Second-Round Reviewer Comment on egusphere-2025-5692

Manuscript Title: The answer is blowing in the wind: seasonal hydrography and mixing of the inner sea of Tierra del Fuego, Southern Patagonia

Authors: Castillo et al.

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 ($v_1 = 0.3$ m s^{-1} in January, $v_1 = 0.1$ m s^{-1} in June) represent a significant improvement over the previous approach based on internal wave phase speeds. The resulting flushing times ($T_F = 7.23$ d in January, $T_F = 8.68$ 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 Q_R derived from the Knudsen balance depends on two quantities that are both poorly constrained: (1) the advective velocity v_1 from the unvalidated model, and (2) the salinity contrast $\Delta S = S_2 - S_1$ between layers. For January, Table 3 gives $S_1 = 29.405 \pm 1.131$ g kg^{-1} and $S_2 = 30.753 \pm 0.357$ g kg^{-1}, so the signal driving Q_R is $\Delta S \approx 1.35$ 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 S_1 *exceeds* the ΔS signal upon which the entire Knudsen balance rests. This explains the very large reported uncertainty: $Q_R = 789 \pm 925$ m^3 s^{-1} for January (uncertainty exceeding the mean) and $Q_R = 3.97 \pm 36.07$ m^3 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 Q_R as precise quantitative evidence 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 T_F and Q_R as order-of-magnitude estimates rather than precise values.

(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 Q_R value, whose uncertainty renders it unsuitable for budget partitioning.

(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.

- **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.

- **Barotropic seiche**

  In the revised manuscript (lines 655–658), the authors compute the natural oscillation period of the basin as $T_N = 4L\,c^{-1}$ 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 sea-level 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).

Minor Comments

- **Wind power:** The comparison between $d\phi_W/dt$ and $d\phi_E/dt$ is now presented. However, I note that the wind mixing power uses $W_{10} = 15\ \mathrm{m\,s^{-1}}$ (a peak-event value), while the estuarine stratification power uses mean seasonal conditions. Since the wind power scales as W_{10}^3, the mean wind speed ($\sim$5–6 $\mathrm{m\,s^{-1}}$, 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.

- **Seal-borne salinity:** As discussed in 'Flushing time methodology', the CTD-SRLD surface-layer summer salinity uncertainty (RMSD = $1.88\ \mathrm{g\,kg^{-1}}$, $r = -0.09$) 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.

- **ERA5 wind** Lines 700–702 correctly note that ERA5 wind magnitudes are typically $\sim$20% lower than meteorological-station observations. Since $W_b \propto \tau$, the actual W_b 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 W_b values are likely conservative estimates of wind-driven pycnocline perturbation.

- **Baroclinic adjustment** The baroclinic adjustment time scale for June is $t_s = L/c_1 \approx 22$ d, yet the estimated flushing time is $T_F = 8.7$ 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.

Recommendation: Minor Revisions

The authors have made substantial and largely satisfactory revisions in response to the first round of reviews. The three major concerns raised in my original review, the Wedderburn number parameterization, the flushing time methodology, and the over-reaching deep-basin ventilation claims, have all been substantively addressed in the body of the manuscript.

I recommend acceptance after the authors address the points above, with particular emphasis on correcting the Abstract, justifying the seiche length scale, and improving the uncertainty framing of the flushing-time estimates.