Reviewer 1

General Comments

The revised manuscript has been substantially improved in response to the previous round of review. The authors have addressed several critical issues in physical scaling, parameter choices, and methodological transparency. In particular, the Wedderburn number (W_B) calculations now employ physically plausible length and upper-layer scales derived directly from the hydrographic data, and the flushing-time analysis has been reframed around an explicit Knudsen-balance approach for the upper layer. The manuscript is now more internally consistent, and the core scientific narrative, that along-fjord wind stress is a first-order driver of mixing and ventilation in this high-latitude fjord, is better supported by the revised diagnostics.

However, I have identified several remaining issues that should be addressed before the manuscript can be accepted. Most are minor and can be resolved with targeted text revisions. These are detailed below.

Major Comments

• Flushing time methodology.

The revised flushing time estimates using modelled velocities (v1 = 0.3 ms−1 in January, v1 = 0.1 ms−1 in June) represent a significant improvement over the previous approach based on internal wave phase speeds. The resulting flushing times (TF = 7.23 d in January, TF = 8.68 d in June) are physically reasonable for a fjord of this geometry and forcing, and the Knudsen-balance framework is well-suited for first-order estimation.

I appreciate that the authors transparently acknowledge the limitation that the model velocities have not been independently validated against in-situ current measurements. However, the quantitative implications of this limitation deserve more explicit treatment. The QR derived from the Knudsen balance depends on two quantities that are both poorly constrained: (1) the advective velocity v1 from the unvalidated model, and (2) the salinity contrast ΔS = S2 − S1 between layers. For January, Table 3 gives S1 = 29.405 +/- 1.131 g kg−1 and S2 = 30.753 +/- 0.357 g kg−1, so the signal driving QR is ΔS ≈ 1.35 g kg−1. Critically, the CTD-SRLD validation in Appendix 2 shows that the surface-layer summer salinity RMSD is 1.88 g kg−1 with r = −0.09 (i.e., the measurement uncertainty in S1 exceeds the ΔS signal upon which the entire Knudsen balance rests. This explains the very large, reported uncertainty: QR = 789 +/- 925 m3 s−1 for January (uncertainty exceeding the mean) and QR = 3.97 +/- 36.07 m3 s−1 for June (effectively indistinguishable from zero).

The qualitative inference that glacial freshwater inputs likely dominate over the Azopardo River discharge is physically sound, the system is fed by multiple tidewater glaciers. However, using the Knudsen-derived QR as precise quantitative evidence 1 for that dominance ("~16 times the river discharge") overstates what this analysis can reliably resolve. I recommend the authors:

(a) Retain the Knudsen calculation as a valuable first-order diagnostic, but present TF and QR as order-of-magnitude estimates rather than precise values.

Ans:

Thanks for your comment, which is highly valuable, in the new version of the manuscript we updated Table 3 taking out the Q_R values considering your suggestion that these first approximation estimations had longer uncertainty thus we decide to inform that estimations as orders of magnitude. We include in the text a sentence that indicates that accurate quantification flushing times requires a second-order study which implies a designed experiment or validated circulation model to determine water column exchanges on this region. **Lines 638-645.**

(b) Maintain the physical interpretation that glacial inputs likely dominate the freshwater budget, but support this argument qualitatively (glaciological context, regional estimates) rather than relying on the specific QR value, whose uncertainty renders it unsuitable for budget partitioning.

Ans:

We maintained the idea that in summer the main freshwater input could be associated with glacial input instead of river discharge. The study of Izaguirre et al (2025) indicated that the region Darwin's Cordillera is dominated by Glacial Lake Outburst Floods (GLOFs) which could supply melting waters to the region of the Almirantazgo fjord. We indicate that additional studies are needed to support quantitatively this asseveration and that was beyond the scope of this study. **Lines 719-721.**

(c) Add a sentence noting that the surface-layer salinity uncertainty (RMSD > ΔS) is the primary factor limiting the precision of the Knudsen-based estimates, and that future work should include direct current observations or a validated model, alongside improved salinity sampling to better constrain the exchange.

Ans:

A suggested sentence was included in **lines 638-643.**

• **Deep-basin renewal.**

The authors have successfully restricted their claims about renewal and ventilation to the upper brackish layer (~30 m depth) at the inner bay at the head of the fjord in the body of the manuscript. This is a major improvement and properly reflects the observational limitations.

However, the Abstract still states: “first-order flushing time estimates suggest slow deep-basin renewal (greater than 5 months).” This sentence has two problems: (1) it contradicts the conclusions, and (2) the “greater than 5 months” figure does not appear anywhere in the revised manuscript body and is not supported by any calculation. The flushing times reported in Table 3 are ~7–9 days and refer exclusively to the upper layer. The authors should either remove this claim from the Abstract entirely or replace it with wording consistent with what is actually demonstrated.

Ans:

Thanks for your comment, we changed the abstract accordingly. **Lines 20 – 33.**

- **Barotropic seiche**

In the revised manuscript (lines 655–658), the authors compute the natural oscillation period of the basin as $T_N = 4Lc^{-1}$ using L = 165 km (Inútil Bay to María Cove), yielding $T_N \approx 3.95$ h, which matches the 4-hour signal observed in the sealevel wavelet. This is a potentially interesting result. However, L = 165 km is the same path length that was criticised in my first review and subsequently replaced with L = 75 km for all fjord-scale wind diagnostics and the Knudsen balance. The manuscript currently switches domains without justification: L = 75 km for the Wedderburn number and baroclinic adjustment, but L = 165 km for the barotropic seiche because it happens to reproduce the observed period. Note that using L = 75 km would yield $T_N \approx 1.8$ h, which does not match the observed 4-h signal.

This discrepancy is physically resolvable, a barotropic seiche may indeed oscillate over a larger connected basin than the effective wind-action or estuarine-exchange domain, but the argument must be made explicitly. The authors should justify physically why the seiche excitation domain (the full inner-sea system from Inútil Bay to María Cove) differs from the effective wind-action domain (the Almirantazgo Fjord proper).

Ans:

Thanks for the comment, thinking of 4h sealevel variability is noticeable but not significant (Fig. 10b) and the scope of the study is wind-induced variability and its effects on the hydrography we decided to delete this paragraph. This high frequency variability will be included in future studies in the region, deploying pressure sensors along the fjord and recording the high variability oscillations to evaluate the oscillations modes of the barotropic pressure gradient ($g\partial\eta/\partial x$) with that information we could study the high frequency oscillations of the sealevel on the Almirantazgo fjord.

Minor Comments

• Wind power: The comparison between dϕW/dt and dϕE/dt is now presented. However, I note that the wind mixing power uses W10 = 15 ms−1 (a peak-event value), while the estuarine stratification power uses mean seasonal conditions. Since the wind power scales as W3 10, the mean wind speed (∼5–6 ms−1, implied by the reported mean stress) would yield a mixing power roughly an order of magnitude lower. The comparison should either use consistent temporal frames (both event scale or both time-averaged), or the text should explicitly note that the comparison demonstrates episodic, not sustained, wind dominance.

Ans:

Thanks for the comment, instead to the previous version, here we decide to inform the mean and percentile 90 wind magnitude and use that values to determine dϕW/dt this new estimation indicates that wind energy it is comparable with stratification energy for intense and episodic events as you indicate. We also indicate that the minimum wind magnitude to be as high as stratification is 9 ms-1. The paragraph was included in **lines 750-765.**

• Seal-borne salinity: As discussed in ’Flushing time methodology’, the CTD-SRLD surface-layer summer salinity uncertainty (RMSD = 1.88 g kg−1, r = −0.09) directly impacts the Knudsen balance. It also affects the along-fjord Freshwater Content (FWC) estimates derived from seal-borne profiles (Figure 7). The authors should include a brief statement in the FWC discussion acknowledging that the summer upper-layer haline structure is more uncertain than the temperature structure from the CTD-SRLD data, and that this uncertainty propagates into the along-fjord FWC gradients.

Ans:

We include a paragraph that compares the values with other estimations of FWC in Chilean fjords and we include the uncertainty of the brackish layer Salinity estimations by CTD-SRLD data**. Lines 668 – 674.**

• ERA5 wind Lines 700–702 correctly note that ERA5 wind magnitudes are typically ∼20% lower than meteorological-station observations. Since Wb ∝ τ , the actual Wb values during strong wind events could be 20–40% higher than those reported.

This reinforces the authors’ central conclusion and should be noted explicitly in Section 4.3: the reported Wb values are likely conservative estimates of wind-driven pycnocline perturbation.

Ans:

Thanks for your comments, we include a sentence that remarks that the wind-based estimations of the study were highly conservative. **Lines 727-728.**

• Baroclinic adjustment. The baroclinic adjustment time scale for June is ts = L/c1 ≈ 22 d, yet the estimated flushing time is TF = 8.7 d. If the upper layer is flushed before the baroclinic signal traverses the basin, the wind-setup mechanism described in the text cannot reach a quasi-steady state in winter. This apparent inconsistency should be briefly discussed, it may reflect the inherently episodic and transient nature of wind-driven exchange in the weakly stratified winter season, which would be a physically meaningful conclusion.

Ans:

Thanks for your comment, we include a paragraph where we discuss about the comparison between ts and TF for January and June in the region. **Lines 695-699.**

Reviewer 2

- Lines 39-41 – Still the cross-sectional diagram shown doesn't make sense to me in the graphical abstract. Specifically, what do the lines and blue colours represent?

Ans:

Thanks, we change the graphical abstract, we include three layers as you request, take out transversal dynamics in the new graphical abstract.

- Line 53 – The Valle-Levinson et al., 2014 paper actually shows that 3 or more layered fjords is typical. As this paper mentions, most of the observational evidence supports 3 or more layers in most fjords. As I mentioned in my previous review, a two-layer system is the exception and not the norm. The authors do state below that 3 layer system are typical in Patagonia so this argument seems confusing to me.

Ans:

Thanks for your comment, we included in the new graphical abstract a schematic three-layer system. We also change the description for shallow-water estuaries (Line 54) and put them in contrast with deep-water estuaries like fjords (line 62) where three-layer system are often observed. We also deleted in conclusions section any reference for a two-layered system of the Almirantazgo fjord. Now we included a paragraph in Discussion about the formation of three layers in deep estuaries and the driving mechanism. **Lines 585 – 595.**

- Line 73 – The is an extract bracket) on this line

Ans:

We deleted the bracket on **Line 75**.

- Lines 220 – 223 – While it's good to report the post-processing steps, a key one is still missing – alignment in space and time of the dissolved oxygen sensor and the CTD. Was this done?

Ans:

Thanks for the comment, but maybe there is confusion. We used time-series data for the oxygen by miniDOT PMEL instrument this was deployed at 30 m depth in the moorings A1 (January-June 2024) and A2 (January 2025). That data doesn't have alignment because is a self-contained miniDOT this instrument record T and DO which were compared with NKE WiSens T, S and Pressure and in both seasons, data showed a highly good relationship as an example in Temperature the comparison between T from both instruments was r =.98. For the CTD data the study only uses Salinity and Temperature from the profiler CTD, even that, we make the Oxygen alignment, but none DO data from the profilers were included in the study.

- Line 237 – what reference salinity was chosen here?

Ans:

We use the maximum Salinity in the inner-sea of Tierra del Fuego which was 31 g kg^{-1}. **Lines 241-242.**

- Lines 243-244 – how were time differences incorporated into this spatial average? In other words, what time window was considered when calculating these spatial averages?

Ans:

Thanks, a temporal window of 3 weeks center at the CTD-AML dates were applied to obtain the typical pattern of the along-fjord CTD-SRDL structure. We included this explanation on **lines 247-248**.

- Line 268 – I suggest that the DO sensor accuracy is reported in mL/L since that is the unit that is used in this manuscript.

Ans:

Now the DO accuracy is reported in mL/L on **line 274.**

- Line 273 – I'm confused about this 5 day time series. Were more data collected or was the time series only 5 days. Please explain.

Ans:

Thanks, we realize that the reviewer version does not include the year of this short mooring which was a different mooring from the longer time-series and which was located at the site A2 (Fig. 2) into Maria cove. That time-series was deployed for 7 days in the region but, we recovered in two different days because weather conditions require to recover thus the time series had a complete set of all the data for 5 days which was presented here. The main goal of this mooring was to show the vertical structure of the water column (from surface to bottom) of the parameters which were not possible to obtain in the longer time-series. We added the year and already indicated that the short time-series was to determine vertical gradients. **Line 279.**

- Figure 3 – the water regions (coloured?) are much larger than shown in Figure 1, making this figure confusing. I suggest using the same topography here as is shown in Figure 1

Ans:

The new Fig. 2 now includes a colorscale from depths and highs of the region based on the GEBCO (https://www.gebco.net/data-products/gridded-bathymetry-data) data. We also changed the position of the triangle in Fig. 1 that covers the location in the previous version, and we changed the colorscale in Fig. 2 because doesn't fit well with the zoomed region of Fig. 2. **Lines 264-267.**

- Line 350 – Figure A1 in the Appendix doesn't contain CTD data. What am I missing here? In general, the figures labeled in the appendix don't match what is written in the text

Ans:

Thanks that was our mistake, we changed to refers Appendix 2. **Line 353.**

- Section 3.3 – I think the number of significant figures used here is too many given the accuracy of the sensors and the uncertainty of the processing. The authors are reporting climate-level accuracy in the data, which is normally only achieved following WOCE protocols. The oxygen data for example can likely only be interpreted to 0.1 mL/L given that most instruments have a +/- 10% accuracy when no in situ bottle data are collected for calibration.

Ans:

We changed the significant decimals in the section and in Table 3, with special care in DO concentration as you suggest, maintain just one decimal. **Lines 353 – 490.**

- Line 391 – I don't see a dashed black line in panel a)

Ans:

We corrected and now we indicate that the stations are present in yellow circles. **Line 402.**

- Lines 394 to 401 – Since the reference salinity wasn't reported and it isn't known if the same reference salinity was used for all data, the calculated FWC shown here doesn't mean much.

Ans:

Thanks for the comment, we include the reference Salinity used in the study based on the maximum Salinity in the deeper part of the Almirantazgo fjord which was 31 g kg-1. **Lines 241-242.**

- Lines 513-516 – The DO gradient is confusing. What do negative numbers means?

Ans:

The difference between the deeper $\triangle$DO= DO (30 m) - DO (0.6 m), similarly to $\triangle\rho$. Deeper water (here at 30 m) showed lower DO concentrations than the surface, due to stratification then $\triangle$DO < 0 most with weak winds because these conditions are favorable for stratification of the water column. Under $W_b > 1$ the stratification breaks and mixing conditions are favorable thus $\triangle$DO tends to zero. We include this description on **lines 748-749.**

- Line 530 – Where are the data for the global wavelet spectrum from ?

Ans:

The global wavelet spectra are the time-averaged spectra of each variable. In other words, is the average of amplitude for each frequency for each data sets following the typical wavelet analysis according to Torrence and Compo (1998) already referenced in the manuscript.

- Lines 581-585 – I suggest rewriting this paragraph. It is confusing (e.g. what does 'This data was highly consistent with the traditional CTD data obtained in cross-fjord measurements.'). I don't understand what this sentence is referring to or where the proof for this statement is. Perhaps referring to sections or figures would help.

Ans:

Thanks, we modify the paragraph and referring to Figures and locations, of what we are comparing here. Also, we now deleted that sentence and changed by “. The mean profiles from the along-fjord CTD-SRDL (Figs. 5c,d) and the mean profiles of the CTD-AML (Figs. 6c,d) were highly consistent between the seasons; highly stratified in January and mixed during June.**”** **In lines 597-610.**

- Line 587 – This is the first time that layers have been defined in this paper. How did the authors decide that a 50 m surface layer is appropriate? This is very deep for a surface layer in a fjord, which is often less than 10 m.

Ans:

Thanks, we use the limit of 50 m to make comparisons between the CTD-SRLD and CTD-AML. This imply that the upper layer was between 4 m – 50 m, and deeper layer was for depths > 50 m. Now we are making similar comparisons for three different limits, 10 m, 20 m and 50 m (content in the previous version) to make the comparisons in the inner the fjord data. The analysis was made for each station CTD2 and CTD3 with available data to compare. In the new version of the manuscript, we are focusing in the 20 m results in the inner-fjord system. Below is the table in which based our Discussion in section 4.1, **Lines 597 – 610.**

Table of Comparison between CTD-SRLD and CTD-AML for upper (4 – 20 m) and deeper (> 20 m). For January (Jan) and June (Jun.) of the Temperature (CT) and Salinity (SA). The RMSD and linear correlation (r) it is shown.

			Depths 4 – 20 m		**Depths > 20 m**	
Location	Season	Variable	RMSD	r	RMSD	r
Inside Fjord E2	Jan.	CT	0.219	**0.992**	0.100	**0.991**
Inside Fjord E3	Jan.	CT	0.370	**0.961**	0.169	**0.990**
Inside Fjord E2	Jan.	SA	2.965	**0.665**	0.650	**-0.803**
Inside Fjord E3	Jan.	SA	2.607	**0.844**	0.559	**-0.724**
Inside Fjord E2	Jun.	CT	1.058	**0.801**	0.160	**0.911**
Inside Fjord E3	Jun.	CT	1.431	**0.855**	0.333	0.055
Inside Fjord E2	Jun.	SA	0.937	**0.845**	1.176	**0.835**

Inside Fjord E3	Jun.	SA	1.102	**0.939**	1.146	**-0.299**

In this version, we include a paragraph in section 3.3 where we describe the upper layer determinations the seasonal changes of the upper layer near the fjord's head, here depth which was deeper in January than June. **Lines 393-397**.

- Line 592 – What is a much deeper layer? How was it defined?

Ans:

As we indicated above, in the previous version we use 50 m depth to separate the upper and deeper layers. For oceanic waters CTD-SRDL and other sources of information detailed at Appendix 2 showed that for profiles with depths > 50 m a good agreement were obtained with RMSD < 0.3 and r > 0.8 for both temperature and salinity. We maintain this description in this version because for the oceanic profiles the results were maintained.