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Major Comments:

I think this paper is GREATLY improved since I first read it, but there are still more improvements needed.

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.
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.

Minor Comments (by line number):

13	In the text "…a return was flow developed…", the "was" should be removed.
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.
142-143	Looks like a few lines repeat here.
Fig. 1	Looks much better with bathymetry!
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.
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?

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.
253	You mean across-front here, right?
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.
281	You are not showing divergence on these plots. If you are going to make claims about where different 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.
Fig. 8	Label the map panel (c) or (d). It is the only subplot without a label.
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.
345	You are not calculating a correlation coefficient, so you should not say there is a correlation.
359	"Extend" should be "extent".
Table 2	Caption...specify each variable and it's long name to reminder readers and provide a thorough description of the table. Also...why are units cm/s here and m/s everywhere else?
386-393	Provide more information on why you are presenting these nondimensional numbers. 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. Make sure to mention the range in values shown in your Fig. 11.

	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?
	Is “U_s” in Fig. 11 the same as “U_f” in the text?
397	Where does 0.33 m/s come from?
401	There is a stray “t” before “LW”.
443	You have 4N^2 plotted, not 4S^2.
446	Label each subplot by group number in Fig. 13.
446-457	This paragraph could be streamlined to just focus on most important patterns. It is a little confusing to read right now.
458	It is not too surprising that shear exceeds stratification in the bottom layer….there is no plume there. This is pretty common.
458-467	After reading this paragraph, you could easily remove the paragraph directly before this.
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.
Eq. 6	Please explain physically what the turbulence potential energy (TPE) is and why it is compared to TKE in Fig. 15.
Fig. 14	The y-axis tick labels are missing from the bottom plots.
525-530	Mixing efficiencies 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 efficiency calculated in multiple ways? (this is never said).
	Which is correct?
	Does comparison between the methods provide insight into mechanisms?
533	Typo in this line. “we have not estimates of” needs to be reworded.
549	Typo: “e_k was of the order of”?
555	Not according to Fig. 13.

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?

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

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.

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

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.