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Discussions

Interactive comment on "Buoyancy-driven effects on turbulent diffusivity induced by a river plume in the southern Brazilian shelf" *by* Rafael André Ávila and Paulo H. R. Calil

Anonymous Referee #1

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General comments: The manuscript by Avila and Calil presents an observational study of the Plata Plume Water on the Southern Brazilian Shelf. The manuscript does not pose any scientific gaps, research questions or research objectives that the work is addressing. It essentially presents some field observations of microstructure measurements in a case study. The overall organization of the manuscript needs to be restructured. It does not offer a discussion or summary section, nor does it present the organization of the paper in the introduction. There are grammatical errors throughout the paper that need to be addressed. I have serious concerns about the methodology behind some of the calculations used in this work, which I will detail below. The authors also seem to use estuarine references in support of river plume patterns, which

is not appropriate. These are considerable flaws and I do not think this manuscript is publishable in current form. I recommend rejection. However, I acknowledge the value of these data and believe that the authors could turn this into a valuable contribution. I encourage the authors to take this critical feedback and repackage these data into a manuscript that fills a targeted scientific gap, informed by a more thorough literature review.

Scientific issues: In measuring turbulence in river plumes, particularly in areas with Kelvin Helmholtz instabilities, the prevalence of intermittency needs to be addressed. Researchers typically average repeated casts to ensure the profile they collect isn't some rare intermittent event. There is no mention of repeated casts, or the total number of casts in this manuscript. I suggest reading MacDonald et al. 2013 for more info on intermittency. You cannot use inferred velocities from CTD measurements to calculate gradient Richardson numbers. Turbulence can locally impact the vertical distribution of velocity, which would not be captured by velocities inferred by using the thermal wind balance. Using this balance is fine for obtaining an order of magnitude idea of velocities, but you cannot use this to address mixing from shear instabilities in a river plume. This is probably why your gradient Richardson number are so large ($\sim$100). The important threshold to assess mixing is 0.25<Ri<1. Everything above Ri=3 has been shown to be converted to internal wave energy. The manuscript mentions that the instrument operated in down cast modes ranging from 0.2 to 1.4 m/s. The only portion of the water column that can be used is when the instrument is descending at a constant rate. This is because the instrument decent speed is used to convert to wavenumber space. Your descent speed should not be varying this much and would introduce errors in your turbulence calculations. Also, there is no mention of angle of attack. Typically, you need to remove bins where the instrument's angle from vertical is larger than +- 5 deg. You need to show example spectra and profiles with confidence intervals in order to provide evidence of the quality of the data. The usage of Peters (1997) and Stacey et al. (1999) to support claims regarding river plumes is perplexing. These are turbulence papers in estuaries, not river plumes and are not appropriate ref-

erences. Matar et al. (2013) did not develop the theory behind Thorpe displacements. Reference the Thorpe text book. I do not understand where the double diffusion analysis comes into this work. How does this relate to river plume mixing? Your conclusions aren't actually conclusions, they are site specific patterns. First, you mention that the source region is stable and shear instabilities are increased farther away. Second, you point out mid field and far field regions of the plume and point out some mixing mechanisms. How do these observations fit into the broader understanding of river plumes? Are these patterns unexpected? Or would they be anticipated from previous studies?

A few minor comments: There are many grammatical errors but given the current state of this manuscript I am not going to go through them. There are way too many acronyms to keep track of in this work. Data is plural. You units are incorrect in the molecular thermal diffusivity Your references to patterns in figures needs to be more quantitative. Say notable values in text.

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