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.

2. The integrated stratification parameter n_s, (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.

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.

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.

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.

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.

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.

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.

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).
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.
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.
4. The introduction discusses the variability in mixing processes, but there is little discussion of the temporal variability in freshwater input, which is relevant to the patterns observed.
5. Line 181 surface not sirface
6. ε: should be $W\ kg^{-1}$ (not $W\ kg^{-2}$ as appears in the abstract)