General comments

This manuscript presents an interesting dataset of CTD and TKE dissipation profiles from two Fjordic regions of Patagonia across different seasons and years. The study concludes that Northern Patagonia is highly stratified by freshwater input, so mixing is focused at sills and narrow passages and is largely driven by semidiurnal tides. Southern Patagonia is more weakly stratified and more uniformly mixed, with tides and persistent winds playing a larger role.
Overall, tidal processes dominate the mixing energy budget, and these patterns offer a framework for anticipating how fjord mixing may shift under future changes in freshwater and atmospheric forcing.

These overarching conclusions are reasonable and broadly consistent with established understanding of fjord dynamics. However, several of the conclusions are only weakly supported by the analysis currently presented. In particular, the regional contrasts in mixing regimes, the attribution of dissipation to tidal forcing, and the implications for future climate-driven changes are stated more strongly than the underlying evidence justifies. The observational analysis is promising, but key elements—such as sampling representativeness, methodological detail for microstructure processing, quantitative comparisons of forcing mechanisms, and uncertainty estimates—need to be expanded or clarified before the conclusions can be considered fully supported. Strengthening these links would significantly improve the robustness and impact of the manuscript.

See below for further detail on these points

Major Points

1. While the spatial patterns of dissipation in fig7 support the conclusion of tidal mixing “hotspots”, the evidence presented for the tide being responsible for this needs to be stronger. Use of h/U3 is sensible but needs to be explained and justified to the reader, with better explanation of the data sources used to calculate it. Current analysis using h/U3 is qualitative and confusing.

   The maps in Figure 9 are unclear, is h/U^3 plotted or tidal energy dissipation? Either way it is hard to reconcile with the section presented in previous figure. I suggest extracting and plotting the h/U3 values along the section directly against the measured depth integrated dissipation or producing line plots of each of these as well as stratification (preferably potential energy anomaly – see point 2), so that their relationship can be examined together on the same horizontal scale. Currently the reader is left trying to reconcile map plots of Figure 9 with sections presented in earlier figures. Greater consistency in labelling of all the maps/section, would make this easier, however bringing stratification, dissipation and forcing together into a single plot would be preferable. By including the wind forcing in this plot the conclusion that wind mixing is of secondary importance could be properly tested. However, high dissipation, measured in the surface mixed layer, figure 7, suggests that the wind plays a role, at least in the surface mixed layer for some of the sections.

Ans:

Thanks for your comment, now we enhance the explanation of the tidal dissipation parameter which was obtained from the tidal model output. To a better understanding the figures were enhanced and labeling of sites wer included to reinforce the findings.

The old figure 9, included both tidal mixing and tidal dissipation, that is possible because there are inverse related (eq. 7), now the manuscript description was focused on tidal dissipation instead of tidal mixing.

We include a new figure in the Appendix 3, on there you can examined the along-fjord changes in both regions northern and southern Patagonia, the distributions of k_{shear} nd epsilon could be directly compared and estimate the typical values from their distributions in the study region. In addition, below (in response 4) we made estimations of the energy of the stratification, and the energy of the wind to make comparison between these three forcing: tidal, stratification and winds. We thinks that estimations and its comparisons, that now are inserted on Discussions, covers the comment in this new version of the manuscript.

2. The integrated stratification parameter ns, (Haralambidou et al. 2010) is used to parameterise stratification. I have not heard of this study or this parameter and cannot find the publication in the Journal of Marine Science, using the reference provided. A wider search suggests it's use in salt wedge estuaries and near shore coastal environments, where the role of tidal dynamics is through tidal straining on short timescales. It is not a standard or widely used parameter and makes an unusual choice in this study. While I can see that the integrated stratification parameter provides a convenient bulk descriptor of water-column stability, it is not the most physically meaningful metric for linking stratification to turbulent mixing in fjord systems. Because ns collapses vertical structure into a single integral of N, it can obscure the energetic significance of thin surface haloclines or deeper weakly stratified layers. In contrast, the potential energy anomaly (PEA) (Simpson 1981) directly quantifies the mechanical energy required to homogenise the water column and therefore offers a more robust basis for comparing mixing regimes and interpreting dissipation patterns. For this study, PEA would provide a clearer and more physically grounded measure of stratification providing quantitative insight and allow the reader to compare with other regions.

Ans.
We deleted the figure and results related to the stratification parameter (ns) and added a new figure and results using the potential energy anomaly (PEA) to provide information on stratification and mixing in the water column. The results of the PEA computation were like those of ns, as was reported in Pérez-Santos et al. (2021).

2021. Pérez-Santos et al. Oceanographic time series reveal asynchrony input of oceanic and estuarine waters in Patagonian fjords, Science of the

Total Environment 798, 146241. https://doi.org/10.1016/j.scitotenv.2021.149241

3. The discussion of tidal mixing and energy dissipation in 3.3, needs to be clearer. The fact that “semidiurnal constituents account for approximately 77% and 97% of the total dissipated” needs explaining. This is the most striking quantitative conclusion, but the method behind it is not clearly laid out. Is it model-derived? Is it a scaling argument? Is it based on local observations? The conclusion may be correct, but the evidence of this is weak.

Ans:

Thanks for your comment, this analysis was made using the model-derived output now we clarify that on the new manuscript on lines XX - XX. The result was obtained by the use of the equation (7) $\beta_{tidal} = \rho C_{bd} U^3$/H, here we estimated the tidal current using all (11) model constituent that give us the total amount of tidal dissipation energy, then we use the same equation only for the semidiurnal tidal currents by M_2 and S_2 (U_{M2}^3, U_{S2}^3) thus β_{M2}/β_{tidal} x 100 was the percentage indicated above (similar case for S_2). When we compare the tidal amplitudes with tide gauges in the region we obtain a similar explained percentages for the semidiurnal constituents, Castillo et al (2025) calculated the tidal regime of the southern Patagonia which give us a form parameter consistent with a marked semi-diurnal regime.

4. While the study nicely contrasts freshwater-driven stratification in the north with more mixed conditions in the south. I don’t agree with the claim in line 27, that southern Patagonia remained vertically well mixed throughout the year, figures 5 and 8 suggest significant stratification and inhibited mixing at depth over the northern half of the southern region. There is a seasonal signal here that is a key feature of the observations but is not investigated. The importance of freshwater input needs to be acknowledged, and could be made stronger, by quantifying stratification and integrating climatology and reanalysis.

Ans:

Thanks for your comments, in terms of the wind influence a companion manuscript (Castillo et al., pre-print https://egusphere.copernicus.org/preprints/2025/egusphere-2025-5692/) here we estimated the wind influence in southern Patagonia by the estimation of the energy of the stratification of the water column and the energy by wind mixing following methods by Denman and Miyake (1973), Bowden (1981) and (Simpson et al., 1990).

The study estimated the energy of wind for mixing the upper water column, the authors estimated in 5.1 x 10^{-3} W m^{-2} the energy of the wind which was higher than the energy of the upper water column (1.2 x 10^{-3} W m^{-2}) during summer of 2024 this was an indicative of the capacity of the wind for mixing the upper water column under highly stratified conditions of summer. Now, we made similar estimations for the Northern Patagonia (PN), you must notice that for the complexity of the region we divide the region in PN1 (Reloncaví–Desertores), PN2 (Desertores–Moraleda), PN3 (Moraleda–Laguna San Rafael).

The energy of the upper water column for summer ranged between 6 x 10^{-7} W m^{-2} (PN1) and 4 x 10^{-6} W m^{-2} (PN3) during summer. In winter the estimations were between 1 x 10^{-7} W m^{-2} (PN3) and 3 x 10^{-5} W m^{-2} (PN1). For the same seasons, the energy for mixing of the wind was of 1.5 x 10^{-3} W m^{-2} in winter and 5 x 10^{-3} W m^{-2} in summer for typical wind intensities during winter and summer.

The new Fig. 10, use a range for the tidal mixing (H/U^3) between 10^{-3} to 10^{-9} W m^{-2} which shows that the inner-sea of Chiloe (between Ancud gulf and Puyuhuapi fjord) are mostly dominated by higher values (O~10^{-3} W m^{-2}) similar values were observed at the Southern Patagonia into the Magellan strait between Punta Arenas (PA) and the connection with the Atlantic ocean where these values were even as larger as 10^{-4} W m^{-2}. Those values were well adjusted to the wind energy indicating that winds and tidal could be important drivers for mixing in Northern and southern Patagonia regions. In regions, like Aysen fjord, Quitralco fjord, and San Rafael lagoon the tidal mixing was lower (O~10^{-8} W m^{-2}) whereas in Almirantazgo fjord and Parry fjord the tidal mixing was O~10^{-7} W m^{-2} indicating that in those locations tidal mixing was lower than wind energy.

In the case of the freshwater input, his study now includes PEA estimation for the different seasons which were included on the new Fig. 7., but a most complete regional calculations of the freshwater input and its seasonality was recently made by Garces-Vargas et al (2026). Here, the authors indicate that freshwater input were higher in northern and central Patagonia and marked seasonal with lower inputs during winter. In southern Patagonia, no major seasonal signal was observed but freshwater input during spring and summer seems to have more freshwater than during the colder seasons.

Castillo, M. I., Zuñiga, C., Barrios-Guzmán, C., Cisternas, N., Garces-Vargas, J., Landaeta, M. F., Piñones, A., Rojas-Celis, M., Guerrero, A. I., and Sepúlveda, M.: The answer is blowing in the wind: seasonal hydrography and mixing of the inner sea of Tierra del Fuego, Southern Patagonia, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2025-5692, 2025 .

Garcés-Vargas, J., Piñones, A., Schneider, W., Landaeta, M. F., Castillo, M. I., Cisternas, N., ... & Barilari, F. (2026). Seasonal dynamics and forcing mechanisms of the Cape Horn Current: insights from reanalysis data and hydrographic observations. Progress in Oceanography, 103665. https://doi.org/10.1016/j.pocean.2025.103665

5. The abstract claims the study “provide a framework for understanding how fjord systems may respond to changing freshwater inputs and atmospheric forcing under a warming climate.”. This is good direction, but without further

analysis of the role of wind and freshwater input the study provides little predictive power. It would be more realistic to claim improved understanding of processes rather than prediction.

Ans.
We edited the sentence according to the comment.

6. The methodological detail on the calculation of TKE dissipation needs to provide enough detail for reproducibility it currently simply comprises of "All profiles were first checked by visual inspection to ensure consistency across sensors". How are fall rates and noise dealt with, which method is used to estimate epsilion (Nasmyth fitting, integration, and what limits are used etc). Provide more detail here to provide confidence in the processing methods.

Ans.
We clarify and better the methodology of the TKE dissipation. A new paragraph was added to section 2.2.

The VMP-250 microstructure profiler measured the vertical shear of horizontal velocity in two orthogonal directions ($\partial u/\partial z$ *and* $\partial v/\partial z$*) using two shear probes mounted in the nose-cone of the instrument (Fig. 2b). The profiler was operated in a downward mode, recording data at a sampling rate of 512 Hz as it descended at a free-fall speed of approximately 0.7* $m\ s^{-1}$*. To ensure data quality, a minimum profiling speed of 0.2* $m\ s^{-1}$ *and a minimum profile duration of 20 seconds were required for a cast to be considered valid. At least two profiles were collected at each station to ensure data reliability and to assess temporal variability. For the computation of the shear spectrum, referred to as the Nasmyth spectrum, several processing parameters were defined: the dissipation length was set to 8 seconds, the high-pass filter cutoff frequency for shear probe data was 0.4 Hz, the low-pass filter cutoff was 30 Hz, and the anti-aliasing filter cutoff was 98 Hz (Lueck et al., 2013). Corrections for profiler vibrations were applied using the method described by Goodman et al. (2006). Data were excluded from analysis when the instrument's inclination angle exceeded 5%. In steady, non-stratified flow, the rate of turbulent kinetic energy dissipation is equal to the rate of production. In contrast, under stratified conditions, approximately 20% of the energy produced is expended in increasing the fluid's potential energy, while the remaining 80% is dissipated as turbulence (Lueck et al., 2013).*

7. The detail on the microstructure data collection is also insufficient. Was there just a single profile at each station, or were time series collected at each station? Given that the sections of dissipation are presented to indicate the spatial variability in mixing across the region, this temporal information is highly relevant, particularly when the tide is implicated. Temporal variability in dissipation over a tidal cycle means that single profiles are unrepresentative of the mean dissipation and this needs to be acknowledged and examined.

Ans.
We edited section 2.2 as we mentioned before. We added the information of the tidal moments to Table 1.

8. I am not familiar with the literature or the regions, so cannot vouch that the introduction is complete or accurate, but it provides a detailed contrast between the geography, hydrology and circulation of the two study regions. The discussion sections 4.1 and 4.2 appear to continue a review of the literature without comparison with the findings of the study, so should be reduced to what is relevant and incorporated with the introduction.

Ans.
We edited sections 4.1 and 4.2 based on the similar comments from R1. Now, a summary of the older 4.1 section was included on Introduction between lines 48 - 55 and into lines 105-113.

Minor Points

1. The order of the sectional plots (fig 3,4, 7) is not chronological, with Spring 2024 coming after Fall and winter. It would make more sense to plot full width sections from top (oldest) to bottom (newest).

Ans.

We ordered the figure subplots from Fig. 3 and 4 according to the suggestion of Reviewer 1, who recommended plotting the seasons of 2023 and 2024 on the same line. We added the new figures to the text.

The subplots were organized in the austral seasons order, e.g., Summer (January, February, and March), Fall (April, May, and June), Winter (July, August, and September), and Spring (October, November, and December).

2. The seasonality in density is small, so that all the panels in figure 3 are hard to discern. A mean section and anomalies would be much more interesting.

Ans.
We calculated density anomalies for both the north and south Patagonia using the average of the seasons. Two new figures were added to the manuscript in the section of the Appendix, as Figure A1 and Figure A2. The description of these figures was incorporated into section 3.1.

3. I found it hard to reconcile the text and the figures as the geographical names referred to in the text are not used in the figures, e.g. lines 278. The names are not consistent between different figures or the map in figure 1, and in many of the figure the writing is very small. I suggest using the abbreviations in figure 1 in all figures and text to help the reader know where in the section to look.

Ans.
We edited Fig. 10 to better represent the results and added the names of the

geographical areas, similar to those presented in Fig. 1 of the manuscript.

4. The introduction discusses variability in mixing processes, but it says little about temporal **variability in freshwater input,** which is relevant to the patterns observed.

Ans.
We edited the introduction section and added new information.

5. Line 181 surface not surface

Ans.

We edited the typo error in equation 5.

6. ε: should be W kg−1 (not W kg−2 as appears in the abstract)

Ans.

We edited the unit by W kg^{-1}.