreement between the microCTD and ADCP dissipation estimates is further confirmation that our values are reasonable. As an additional check, a downward-profiling VMP 250 was deployed from the vessel simultaneously with the microCTD. These values are affected by the ship-generated turbulence in the surface layer, but show good agreement at depth (results presented in Papageorgiou 2023). I'm including the figure here showing the comparison for the relevant stations, much of the near-surface VMP data had to be removed due to high angle of attack and/or vibration, and there is likely still contamination due to the ship, causing some of the bias down to 6 m. Red is VMP and black is microCTD.

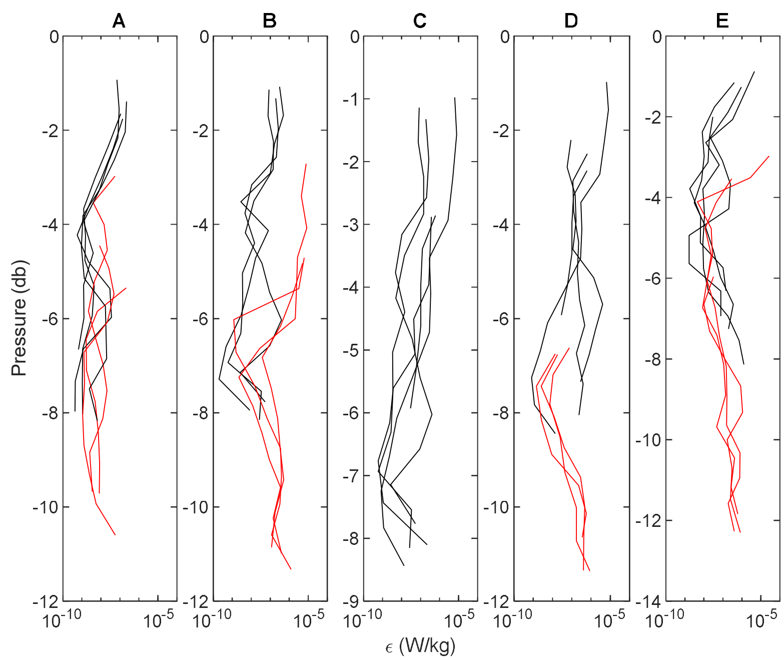

Fig. 2 The caption for panel (c) is written in kind of a confusing way. Tidal elevation is black line. Solid red is recorded discharged and dashed red is tidally corrected discharge…correct?

It is fixed by adding colors to caption description

Fig. 6 Looks much better. To really make it easy, you could label direction directly on the figures or colorbar: i.e., blue on top panel = traveling with front, blue in second panel = towards coast, etc.

Also, directly label which panel corresponds to u' and which is v'. Having both variables on the colorbar makes it less clear. I understand it is in the caption, but make it clearer so readers do not necessarily need to read the caption to know.

The figure has been modified as suggested.

253 You mean across-front here, right?

Yes, it has been clarified in the text

270-274 You are saying the Garvine model explains these vertical current patterns? Or that frontal convergence in general drives vertical currents? Clarify….these lines are a little opaque.

The Garvine model refers to the shallowing of the base of the front and not on the flow convergence / divergence. This is argued to be a secondary effect because of that shallowing. If the base of the plume was horizontal (no shallowing) then du/dx at the z of the base will be zero. It should be noted that in the revised paper the convergence has been revised; it is divergence. This has been updated in the text and discussion of the vertical flows is discussed accordingly.

281 You are not showing divergence on these plots. If you are going to make claims about where diferent types of divergence are correlated, you should plot it. You could mark lines where dw/dz = 0 and du/dx = 0. The addition of divergence is good but it seems half-explained at the moment.

We have no information on du/dx this is inferred from the plot of du/dt at the z where u'(z) = 0. We have made lines where u'(z)=0 and the elevation of this line appears to be deeper than the w divergence line. The latter is easily identified by the white contour in the plot where w=0.

Fig. 8 Label the map panel (c) or (d). It is the only subplot without a label.

Done as suggested

335-336 Looking at Fig. 9, there doesn't seem to really be a difference in shear at depth before and after front passage. It is a bit strange that dissipation is increasing.

While the shear on the scale of these figures is relatively low, stratification is also quite low, and so it may not take much shear to de-stabilize the lower part of the water column, where tidal currents (lower amplitude) are dominating the velocity signal. Furthermore, the concept of the turbulence layer invoked later on might explain this discrepancy,

345 You are not calculating a correlation coefficient, so you should not say there is a correlation.

Although correlation does not mean correlation coefficient, for clarity "correlation" was replaced with "relationship".

359 "Extend" should be "extent".

Fixed, thank you!

Table 2 Caption…specify each variable and it's long name to reminder readers and provide a thorough description of the table.

Done

Also…why are units cm/s here and m/s everywhere else?

This is due to the small magnitudes of the numbers we could replace if the editorial staff thinks that this is worth it, otherwise we would prefer to keep it as is.

386-393 Provide more information on why you are presenting these nondimensional numbers.

In the text we note that this is to provide information on the dynamics of the plume not only at the front but also behind it.

Also, for the Froude number varying with time in Fig. 11, is the layer depth varying with time as well? Or just velocity? You only mention an average Froude number in the text which is why I ask.

Yes the, estimation considers the layer depth too, as per the equation presented in the text. That's why the two lines do not mirror each other.

Make sure to mention the range in values shown in your Fig. 11.

A mention is made in the text that the Fr values are consistently above 1. The exact values are easily read on the plot itself.

Similar point for frontal Reynolds number. Also, why is 0.36 m/s used for Uf for that number while it appears Fr is calculated with Uf = 0.61 m/s?

Using D=2.2m g'=0.034 m/s2 and Uf=0.36 m/s we get 1.316 ~ 1.32. If Uf=0.60m had been used then the value would have been 2.23. Uf=0.36 m/s is the relative velocity of the entire plume layer, rather than just the near-surface speed, which is faster (as shown in Fig. 11).

Is "U_s" in Fig. 11 the same as "U_f" in the text?

No U_s is the absolute surface current / speed This is defined in the text now as well

397 Where does 0.33 m/s come from?

This is an averaged plume velocity over 2 hours after the front arrival. This has been clarified in the text now.

401 There is a stray "t" before "LW".

Fixed

443 You have 4N^2 plotted, not 4S^2.

Thank you for catching this, the figure is correct but the text had an incorrect formula, with N and S flipped in the gradient Richardson number. This has now been corrected in the text.

446 Label each subplot by group number in Fig. 13.

We do not think this is necessary. The subplots of the lower raw have the groups that correspond to the subplot on the top row

446-457 This paragraph could be streamlined to just focus on most important patterns. It is a little confusing to read right now.

The paragraph has been re-written to focus on the most important points regarding trends in buoyancy and shear.

458 It is not too surprising that shear exceeds stratification in the bottom layer….there is no plume there. This is pretty common.

This has now been acknowledged in the text.

458-467 After reading this paragraph, you could easily remove the paragraph directly before this.

The paragraph has been re-written and we elected to leave it in the paper.

481-499 This is good…my only comment is you mention "Smith (2020)" in most sentences. Probably not necessary and would read more natural if not mentioning the reference so much.

This has been addressed as suggested by the reviewer.

Eq. 6 Please explain physically what the turbulence potential energy (TPE) is and why it is compared to TKE in Fig. 15.

A definition / what is the TPE has been added following earlier on, after eq 2 where TPE appears for the first time. As shown in there this is the "theoretical" definition of the mixing efficiency (i.e., kinetic energy required to destroy the potential energy the stratified flow processes). A sentence reminding the reader that the ratio of TKE dissipation rate over TPE dissipation rate is used to defining mixing efficiency has been added in the paragraph describing Fig 15 too.

Fig. 14 The y-axis tick labels are missing from the bottom plots.

Not sure more ticks are needed, in addition the grid-lines clearly show the different tick levels.

525-530 Mixing eficiencies calculated by the TPE to TKE ratio (Table 3) obviously differ from those determined by the flux Richardson number (Fig. 13). In fact, they present nearly opposite messages about where mixing is important. I do not see any text here reconciling that. This brings up a few questions:

Why is mixing eficiency calculated in multiple ways? (this is never said).

This has been addressed now (see our response in major comments). The one is parametrization the other is based on overturning scales. We believe the latter one is more appropriate and we have explained our rationale in the revised paper.

Which is correct?

See above

Does comparison between the methods provide insight into mechanisms?

As explained in the revised text the reason for the parameterized values is to demonstrate the contradictory results someone would obtain using this method.

533 Typo in this line. “we have not estimates of” needs to be reworded.

Fixed

549 Typo: “e_k was of the order of”?

Fixed

555 Not according to Fig. 13.

This part of the manuscript has been re-written as explained in our response to the major comments above.

542-555 There are a handful of grammatical errors in this paragraph which should be addressed. I also struggle to follow what is important here. The authors bounce between nondimensional numbers but do not make clear why. What is notable about the decrease in diffusivity after frontal passage? Is this surprising?

In here we describe the observations.

571 I thought the lower layer difered significantly from what we would expect the tide to do (i.e., Fig, 8)?

Since the tidal velocities are increasing over the data collection period, it makes sense that shear is also increasing and therefore bottom-induced mixing is elevated as the tidal velocities strengthen.

575 Again…Fig. 13 shows high mixing efficiencies throughout the top layers for all groups. So you have conflicting results which need to be addressed.

We believe this has been addressed see response to major comments

580 Spicer et al. (2021) was an idealized modeling study so no observations in the

CT river plume.

Language adjusted to reflect this.

581-595 This is the first mention of the discrepancies in results. This should be in the discussion and expanded on more….not saved for the last paragraph of the conclusions.

Further, the idea of straining as a mixing/stratifying mechanism is mentioned in the introduction then again here in the conclusions…. but I don't think there is any analysis or discussion on straining in the remainder of the text. If there is, I seem to be forgetting it. Either completely remove the idea of straining from this paper or provide a true analysis of the mechanism.

We have expanded on these in the revised manuscript as suggested. See our response in major comments and revised paper.