Reviewer #1

My apologies to the authors and editor for the delay in submitting this review. The manuscript by Sola-Hidalgo and co-authors describes the dynamics of deep-water ventilation at the gateway connecting a complex fjord system in southern Chile and the adjacent ocean. The study is based on the analysis of a unique time series of dissolved oxygen, temperature, and salinity, collected at 170 m depth from 2016 to 2024. A somewhat shorter version of the data has been described in previous papers by the same group. In this paper the authors make a clear distinction between seasonally and short-term ventilation sequences, referred to as “periods” and “pulses”, respectively. Despite the differences in time scales, the observed oxygen variability appears to be closely associated with changes of along-shore (meridional) winds through Ekman dynamics.

Major comments:

Overall, the manuscript is very well written. The most important result is the clear association between the near-bottom oxygen variability and the changes in wind pattern, which seems to hold for the short-lived synoptic-scale changes and the more persistent seasonal changes. The ventilation events appear to be associated with onshore Ekman transport and downwelling as a response to northerly winds, and a contribution of Ekman pumping. These results are fairly well supported by the available evidence (particularly Figs. 2, 5, 7, and 8). The authors conclude that the near-bottom ventilation is associated with the replacement of equatorial subsurface water by the relatively warm, fresh, and well-oxygenated subantarctic water. This later argument is a lot weaker, simply because the time series observations are only available near the bottom. Also, one would expect a northerly (downwelling) wind to promote a stronger southward advection of EESW. Please address this in the Discussion.

We agree with the comments, but even northerly winds alone could contribute to the poleward advection of ESSW; sinking winds lower the upper limit of oxygen-poor waters transported by ESSW, thereby facilitating the entry of oxygenated waters of subantarctic origin (Linford et al., 2023). However, we expanded section 4.2 to clarify that while northerly winds can indeed promote poleward advection of the ESSW, their dominant local effect at the coastal boundary is downwelling.

In any case, the authors need to further develop the description of the processes linking the Ekman layer and the changes in near-bottom water masses, and provide more evidence to support their conclusion. These issues are revisited in the Discussion “… salinity at 170 m emerged as an important local factor controlling the strength and persistence of the ventilated state, even if part of this salinity signal was itself linked to the same forcing that promoted renewal.”. I strongly suggest reconsidering the wording, there is no evidence that salinity controls the strength nor the persistence of the ventilation. Coexistence does not imply control. Moreover, with the available observations it is not possible to determine whether the ventilation is local or associated with a ventilated near-bottom inflow.

We agree with the comment and decided to delete this sentence included in the discussion section.

The association between near-bottom oxygen variability and atmospheric rivers (paragraph in line 539), summarized in Fig. 9, is highly speculative. Please clearly spell out what the relevance of water vapor is. Fig. 9 displays a tight relationship between wind magnitude, zonal Ekman transport and integrated water transport. However, the implied moisture impact is totally unclear, and, most important, the oxygen response to the strong wind events (12, 14, 17, and 19 July) is not obvious. Why does DO drop after the 25 July pulse? What makes the 19 July event distinct to justify it being highlighted in Fig 9?

It is not possible to conclude much from this unless the analysis period is extended at least a few weeks prior to 11 July and a few weeks after 27 July. If that extension shows much lower DO during periods of weak winds, then the evidence will be somewhat more robust. However, I don´t think this adds much to the otherwise pretty solid paper.

The austral winter seasons are influenced by several synoptic events, such as the passage of low-pressure systems, migratory high-pressure systems (Pérez-Santos et al., 2019), and, more recently, the reported arrival of atmospheric rivers (García-Santos et al., 2025), which favor intensified wind mixing. We investigated the window during which oxygen increased (ventilation pulses), and in this case, atmospheric rivers coincided with the ventilation pulse starting on 19 July 2019. An atmospheric river is identified when the integrated vapor transport (IVT) exceeds 250 kg m s^{-1}. In this case, we propose that this event contributes to the gradual increase in oxygen. Still, we agree that the relationship is not direct, likely because gas fluxes respond to wind and water-column conditions, creating an opportunity for future research to clarify our understanding of synoptic events that promote ventilation.

Likewise, I find the association with ENSO plausible, as shown in Linford et al (2023), but the way it is being presented here is somewhat speculative. First, late 2023 and early 2024 also presented El Nino conditions, and no apparent anomaly at the mooring location. Second, are the concurrent warming (beyond 10°C and freshening under 34) observed during the 2016 event compatible with the alleged advection of SAAW? In line 792 you state that the lower boundary of ESSW deepened in 2015-2016, I presume you mean the upper boundary of ESSW.

Thank you for your comment. We agree that part of the low-frequency signal variability could be related to remote drivers, such as El Niño. While this relationship likely need to be further studied and verified, it is beyond the scope of this work. Therefore, we have removed Section 4.3 ("Interannual variability") from the manuscript to focus on our main contribution: evaluating seasonal and short-term ventilation patterns in the study region.

In contrast, section 4.4 is very good.

Overall, the Discussion is rather long, a bit wordy and repetitive. Shortening this section will help conveying a clear message. I also suggest downplaying the potential role of pulses in sustaining the duration of the longer-term periods. Note that, 3 out of the 4 longest periods (8, 9, and 16), all exceeding 100 days, present no pulses, indicating that pulses are not a necessary condition for the development of extended ventilation events.

Adressed in new discusion

Terminology and description of events:

The short-lived ventilation events are rather loosely referred to as “events” (l. 390, 391), pulses (pretty much elsewhere), please be consistent. In general, to avoid confusion, I suggest avoiding referring to “events” without stating “period” or “pulse”. This should be checked throughout the manuscript.

Corrected

The text refers to specific events by numbering them sequentially. To aid the reader following up the arguments, each period and pulse should be labeled in Figure 3.

All periods have been labeled on the figure; however, the pulses were too numerous to individually number.

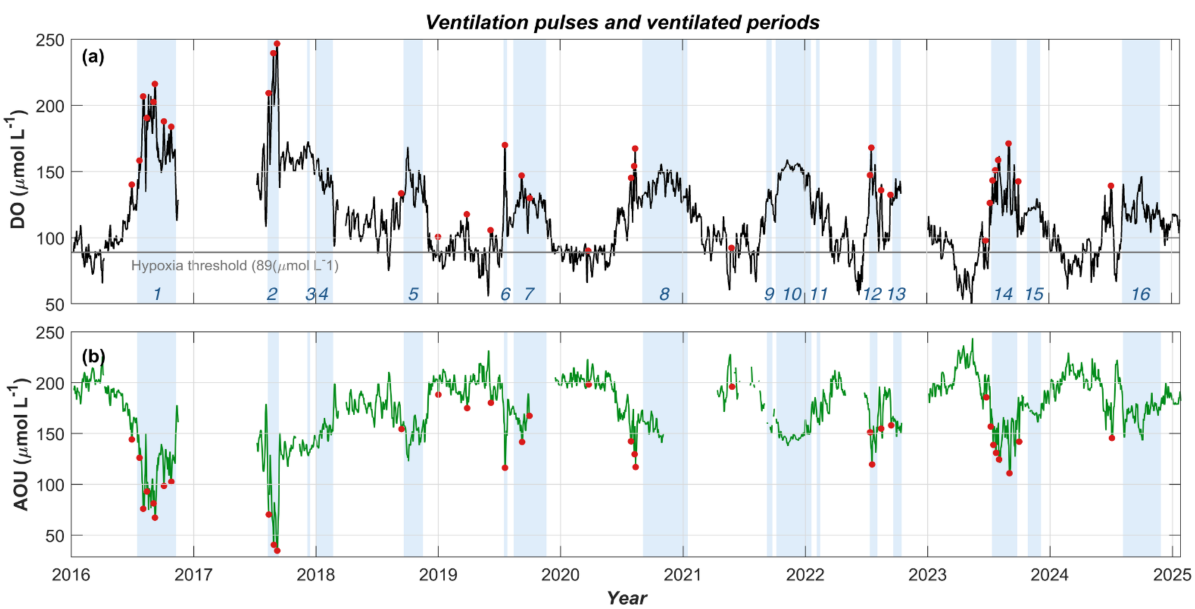

Figure 5. Daily averaged time series of (a) dissolved oxygen (DO) and (b) apparent oxygen utilization (AOU, µmol L⁻¹) at ~170 m depth in Guafo Mouth (2016–2024). Light blue shading and numbers from 1-16 indicates ventilated periods identified from the filtered daily DO series. Red circles mark ventilation pulses, represented by the local DO maxima in panel (a) and the corresponding AOU minima in panel (b). The horizontal grey line in panel (a) indicates the regional hypoxia threshold (89 µmol L⁻¹).

Description of data processing:

The data being reported are of great scientific value. I commend the authors and responsible institutions for maintaining such a long time series (2016-2024). However, maintaining science-quality data for such a long period poses numerous challenges due to bio-fouling and sensor drift. You provide some information about the oxygen calibration procedures. It would be useful to see some sensor-Winkler comparisons. Conductivity sensors are also quite sensitive. Inclusion of more calibration details is of fundamental importance. This can be added as supplementary material.

We agree with the comment. In lines 182-185 of the original manuscript, information about Winkler calibration was included.

"Additionally, in 2016, Winkler samples were collected seasonally along the water column. This experiment was repeated in October 2022 (Linford et al., 2023). Comparison between the iodometric titration (the Winkler method) samples and the DO time series of the GM showed good agreement with a correlation coefficient of 0.97, and a centred root mean squared difference (RMSD) of 11.2 $\mu mol\ L^{-1}$".

For most of the analyses you use filtered hourly data re-sampled at daily frequency, yet, the filtering information is unclear and is a bit scattered. You first state that data were collected hourly and daily averaged "… for reducing high-frequency variability …" (l. 166). Later on (l. 201) you state that all hourly data were low-passed filtered with a cutoff period of 48 hours "…to suppress diurnal and semidiurnal tidal variability…". I presume these two lead to similar results, but which one was used?

We thank the reviewer for pointing out this ambiguity in our methodological narrative. The short answer is that both methods were utilized, but for distinctly different analytical purposes, and we agree that we failed to make this distinction clear in the original text.

To clarify:

Simple Daily Averages (Section 2.2): A straightforward daily average of the hourly data was used to construct the general time series plots and to calculate the long-term monthly climatologies (e.g., Figure 2 and Section 3.1). This approach is entirely sufficient for describing the seasonal and interannual background trends.

Low-Pass Filter (Section 2.3.1): However, for the specific mathematical identification of short-lived synoptic ventilation pulses and periods, a simple daily average is insufficient because residual tidal aliasing (such as isopycnal heave) can artificially trigger the detection thresholds. Therefore, specifically for the pulse/period detection and their associated hydrographic anomalies, we applied the more rigorous 48-hour cosine–Lanczos low-pass filter to the hourly data prior to daily resampling.

We have now updated Section 2.2 to explicitly state that the simple daily average was used strictly for the general climatologies, and we have rewritten the opening of Section 2.3.1 to explicitly connect and justify why the stricter low-pass filter was necessary for event detection.

Then in l. 290 you state that hourly wind data were daily-averaged. Finally, in l. 324 you state that all data were also high-passed filtered with a cutoff frequency of 250 days to focus "on the synoptic-to-intraseasonal variability…". Please provide all filtering and smoothing information in a single subsection within Section 2 and then always clearly state whether each of the analyses is carried out on the low-pass filtered daily-averaged data, the original hourly records, or the low-pass and high-pass filtered data.

We sincerely thank the reviewer for pointing out these methodological ambiguities. We agree that our original text did not properly untangle how the datasets were processed and compared across the different analyses. Regarding the recommendation to consolidate all filtering information into a single subsection, we respectfully opted to keep the specific filtering steps within their respective methodological subsections (e.g., Section 2.3.1 for event detection, Section 2.5 for spectral analysis). Because each filter was tailored to a completely different analysis, separating the filter description from the analysis itself would strip away the physical justification and potentially cause further confusion. However, we completely agree that we must explicitly state which version of the data is used in each section. We have meticulously reviewed Section 2 and explicitly labelled the dataset used in each step:

- Oceanographic Time Series & Climatology (Section 2.2): We clarified that a straightforward daily average was used to construct the long-term climatologies.
- Ventilation Pulses & Periods (Section 2.3.1): We clarified that, to remove isopycnal heave, the event detection strictly required the 48-hour low-pass filtered data (resampled to daily).
- Wind Data (Section 2.4.1): We added text explaining that because ERA5 atmospheric data do not contain oceanic tidal harmonics, a low-pass filter was unnecessary. Thus, a simple daily average was used to isolate synoptic scales and compare against both oceanographic processing streams.
- Spectral Analysis (Section 2.5): We explicitly stated that the 250-day high-pass filter was applied strictly to the daily-averaged data (from Section 2.2) and exclusively for the wavelet/coherence analyses to remove the dominant annual cycle.

We believe these targeted clarifications throughout the text successfully resolve the confusion while preserving the physical rationale behind each analytical step.

You describe how data gaps were dealt with. Did you check how sensitive the period durations are to the choice of the 7-day gap filling maximum? Some information could be added as supplementary material.

First, we would like to make a brief terminological distinction to avoid any confusion: the 7-day threshold does not refer to interpolating missing data from the instrument record (data gaps). Instrument data gaps were handled separately (as described in Section 2.5). Rather, the 7-day threshold refers strictly to our event consolidation criterion (Section 2.3.3), where consecutive ventilated periods separated by brief intervals of ≤7 days (where oxygen temporarily dropped below the threshold) were merged into a single coherent physical episode. Regarding the sensitivity of this consolidation threshold, the reviewer raises an excellent point. We did evaluate the sensitivity of the period durations to different thresholds (e.g., 3, 5, and 7 days). We found that using shorter thresholds (e.g., 3 days) resulted in the artificial fragmentation of otherwise continuous winter ventilation events. For example, during persistent downwelling seasons, the wind forcing often relaxes for 4 to 5 days before the next synoptic storm arrives. A 3-day threshold would split this single seasonal oxygenation response into multiple separate events, misrepresenting the physical continuity of the process. The 7-day consolidation threshold was deliberately selected because it aligns with the natural synoptic timescale of atmospheric storm passages in the region (typically 5 to 8 days). Following the reviewer's suggestion, we have added a sentence in Section 2.3.3 acknowledging this sensitivity.

Parts of the text appear to be written for experts in the region, please consider readers unfamiliar with the region (see minor comments).

Corrected.

Please check the mooring location provided in the caption of Figure 1 and also in line 162, it does not seem to match the position indicated by the yellow circle in Figure 1c. Note that most of the paper is based on the analysis of data collected at this site.

New Fig. 1 added.

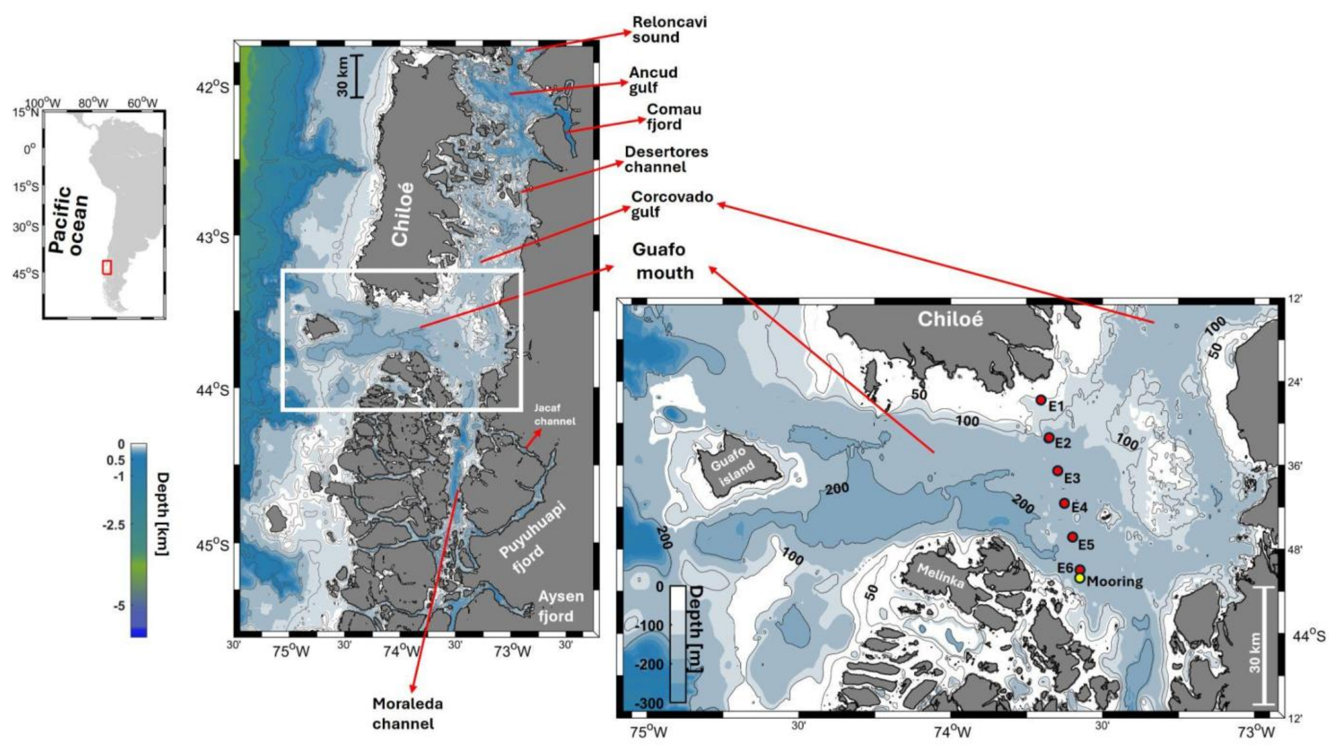

Figure 1. Location and bathymetric features of the study area in the northern Patagonian fjords. (a) Continental map of South America indicating the location of the study region (red box). (b) Regional map (white rectangle delineates the Guafo Mouth area). (c) Bathymetric map of the Guafo Mouth and adjacent zones. Red circles denote the locations of the oceanographic stations (E1–E6) used to generate the seasonal vertical profiles. Yellow point (43° 51' 44.10"S,

73° 33' 24.10"W) also marks the specific position of the mooring. Color scales in (b) and (c) represent depth in kilometres and meters, respectively. Depth data were downloaded from ©GEBCO Compilation Group (2025).

You find a fairly tight relationship between monthly DO and potential density variability (Fig. 2). It seems very useful to explore if this holds for the daily data and also to present DO vs Θ, SA, and σ scatter plots. These may also provide somewhat more robust evidence on the near-bottom water masses, which the manuscript lacks in its present form.

We added a new figure to the manuscript.

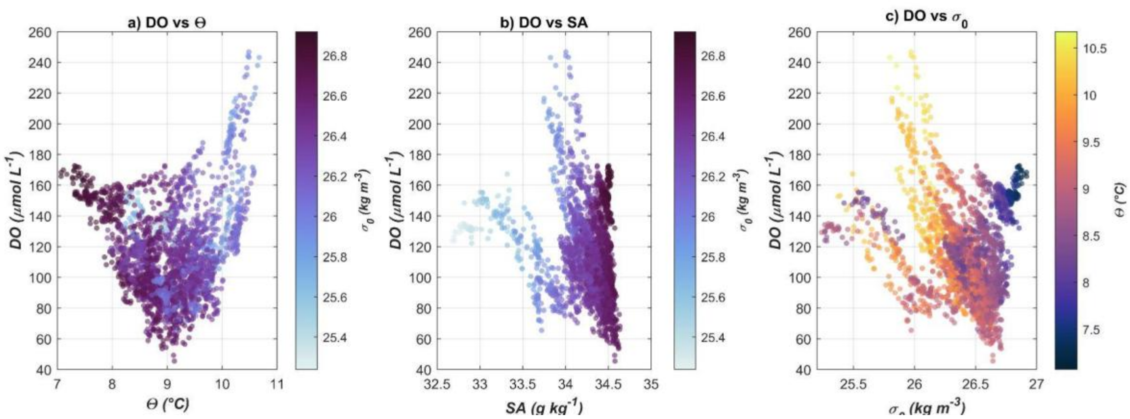

Figure 4. Scatter plots showing the relationships between daily dissolved oxygen (DO) and hydrographic properties at ~170 m depth in the Guafo Mouth (2016–2024). (a) DO versus Conservative Temperature, with the colour scale representing the potential density anomaly. (b) DO versus Absolute Salinity (SA), coloured by density anomaly. (c) DO versus density anomaly, with the colour scale representing Conservative Temperature.

Minor points Camila

Conservative Temperature and Absolute Salinity should be capitalized throughout the manuscript

corrected

Line 15, two “essential” in this sentence, consider rewriting

corrected

Line 22, confusing sentence, consider rewriting: “Data from ERA5 reanalysis and from an oceanographic platform…”

corrected

Line 29, for clarity please replace “northern wind stress” by “northerly wind stress”

Corrected in all similar cases

Line 47, delete bracket before Hannah

corrected

Lines 59 and 61, state latitudes for the “Patagonian fjord system” and the “southern coasts”

corrected

Line 67, Guafo region, refer the reader to Fig 1

corrected

Line 128, clarify that the range 31-33 g kg^{-1} corresponds to Absolute Salinity

corrected

Line 297-298, no need to repeat the latitude dependency of the Coriolis parameter in the same sentence

corrected

Line 313-316, you used a 50 km cross-shore length-scale to derive the vertical transport per unit coastline based on the estimated Ekman pumping velocity. Please discuss how does this length scale compare with the characteristic width of the downwelling regions shown in Fig 6.

We clarified the information in the text and added a new sentence as follows:

“The Ekman pumping velocities were integrated up to 50 km offshore, as proposed by Pickett and Paduan (2003) in the California upwelling system. They were subsequently applied by Pérez-Santos et al. (2019) to quantify Ekman transport and pumping along the oceanic coastal waters adjacent to the Patagonian Fjord system. In the present manuscript, the Ekman pumping velocity was approximately equal to the width of the downwelling region reported for the austral fall and winter seasons.”

Line 343, “Environmental variables were averaged within a ±10 d window relative to the pulse onset …”. I first found this very confusing, how would averaging the data within a ±10 d window help understand the dynamics a short-lived (2-4 days) pulse? Please consider rewriting so that the reader understands that this is not a time-average within each pulse but rather a time-average of each daily value across all pulses.

We have rewritten this section to explicitly clarify that we performed a composite analysis. The text now clearly states that we synchronized all identified pulses at their onset (defined as day 0), and then calculated the ensemble mean of the environmental variables across all events for each daily lag within a -10 to +10 day window, rather than temporally averaging within a single pulse.

Line 373-374, “The seasonal cycle was evident, with a stronger amplitude (~3 °C at 170 m depth), as observed in the DO time series.” I don´t understand this statement, the seasonal amplitude is larger than what? And what has the DO time series to do with this?

Definitely a tipping mistake, thanks for noticing. The sentence was erased.

Line 379-380, As you point out, the lack of phase alignment between Conservative Temperature and oxygen variability indicates that the oxygen changes are not solubility driven, note that this is also evident from the nearly perfect match between DO and %DO saturation.

Thanks for pointing out this excellent additional piece of evidence. We have modified the text in Section 3.1 to explicitly state that the “nearly perfect match between DO concentration and % DO saturation” (as seen in Figure 2a) provides further confirmation that the oxygen variations are physically driven by water renewal rather than by temperature-driven solubility changes.

Line 383, 385, “occurred…”, not “was occurred”

corrected

Line 393, replace “… longer moments …” by “… longer duration events …”

corrected

Line 451, “In terms of wind stress intensity, a northward maximum area was observed during austral summer …”. Since you are referring here to wind stress I strongly suggest you stick to the meteorological convention, from where the wind blows, so this would be “… a southerly maximum area …”. Likewise, in the following line I suggest replacing “… southward wind stress …” by “… northerly wind stress …”. Similar changes needed in the caption of Figure 4. Please check throughout the manuscript as this can lead to misinterpretation of the results.

corrected

Line 493, “… utilized to obtain the Ekman time series of the transport …” replace by “… utilized to obtain the time series of Ekman transport …”

corrected

Line 494, “The zonal Ekman transport (U_e), projected onto the cross-shore direction …” Do you really project the zonal Ekman transport? It is unclear what would the cross-shore direction be, depending on whether you use the mainland coast or the southern coast of Chiloe Island.

Please elaborate and clarify whether that direction falls close to the main cross-shore flow pathway

We clarified this information in the new section 2.4.3 of the manuscript.

Line 587, unclear what is meant by “unfiltered DO”. Are you referring to the low-pass (2-day cutoff) or the high-pass (250-day cutoff)?

By “unfiltered,” we meant that the data was not subjected to any filtering process at all (neither the low-pass nor the high-pass filter). To make this absolutely clear to the reader, we have modified the sentence in the text to explicitly state: "Initial wavelet analyses of the unfiltered DO record (i.e., without any low-pass or high-pass filtering) revealed...".

Line 681, “… upper annual media” (?)

It meant to be “mean”, corrected.

Line 692, the low salinity may be “a result of" rather than "favor" ventilation

Addressed in new discussion

Line 758, although pulses “can occur at almost any time of the year”, Fig 3a shows that out of 35 pulses only about 10% occurred during periods of very low DO.

It's a good point. A substantial number of pulses also occurred outside grouped ventilated periods. But rather than occurring randomly throughout the year, the majority of these isolated events were still confined to the typical ventilation window (July to November). Only a small fraction (7 pulses) occurred outside this season, all of which were characterized by low oxygen and favorable wind conditions (downwelling favourable)… We adjusted the affirmation.

Lin 786, according to Table 1, period 8 lasted 135 days and was therefore longer than period 1.

Yes, the sentence was changed to the “second largest”.

Figures

Figure 1: bottom topography is of fundamental importance. A brief description is provided in section 2.1. Yet, it is difficult to determine the depths of the channels and slopes. Please clearly indicate and label a few relevant isobaths in panel b. Also please make sure that all (numerous) geographic features mentioned in the text are marked in Figure 1.

There is no need to repeat the observed variables in the Figure caption. Please explain the meaning of the red circle.

corrected

Figure 3, What exactly is it meant by “High-resolution time series”? Are these unfiltered hourly data. If these are filtered daily averaged data I suggest just stating it that way. I strongly suggest labeling each period and pulse with their respective numbers. It is otherwise very hard to follow parts of the text.

It is filtered daily averaged data, corrected in caption and periods labeled.

Figure 4, caption (line 458) replace southward by northerly and northward by southerly

corrected

Figure 9a: Please use either µM or µmol/l throughout

We edited the figure.