Review #1

This manuscript has a wealth of information relative to fjords in the northern Patagonia region. It explores, using many diverse datasets, the physical and biogeochemical properties of these fjords and evaluates the drivers of low oxygen/hypoxia. However, I think that some of the data were not described appropriately or used to their full extent. In other words, even if the manuscript shows a lot of data/plots/numbers, some of the main conclusions remain qualitative (or at least, it feels that way). Furthermore, while the text is easy enough to follow for the most part, many parts are unclear. I do believe that these issues can be fixed by a careful review/improvement of the writing (and maybe in the structure of the text). Below, I share the rest of my concerns/comments, divided by major, secondary, and minor. I believe that this manuscript could be accepted for final publication after these major revisions are undertaken.

Major comments and concerns:

* The use of the term "deoxygenation" is misleading/confusing in this manuscript (even in the title). In my mind, and I assume that in many others working in the field of ocean deoxygenation, this word implies a trend; i.e. the loss of oxygen with time in the ocean. The first few sentences of the abstract do use the term in this sense. However, in most of the manuscript "deoxygenation" is used to refer to "loss of oxygen leading to hypoxia/LDOW". I strongly suggest to revise the use of this term. For instance, I believe a more accurate title would be "Oceanographic processes driving low oxygen conditions inside Patagonian fjords" or something along those lines.

We agree with this comment and change the title of the manuscript "Oceanographic Processes Favoring Deoxygenation Inside Patagonian fjords" by "***Oceanographic Processes Driving Low Oxygen Conditions Inside Patagonian Fjords***". Additionally, we eliminated from the text the reference term of Deoxygenation and change to loss of oxygen, etc.

* I commend the authors for adding models to their toolkit. However, the model evaluation is lacking. Only salinity is evaluated by qualitatively comparing model vs observed transect plots – and these plots do not even show the key isohaline (33 g/kg) mentioned in the text! I understand that this is not a modeling paper, but the model does add an important component to the analysis (it is key for the discussion related residence times). Therefore, more effort is needed to show that the models represent the circulation well. The evaluation of other variables must be added (temperature as a bare minimum, but also sea surface height, and ideally, currents). If possible, include quantitative metrics (in addition to qualitative plots).

Thank you for your constructive feedback. We agree with your suggestions regarding the model evaluation.

We'll include temperature, sea surface height, and currents in our revised model evaluation, which will certainly strengthen its representation of the circulation. Additionally, we'll incorporate quantitative metrics along with qualitative plots to offer a more robust model assessment.

Thank you for your keen observation regarding the key isohaline (33 g/kg). While it's true that this isohaline doesn't visually appear within the model domains, we believe the model is still successful in reproducing the spatial structure, indicating that the processes controlling the salinity transport within the fjords are incorporated.

While the model may not replicate these processes with absolute accuracy, it enables us to understand the fundamental physical transport mechanisms at work. We will make sure to provide a clearer explanation of this in our revised manuscript.

* The analyses and discussion many times jump from describing long-term seasonal means to snapshots (ie, data from specific expeditions). These changes are not done/explained with care in the text, which is misleading or confusing at times. I think the flow of the text needs improvement to make sure it is clear when a conclusion is taken from a long-term mean or a snapshot. For example, in the Results section, Figure 4 (snapshot) and its description are found between a larger discussion of Figs 2, 3 and 5 (long term means); I feel that if Fig 4 and associated description were removed (which do not add a lot anyways), the flow of the text would be much improved. Other improvements are likely needed in the Results and Discussion sections.

We organize the Results section to present first the descriptions of the long-term and next the snapshot. We believe that the examples of areas where extreme low DO waters were detected (Fig. 4 and new Fig 6) is important to the context of the manuscript.

* The authors calculate the differences in T, S, DO, NO3 and PO4 between a cruise in March 1970 and another in February 2021. They calculate trends with just these two time points and discuss deoxygenation (in the sense of long term decline of oxygen concentrations). Calculating trends and discussing long term changes with only two data points is a stretch. There is no way to prove that these differences are indeed trends and not due to interannual variability. While I do agree that it would be incredibly valuable to show trends and long-term DO decline, it has to be done properly by using a longer time series. I truly hope the authors can gather enough data from their dataset to calculate trends with more data points. But if for any reason they cannot, I strongly recommend to remove this trend analysis. If the authors want to keep Fig 6 in the manuscript, then the text should clarify that changes shown are only informative and that trends cannot be computed with only 2 points in time should be made explicit.

We eliminated the Figure and the description from the text.

* The figures using interpolation/extrapolation need careful revision. In particular, Figure 2 shows ESSW waters (as defined in the salinity panel) with really cold temperatures in spring, which I believe is just a result of incorrect extrapolation (no observations on those values under 8 degreesC).

We better the interpolation of Figure 2.

* The POC partition into autochthonous and allochthonous is not clearly explained. There are typos and different subscripts in the text and in the equation (lines 207-212), making the explanation hard to follow. Furthermore, in the equation the term chosen is POCterr, while later the text refers to POMalloc and the figure 10c shows POCorg (at least that's what I can read in panel 10c with 300% zoom – the caption reads POCallo). Furthermore, the use of carbon and nitrogen isotopes has to be properly described in the methods section (beyond the description of how they were measured). This change, along with improvements in the descriptions and discussions related to the isotopic distributions, will make it easier to understand the conclusions drawn in the Discussion section.

We edited the methodology section and improved the Results and Discussion sections of the Biogeochemical variables and analysis.

* The fontsize in most figures is too small. I needed 200% zoom to read the tiny text in some figures. For instance, Figure 1 might need to be reorganized to 2 columns/3 rows, such that it can take more of the page and doesn't need to be shrunk to fit the width of the page.

We edited the fontsize of many figures and changed the Figure 1 by a new figure fallowing the recommendations.

* I found several places where the > and < symbols were incorrectly used (e.g., > used for "less than").

We corrected the symbols in the manuscript.

Secondary comments:

* The last sentence of the abstract is very vague. I suggest to replace it by a more meaningful and concise conclusion.

We deleted the last sentence of the abstract and added a new sentence.

* The word "parameter" is used in this manuscript as "variable", as in temperature being a parameter. While it is not an uncommon use, I would suggest to favor the use of "variable" instead. In ocean modelling and more generally in math, a parameter is a component of the equation that one uses to try to represent the variable.

We changed the word "parameter" to "variable" across the manuscript.

* L100: "Finally, most published manuscripts hypothesize and discuss the processes favoring deoxygenation inside Patagonian fjords but never show any quantification": Besides the already mentioned issue with the term "deoxygenation" (which here could be replaced by "leading to low oxygen conditions"), the text "but never show any quantification" sounds unnecessarily harsh and dismissive of past publications. I'm sure this was not the intention, but I do suggest to find other wording. Furthermore, in the Discussion Section the reader finds a long list of references given regarding the study of "processes impacting this fjord

system, such as the hypoxic conditions due to the influence of the ESSW". None of these studies ever showed any quantification?

We agree with you and change the idea of quantification by words as: evaluate, estimates, analyses, scrutinizes, etc.

* I would appreciate some clarity on how the transects through all fjords were plotted as a continuous section (eg, Fig 2, 3, 5). It seems to me there should be discontinuities, unless data are repeated (e.g., from mouth to head and then back) or arbitrarily interpolated. For instance, starting in San Rafael Lagoon we can plot a continuous transect to the head of Cupquelan fjord; however, to go then we need to go back to the mouth of Cupquelan to start plotting the way to Quitralco fjord. My interpretation is that the authors lined up the stations of each fjord and then interpolated the whole dataset. But, if that's the case, I question whether the interpolation between the head of a fjord to the main channel (or mouth of the next fjord) is meaningful. Particularly, this interpolation may confound the interpretation of horizontal gradients seen in these transect plots. I'd suggest to blank out the locations where the transect is not continuous.

The data presented in the new Figures 2, 4, and 5 is no continue and included the passed over land market in white rectangles.

* I suggest to move section 2.3 "Satellite images" after section 2.4, so the biogeochemistry section comes right after the biology one.

We moved the section 2.3 to 2.4.

* Section 2.6: A brief description of the model would be beneficial for those not familiar with MIKE 3 FM. Is it hydrostatic? Is it finite elements or volume? What vertical coordinate system does it use? Also, please note that the reference list is missing "DHI (2019)", which is the only citation given for the reader to find out more about the model. Lastly, it would be interesting to know why you have two domains (that seem to overlap at their N and S open boundaries), instead of just one (note: by no means I imply that you should have one big domain; but a modeler wonders if the reason is computing power/efficiency, legacy, etc).

Thank you for your suggestions. We will provide a brief description of MIKE 3 FM, including its hydrostatic nature, use of finite volume elements, and the vertical coordinate system it employs.

We apologize for the missing "DHI (2019)" reference and will ensure to include it in our revised manuscript. The use of two overlapping domains is due to budget constraints and computational capacity that led to staggered development over time. This detail will be included in our revised manuscript.

* Water mass analysis in section 3: 1) The description (L276-280) gives the impression that the Estuarine Water (EW) is a water type associated with the freshwater from the San Rafael Lagoon, which then moves north. However, I think that the surface EW identified at the different fjords is likely originated from the freshwater inputs in each fjord. 2) Fixing the

previous point will also improve the description of MSAAW. The text describes MSAAW as a mixture of EW and SAAW, and figure 2c-d shows it as one water mass; however, the northern and southern 'branches' of MSAAW show quite the different temperatures. The latter might be explained by EW in the north being associated to the freshwater in the northern fjords (rather than the ice melt from the Lagoon).

We edited the results presented in this section and added: *"Moreover, the EW was also observed in the northern domain of the study area, especially at the surface layer in the Reloncaví system and the Comau and Reñihue fjords, contributing to the formation of the MSAAW. In this region, EW's origin was mainly due to the freshwater supply by river discharge. We identified two different sources favoring the EW's origin and the MSAAW's formation. 1) The combination of ice melting from the San Rafael with river discharge in the southern region and 2) the freshwater supply by river discharge in the northern region, both sources contributed to the difference in conservative temperature shown by the MSAAW".*

* Further on water mass analysis: The authors do not discuss temperature when defining water masses (which is OK for the most part). However, I was surprised by seeing the ESSW colder than SAAW in Figure 2a,b. I think it would be valuable to add a brief description of the temperature ranges for these water masses and/or why an equatorial water mass is colder than a subantarctic one in this region. Note: not being an expert in this region, I did a quick search and found ESSW ranges of 13-14C (Silva et al 2009) and SAAW between 7-9C (Palma and Silva 2004), so now I'm even more curious about the temperatures in your figures.

We added a new table and figure (Table 4 and Figure 3) to present and quantify the water masses in the study area. The results showed that ESSW was in general colder than SAAW as was obtained by Linford et al., (2023, Table 3). The difference between our results with Silva et al., (2009) and Palma and Silva (2004) could be attributed to the depth range used in the calculation of water masses.

* GPP:CR analysis: I am wondering if there is any chance that the spring bloom is in September/October and that your sampling missed it. Could you comment on this?

Yes, the spring bloom could have occurred in September/October or even earlier... during August (winter late bloom, Montero et al. 2017) and we might not have recorded it. However, sampling campaigns were carried out within the productive season of the annual productivity cycle, and GPP and CR rates showed values that agreed well with measurements reported in previous studies for the Patagonian fjords during the productive period.

We have now added a short paragraph where this situation is indicated.

* CDOM: There is a clear (and striking) pattern of high-low-high CDOM in the northern side of the region of study (>~800km in Fig 10g). However, the authors only mention the high CDOM at the very north (Reloncavi) and then the low CDOM south of Jacaf Channel (L415-416). This should be fixed. That said, the authors do not discuss CDOM beyond the

description of the fig 10g. I believe that the authors should either add a discussion on what we learn through these CDOM data or remove the panel and associated description.

We edited the Results and Discussion sections in terms of CDOM descriptions.

Minor comments/edits:

L36: mechanismS

We change “mechanism” by “*mechanisms*”.

L50: While COASTAL hypoxia

In the manuscript of Breitburg et al. (2018) a global map of hypoxia that occurred in the Oceans and coastal waters was presented. We edited the sentence *“Hypoxic conditions and deoxygenation have expanded globally over the last decade along coastal waters and oceans”*.

Breitburg, D., Levin, L. A., Oschlies, A., Grégoire, M., Chavez, F. P., Conley, D. J., ... and Zhang, J.: Declining oxygen in the global ocean and coastal waters, Science, 359, 6371, doi:10.1126/science.aam7240, 2018.

L57: replace “y” by “and”

We replaced “y” by “and”, e.g., *Díaz and Rosenberg, 1995.*

L65: this sentence is confusing as is. I believe you meant “In a Patagonian fjord used in recreational fishing for rainbow trout (Salmo gairdneri),”

We edited the sentence.

L80: this sentence is unclear. “the Eastern South Pacific (ESP) OMZ extends poleward, diminishing its influence on the adjacent coast of the Patagonian fjord system”. Do you mean something like “, decreasing in strength and size to the south near the Patagonian fjord system”? No need to use my words, but please describe better what you mean by “diminishing its influence”.

We edited the sentence.

L81 and other places in the manuscript: Linford et al (2023) is under review. Might need to change this reference if the paper is not accepted by the time this one is.

The manuscript of Linford et al. (2023) was recently published. We edited the references in the reference section.

Linford, P., Pérez-Santos, I., Montes, I., Dewitte, B., Buchan, S., Narváez, D., et al.: Recent deoxygenation of Patagonian fjord subsurface waters connected to the Peru–Chile

undercurrent and equatorial subsurface water variability. Global Biogeochemical Cycles, 37, e2022GB007688. https://doi. org/10.1029/2022GB007688, 2023.

L87: Guafo mouth (FIG. 1A),

We edited the text.

L89-90: “far from the influence of ESSW”: “far” is misleading here, since many of these fjords do not seem far from each other. According to Silva & Vargas (2014), they are behind sills that prevent the ESSW flow into the deep waters of the fjords.

We change the sentence “Nevertheless, these conditions are also found in other areas far from the influence of ESSW” to “*Nevertheless, these conditions are also found in other areas where sills block the pass of the ESSW…”*

L102: “with salmon aquaculture occupying the first position”. I understand what you mean, but this doesn’t sound the best way to put it (in English).

We edited the sentence “with salmon aquaculture occupying the first position, with a national production of ~1,000,000 tons of salmon in 2019” to “*The Patagonian fjord ecosystem is under substantial continued economic pressure due to the salmon aquaculture and other economic activities (Billi et al., 2022)”.*

L103: “The northern Patagonian fjord (a region similar to the study area; Figure 1b, 1c)”: the paper refers to the area of analysis as northern Patagonian fjords, so I’m confused about “a region similar to the study area”. Are the authors referring to the study area or somewhere else?

We edited the sentence as “*The northern Patagonian fjords (Figure 1b, 1c)….*”

L146: Table 1: what does the * represent next to (Temperature, Salinity) in the HUDSON expedition?

We added the following sentence *“*Temperature and salinity were measured with reversing thermometer and inductive salinometer respectively”.*

L242: how do you provide current and sea level boundary conditions based on CTD stations?! Please explain better your velocity and sea level open boundaries.

We appreciate your insightful feedback. To elaborate further:

Our methodology employs CTD data solely for setting temperature and salinity conditions at the boundaries. For determining the water levels at the open boundary, we implemented a harmonic analysis (as advocated by Pawlowicz et al., 2002), utilizing data from a regional barotropic model (citing Pinilla et al., 2012).

Importantly, the flow data at the boundaries in our hydrodynamic model is prescribed as zero. This results in the current dynamics within the model domain being balanced internally across space and time. We intend to provide additional clarity on this aspect in our revised manuscript.

To enhance the robustness of our model, we are planning to incorporate validation using water level and current series at various points within the domains. This will serve to verify the model's performance and ensure its accuracy across diverse locations.

We hope this comprehensive response addresses your queries and provides you with a clearer understanding of our methodology.

L250-252: this sentence is too long and grammar is a bit unclear towards the end.

We deleted this sentence and presented a new description.

L254: please add a few words on whether the performance of FLOW-IFOP is good/acceptable (rather than just pointing to the evaluation).

We appreciate your input. In the upcoming revision, we will explicitly address the reliability and efficiency of FLOW-IFOP in our study. Additionally, in response to your suggestions, we will incorporate specific metrics to assess the model's effectiveness in Northern Patagonia's principal rivers.

Section 3: it would be good to reiterate here (with just a few words) that the long-term seasonal mean used all data shown in Table 1.

We added e new sentence: "*The long-term annual mean includes all data sets presented in Table 1. This section scrutinizes the behavior of the conservative temperature, absolute salinity, DO, and inorganic nutrients*".

L274: Desertores Pass in not shown in any of the maps, as far as I can tell. Also, add a call to Fig 2a,b at the end of the sentence.

We added the label Desertores Pass to Figure 2a and Figure 2b and edited the sentence.

L277: I think it should say Figure 2c,b

We edited the sentence.

L285-289: please revise the grammar and clarity of these two sentences.

We edited the sentence as follow: "*In the area contained by the ESSW, low DO (LDOW) and hypoxic waters were observed, but LDOW was registered at Reloncaví Fjord, where ESSW was not observed. In general, the Chiloé Inner Sea (CIS) showed a homogenization of the water column, mainly during the fall-winter seasons, in which high DO values (267−312 $\mu mol\ L^{-1}$) and oversaturated waters (< 100% DO Saturation) were registered. Additionally,* *more extensions of the hypoxic conditions and LDOW were registered during the spring-summer seasons (Figure 2e–f)*".

L290: Figure 2 caption: why not say explicitly temperature, salinity and oxygen?

We edited the Figure 2 caption, "*Figure 2. Long-term seasonal mean of (a-b) conservative temperature, (c-d) absolute salinity, and (e-f) dissolved oxygen collected along a vertical section in the northern Patagonian fjord during the fall-winter and spring-summer seasons*".

L308: Magdalena Sound: this location identifier is not clear, as it is not shown the x axis of fig 3. I suggest that you add within the parenthesis already showing lat/lon (which doesn't help much when looking at fig 3), that this sound is found between the Puyuhuapi Fjord and Jacaf Channel.

We edited the sentence and added the label "Magdalena Sound" to Figures 1b, 1c, and 1e: "*The analysis of hypoxia and LDOW conditions in the northern Patagonian fjord system highlighted the presence of two areas with water bodies with these characteristics, e.g., the Puyuhuapi-Jacaf and the Reloncaví regions. Moreover, Magdalena Sound (44.6° S / 72.9° W, located between the Puyuhuapi Fjord and the Jacaf Channel, Figure 1) showed shallower hypoxia over the entire Patagonian region*".

L308: also, in this line it says "over the entire Patagonia region". Do you mean over the entire region of analysis (ie, northern Patagonia fjords) or really over the entire Patagonia (for which you are not showing results, so you would have to add a citation)?

We edited the sentences: "*Moreover, Magdalena Sound (44.6° S / 72.9° W, located between the Puyuhuapi Fjord and the Jacaf Channel, Figure 1) showed shallower hypoxia over the entire northern Patagonia fjords*".

L315: caption missing C and F in the first two sets of parenthesis

We edited the sentences: "***Figure 4***. *Vertical section of dissolved oxygen carried out along (a-c) Magdalena Sound (-44.65° S / 72.87° W) and (d-f) Quitralco Fjord during November 2016 and November 2020, respectively*".

L319: BruNt

We changed "Brut" to "Brunt".

L320: I believe you mean <. Also note that the text refers to the mixing in the whole water column but the figure only shows the top 50m. I suggest to explicitly mention that the figure zooms into the top 50m.

We edited the text and changed the figures to show the water column.

L324: I believe you mean >

We edited the text.

L380: “the observed high concentration of sediments was due to the presence of a diatom bloom mainly formed by *Skeletonema costatum*”: do you mean SPM instead of sediments? Is the high SPM outside of the fjord mouth in Fig 9a representing the bloom occurring at the time (then, SPM would not be necessarily sediments) or did the bloom occur earlier and the high SPM represents resuspension of the previously sedimented diatoms? A clearer explanation is needed

The diatom Skeletonema costatum has a silica shell which has spectral characteristics that are complex to determine from satellite measurements (which typically only measure a few spectral bands). Complex waters, with a mix of sediments, chlorophyll, and other particulates, are particularly challenging for remote sensing. In Figure 9a we can appreciate whitish watercolors getting out of the fjord, and, more to the south, greenish waters which correspond to the Skeletonema costatum bloom region. So these are rather two separate water masses, and it would be difficult to assess, based on a cloudy and 5-day time step satellite time series, which one would occur before.

(the text in the manuscript, starting in line 380, should be changed to the following, for more clarity: “In the southeast part of the Reloncaví Sound, the observed high concentration of sediments was due to the presence of adiatom bloom **(seen as greenish waters in the southeast part of the Reloncaví Sound),** mainly formed by Skeletonema costatum, which attained concentrations of more than 5 million cells L -1within the fjord (Figure not shown)”

L410: >

We edited the text.

L412: showed light what? Overall, I think the paper needs a clearer description (in the methods section perhaps?) of what the isotopic distributions mean/what information we extract from them.

We changed the sentence “At depth, the organic matter showed light relative to the surface (Figure 10e)”, to “*At the deep layer, the organic matter showed relatively light values compared to the surface layer*”.

L453: need to mention Fig 12 at the end of the first sentence. Then, you can remove the sentence in Line 454-455 (“This result…. current speed”).

We edited the sentence.

L458: DEEP flow

We edited the sentence.

L486: "processes favoring hypoxia and the reduction of DO, such as primary production". Please replace "reduction" by "decrease" or "decline" (I do not think you mean the reduction of DO to water or hydrogen peroxide). Also, the second part of that line is shocking as written, because primary production does not consume DO. Maybe you mean eutrophication? If not, please improve the sentence and explain the connection between PP and hypoxia/DO decrease.

We changed the sentence for: "*Furthermore, other studies have contributed to the understanding of processes favoring hypoxia and the decrease of DO, such as organic matter degradation and community respiration, associated with the abundance of different phytoplankton species*".

L510: "production": mixing does not produce oxygen. Replace by "ventilation", "reaeration" or a similar concept.

We changed the sentence for: "*This explains the importance of this area for DO ventilation and redistribution in the northern Patagonian fjords*".

L513-514: "even though the biotic and abiotic processes that occur in every body of water respond to the biogeochemical cycles to some extent": I do not understand this phrase. BGC cycles ARE a combination of biotic and abiotic processes…

We deleted the sentence.

L526-527: "but there is no evidence of gradient of nitrogen isotopic signals": is this a conclusion from the data presented here? Looking at fig 10f, Chiloe Inner Sea seems to have higher values than regions to the south.

We edited the sentence: "*but there is no evidence of gradient of nitrogen isotopic signals before this study. Nevertheless, the pronounced phytoplankton bloom resulted in an enrichment of $\delta^{13}C$ in SPM (Figure 10), similar to observations made in other fjords around the world (Remeikaite-Nikiene et al., 2017). In the same way, the enrichment of $\delta^{15}N$ of SPM results in primary production evidence that, in at least some regions of the fjords (e.g., Chiloé Inner Sea), the isotopic signals it was of mainly autochthonous origin (Figure 10)*".

L529-535: please improve writing, since sentences are hard to follow. Also, I don't think that is was clearly explained how the results suggest that "the cause of the variability in C and N isotope fractionation was biological processes"

We clarified the sentence in the Discussion section.

L538: "maintaining"? I'd think it would be "adding", since "maintaining" implies that there is nutrient consumption at these depths, which is not likely.

We edited the sentence.

L540-542: "This process seems…": this sentence is confusing. "This process" refers to the accumulation of nutrients through remineralization, but then the sentence ends on how organic matter in the southern region was retained and sedimented, leading to LDOW and hypoxia. Also, while I agree with the overall suggestion of how the hypoxia was generated, I think the explanation is speculative, rather than well justified by the data (eg, figures 2 and 3, which are used as reference, have no information on the organic matter; there is no information on sediment oxygen consumption, beyond the fact that hypoxia was observed close to the seafloor in one profile).

We edited the sentence: "*This feature seems to be even more evident from the Jacaf Channel to the south (Figure 3), where it seems that probably organic matter was mostly retained and sedimented, favoring zones more prone to LDOW and hypoxia (Figure 2 and Figure 3)*".

L543: fjord heads: note that Fig 9 shows that the rivers do not necessarily flow into the fjord heads.

We edited the sentence: "Inside the fjords, where low salinity….."

L570: (greater THAN GPP)

We edited the sentence.

L572-573: "Nevertheless, Puyuhuapi, Reloncaví, and Compu fjords always showed high oxygen production values (GPP >20 mmol O2 m-3 d-1) at 2 m depth (Figure 8), confirming that Patagonian fjords act as a net sink of atmospheric CO2 due to the high surface primary production rates (Torres et al., 2011)": The first part of this sentence is not always true, since Fig 7 shows GPP <= CR at 2m for Compu (2020, 2022) and Puyuahuapi (2020). Furthermore, it is not appropriate to confirm a net sink of atmospheric CO2 only through GPP observations (eg, strong GPP could be fueled by upwelled nutrients; but those upwelled waters could also be high in inorganic carbon and lead to an overall source of CO2 for the atmosphere).

There was an error in graph 7 with the values of GPP for Puyuhuapi. We have now fixed it.

Also, we have now rewritten this paragraph, following the reviewer's suggestion about the atmospheric CO2 sink.

L595: I think this sentence needs to add BIOGEOCHEMICAL before 'processes' to be accurate, since Linford et al (under review) seems to have looked into the advection of LDOW from the ESSW.

We edited the sentence by *"….first time the quantification of the oceanographic and biogeochemical processes contributing to the hypoxia and deoxygenation of the Patagonian subsurface water.*

L601: ", owing to the entrance of allochthonous organic matter (natural and/or anthropogenic) to the fjords". I'm not convinced this was shown quantitatively. But I trust that after improving the POCalloc methods, results and discussion, this issue will be solved.

We clarified the information in the manuscript as was mentioned before.

L611: why are some residence times significantly higher in some fjords? Is it because of the presence of a sill? I think the authors should explain the "Why" of the higher residence times.

Thank you for your insightful question about the variance in residence times among fjords. You're correct to suggest that certain geographical features like sills may affect the residence times.

The dynamics of each fjord are complex and not completely understood yet. However, we've observed that fjords with less river discharge, in comparison to others, tend to be more dynamic with shorter residence times. Moreover, fjords with steeper or shallower sills dissipate more energy from tides and can be slower towards their interiors.

We acknowledge that this information would provide valuable context to our study and will ensure to include it in the revised version of the paper.

L613: "DO consumption during primary production by phytoplankton": This is wrong. Please fix.

We edited the sentence by "…*DO consumption during the community respiration by phytoplankton, and secondary production by bacteria".*

Review #2

Review of *'Oceanographic Processes Favoring Deoxygenation Inside Patagonian Fjords'* by Linford et al

The manuscript by Linford et al. uses physical and biogeochemical data from multiple sources to identify regions with high and low oxygen in the Patagonia coastal system. I was impressed by the scope of data used to address this issue. Deoxygenation in coastal waters is a very important issue and one that deserves attention from the scientific community. While the topic is important and the amount of data used was impressive, there were some gaps in this manuscript. I detail these below in the major and minor comments.

Major comments

My first major concern is the term **deoxygenation**. I think that the authors are basing this on Figure 6, which compares 2 physical and chemical profiles – one collected in March 1970 and one collected in February 2021. While it is likely that deoxygenation is happening in the Patagonia coastal system, using 2 profiles to quantify deoxygenation is insignificant. If quantifying deoxygenation is one of the central foci of the manuscript, I suggest the authors examine the entire time series after the seasonal cycle has been removed. See Aksnes et al., 2019 (https://doi.org/10.1016/j.ecss.2019.106392) and Jackson et al., 2021 (https://doi.org/10.1029/2020GL091094) for suggestions on how to best show deoxygenation in fjord systems.

We eliminated the focus on deoxygenation of this manuscript due to the lack of data to quantify this process. We concentrate the attention in the physical and biogeochemical processes contributing to hypoxia conditions and low dissolved oxygen (LDOW).

My second major concern is the figures. Most of them were illegible due to small font size. Many figures were of maps and were difficult to interpret for those not familiar with the region. I think most of the figures need to be overhauled to make them more legible.

We edited most of the manuscript figures to better understand the numbers and font sizes.

My third major concern was lack of quantification of the water masses. While the major water masses were named, how they were defined was not stated. Of particular concern is that the authors discuss mixing and modification of the water masses and how the modification could impact oxygen yet neither mixing nor modification was explicitly examined or quantified.

We added a new table (Table 4) and figure (Figure 3 new) to identify and quantify the water masses presented in the study area using a TS diagram with the addition of DO data. In section 2.1, we incorporated the methodology applied, and new results and discussion were also added to the text.

My fourth major concern is the lack of chlorophyll data presented in the manuscript. The authors report many times about phytoplankton production and respiration. While really useful, these data were only collected sporadically compared to the physical and chemical data. I think adding chlorophyll data to Figure 2 would help the reader see how oxygen concentrations compare to chlorophyll concentrations.

We added a new figure to show the chlorophyll-a patterns during two oceanographic campaigns.

Minor Comments

-Line 30 – should ‘decade’ be ‘decades’?

We changed “decade” by “decades”.

- Line 31 – I think you mean both surface and subsurface waters her?

We edited the sentence.

- Lines 37 to 39 – I found this confusing because Puyuhuapi Fjord was mentioned twice for low oxygen water. Please clarify

I agree with you, but in the case of Puyuhuapi Fjord, Hypoxia conditions (<30% oxygen saturation) and Low DO water (30%–60% oxygen saturation) was registered.

- Lines 50 to 52 – I find the sentence confusing and contradictory. Please clarify.

We edited the sentence.

- Line 74 – I don’t think this is true – not all OMZs are related to upwelling.

We deleted this sentence, even though most of the OMZ zone coincided with upwelling systems (C. Aguirre et al., 2019), excluding the Indian Ocean OMZ.

- Figure 1 – Due to the small font size, it is very difficult to read where d) and e) are located in the map. Also, in a) the authors don’t show where the study area is in relation to the South American continent.

We edited and presented a new Figure 1.

- Line 128 – ‘Profilers’ should be ‘profiles’

We changed “Profilers” by “Profiles”.

- Line 138 – How was the AML data processed? There are no standard CTD processing steps for AML instruments so showing the reader that these data were properly processed is important.

We added the following sentences, *"In the cases of SEB CTDs, data were processed according to the manufacturer's protocol and software (SBE Data Processing). To the AML CTD, the data raw passed a quality control, eliminating records out of range to finally average data every one meter."*

- Line 139 – What oxygen sensors were used? What is the error estimates for the oxygen measurements both when they were compared with Winkler titrations and when they weren't compared with Winkler?

We edited the sentence: "*Membrane, optical sensors (e.g., SBE 43 and Optode 4831) and the Winkler method have been used to obtain DO data (Strickland and Parson, 1968). Some experiments were conducted to validate CTD DO data with the Winkler method showing satisfactory results and high statistical correlation (e.g., R^2 range from 0.92-0.99)*",

- Table 1 – I suggest making this table into a figure that shows the density of data collected by year and by season. It is really difficult to visualize how much data were collected in a table.

The Figure 1b and Figure 1c showed a map of the space distribution of all the data collected and included in this manuscript. The dataset was divided into seasonal stations (e.g., fall-winter and spring-summer). On another side, the Table 1 presented the information divided by years.

- Line 156 – I think some justification is needed here if the incubation time was not consistent. In other words, how can the data be comparable if the incubation time was different for each sample?

The incubation period was the same for all samples, approximately 8 to 9 hours. Time-zero bottles should be immediately set to measure the amount of oxygen when incubation begins. Now we have added the incubation period in the text.

- Line 166 – It took me a few minutes to understand how these 5 BP experiments fit into the 9 in situ experiments mentioned on line 149 – I suggest referring the reader to Table 2.

We added the reference of Table 2 to the end of the sentence.

- Section 2.5 – Information is missing here – what is the time period of the moorings? What were the sensor depths? Were other sensors on the moorings in addition to the ADCPs? In addition, I don't see the mooring locations in Figure 9.

We added information to the section. *"The moorings covered the period of January to December 2016, and the ADCP sensors were moored between 40 m and 100 m*".

- Line 237 – what is the resolution of the SHOA nautical data?

The resolution of the nautical data is 500 meters.

- Section 3.1 – I'm not sure how the mean was calculated. Specifically, were all stations consistently sampled? Were there consistent measurements in the two seasons? A figure, as mentioned above, that shows the number of profiles as a time series would be helpful.

We added a time series of all CTDO profiles used in Figure 2. As the time series shows, the spring-summer seasons presented a major number of CTDO profiles in comparison to the fall-winters seasons, but at least 10 cruises were used to obtain the long-term seasonal mean. The mean was calculated using the Data Interpolation Variational Analysis (DIVA) gridding software developed by the University of Liege (http://modb.oce.ulg.ac.be/mediawiki/index.php/DIVA). DIVA analyze and interpolate data set in an optimal way as the optimal interpolation method, taking into the account the coastline and bathymetry of the study area. The calculation is executed on a finite element mesh adapted to the gridding domains (Troupin et al., 2010).

Troupin, C., F. Machín, M. Ouberdous, D. Sirjacobs, A. Barth, and J.-M. Beckers (2010), High-resolution climatology of the northeast Atlantic using Data-Interpolating Variational Analysis (Diva), J. Geophys. Res., 115, C08005, doi:10.1029/2009JC005512.

We added this information to the section 3.1.

- Line 277 – What proof do the authors have that ice was melting in San Rafael Lagoon?

We edited the sentence: "*This location also had the lowest salinity (15–21 gkg^{-1}), indicating the presence of estuarine water (EW) owing to the contribution of the ice melting from the San Rafael Lagoon and rivers discharges (Figure 2e-f)*".

- Lines 277 to 279 – What proof do the authors have of mixing? Perhaps showing a TS diagram would help to show the authors that water mass modification is occurring.

We added a TS diagram to present and quantify the water masses identified in this study.

- Lines 281 to 282 – A reference is needed to support this statement

We added the reference in the Discussion section, *"This water mass enters Patagonia by the Guafo mouth and moves south throughout the Moraleda Channel to finish inside the Puyuhuapi Fjord and Jacaf Channel (Linford et al., 2023)".*

- Figure 2 – A note is needed as to why so much less oxygen data were available. Again, adding a time series figure with the number of oxygen profiles would help. Also, I don't know what direction the x-axes is facing (i.e. north to south or south to north). The illegible font size makes this especially challenging. Where are the station locations in relation to the maps in Figure 1? Also, it would be helpful if a hypoxia contour line was added to oxygen.

We edited the Figure 2 to better the understanding. A subplots showing time series of profiles was added to the new figure. In case of oxygen data, less records were analysis in comparison to the CTD, because most of the CIMAR cruise obtained the DO by Winkler method at standard horizontal levels (0, 10, 25, 50, 75, 100, 150, 200, 250, 300, 350, and 400 meters) and not with CTDO at 1m.

- Figure 3e – I was struck by the low silicate near the surface. Is silicate limiting for phytoplankton here? Again, it would be useful to compare this figure with phytoplankton concentrations.

The low silicate near the surface layer means the use of this inorganic nutrients by the phytoplankton community as we mentioned in the discussion section, "*In the case of northern Patagonian fjords, EW is rich in silicic acid because of the riverine supply (Silva and Vargas, 2014), with characteristic pulses at the surface layer that changes throughout the year due to the organisms' consumption (Montero et al., 2011), as observed in northern Patagonia, including the Chiloé Inner Sea (Figure 4)".*

The Chl-a data presented in the new Figure 7 shows high concentrations of Chl-a in the similar area where the silicate was lower.

- Line 308 – I don't think that Magdalena Sound is labeled on the map in Figure 1.

We added the position of Magdalena Sound to Figure 1.

- Figure 4 – The map insets are illegible.

We eliminated the map from the figures and added to the Figure 1.

- Figure 6 – The subsurface oxygen minimum shown in e) has been well described in the literature. Please see Jackson et al. (2021, https://doi.org/10.1007/s12237-021-00999-y) and Rosen et al. (2022, https://doi.org/10.3389/fmars.2022.1000041) for examples of this feature in Canadian waters.

- Figure 7 – I can't read this figure at all. So I am unable to assess it.

We divided the figure to better represent the results.

- Section 3.3 – I find this section very descriptive and qualitative. I understand that the satellite data were used to estimate where terrigenous material could be entering the ocean. But why weren't the SPM and POC data also used to prove that the areas identified as high SPM by satellite were verified with the SPM/POC data?

The images described in section 3.3 correspond to periods with cloud-free conditions, necessary to obtain the SPM estimates with satellite data. There is not always a match between in situ data and available Sentinel-2 data, which has a revisit time of ~5 days. The satellite data provide a different view of the situation, i.e., the spatial extent of these episodes.

- Lines 393 to 394 – I don’t think that the direction of river outflow can be observed from Sentinel satellite data?

If the water rich in sediments coming from the Rio de los Huemules is seen to penetrate into the fjord, we can assume the current flow is directed towards the fjord, and viceversa. Sediments (and chlorophyll, temperature and other variables) can serve as tracers of water dynamics.

- Figure 9 – I have no idea where these regions are in relation to the Patagonia coastal system

We added a subplot to show inside the study area the region selected to present the SPM results.

- Figure 10 – This is confusing – why are SPM and POC inversely correlated? I would expect that regions of high SOM would also have high POC.

We edited the Results and Discussion sections to clarify the description of Biogeochemical variables.

- Figure 11 – In c), why were the meridional and not the zonal currents shown? Also, I’m struggling to understand how these data contribute to the overall storyline.

We added the marine currents data to demonstrate the decrease in the intensity of currents from the outside zone (Corcovado Gulf) to the inside area (Puyuhuapi Fjord). This regime contributes to the residence time and is one of the reasons favoring hypoxia conditions in the Puyuhuapi Fjord. The zonal and meridional currents presented in the figure were selected according to the current roses' behavior and were added to the new figure.

- Line 466 – In most coastal systems, the residence time of the water varies with depth (e.g. Pawlowicz et al., 2007, https://doi.org/10.3137/ao.450401). What depth is the residence time calculated for?

We appreciate your query about the depth at which the residence time is calculated. We apologize for the lack of information in the model methodology section. In this study, the residence time was calculated using the average from 50m to the bottom, typically the zone where the lowest oxygen values are observed.

Your feedback is valuable, and we will add a paragraph to better describe this aspect in our revised manuscript.

- Lines 490 to 491 – Based on my comments above, I don’t think that the authors can prove that the high SPM in satellite data is allochtonous organic matter

We deleted the sentence.

- Line 505 – Mixing was never investigated in this manuscript. Rather, the authors calculated buoyancy frequency, which shows the absence of stratification.

We edited the sentence: *"An absence of stratification was observed throughout the year in the Chiloé Inner Sea (Figure 5)".*

- Lines 522 to 523 – No chlorophyll data were shown in Figure 10..adicionar la referencia a Figura 2 nueva con Chla

We added the reference of the new Chla figure to this senstence.

- Lines 536 to 537 – What evidence to the authors have to support that ESSW and SAAW are strongly connected to the estuarine water masses?

We edited the sentence: *"The SAAW is strongly connected with the estuarine water masses (MSAAW and EW), and the ESSW interacts with the SAAW, as observed in this study (Figure 2 and Figure 3)".*

- Line 573 – I don't think that high O2 always means that a region is a sink of atmospheric CO2. This relationship can be complicated in some coastal region (e.g. Johannessen et al., 2014 **https://doi.org/10.4319/lo.2014.59.1.0211**

We have now rewritten this paragraph, following the reviewer's suggestion about the atmospheric CO2 sink. (Line 587)