Reviews from Meyer et al. submission to the Ocean Science Special Edition on underwater gliders: “Critical uncoupling between biogeochemical stocks and rates in Ross Sea springtime production-export dynamics”

Date: 21 Feb. 2025
Review response and resubmission: 31 Mar. 2025

We thank the reviewer for their helpful comments and suggestions. Our responses are added below in bold.

Reviewer 1

This study collates 3 glider campaigns in the Ross Sea to provide rolling 3-day estimates of oxygen-derived NCP and optical-backscatter POC export rates. The study supports prior classification of the Ross Sea as high productivity, low export; and highlights high-short term (ie. sub-weekly) variability of NCP. I present below a list of comments that I hope will help strengthen the message and clarify points of discussion for the reader.

Several assumptions are necessary – particularly in regions such as the Ross Sea with limited in situ sampling to support glider observations - to make such estimates, which always leads to questions around the accuracy of such estimates, the authors do the best possible in these conditions to provide quantitative metrics of NCP and export. Although the accuracy of the numbers can be brought into question, it is the patterns and interplay between NCP, POC and export which are revealing. I find the main strengths/key results of the paper to be [1] the use of a fixed criteria (within 90% of peak chlorophyll) to define a coherent bloom period which could be investigated interannually, [2] that the system is dominated by changes in dO2/dt (although I have questions about that, see next paragraph), and that [3] high biomass does not mean high NCP or export (although lines 265-267 seem to contradict this).

My main issue with the manuscript is that the authors use the term “temporal variability” as a catch-all, which can lead to confusion within the text. It is also used as a justification to ignore spatial variability. I acknowledge that the advection term is low, but this does not negate the existence of spatial variability: crossing fronts and filaments may lead to rapid changes in dO2/dt and NCP (ie. meltwater filaments are cold and fresh, leading to generally greater O2 concentrations). These are prevalent in regions of ice melt/formation like the Ross Sea. Some physical diagnostic figures would help alleviate these concerns. A timeseries of surface mixed layer [1] density and [2] oxygen solubility would reveal [1] sharp fronts which may help explain peaks in productivity (mid December??) and [2] biases in NCP due to changing water masses such as meltwater filaments. Likewise, if in the scatter plots, no obvious pattern appeared between NCP and d(solubility)/dt then it would lend further credibility to the dominance of biological processes in regulating dO2/dt.

We believe the reviewer is right and have added language to help clarify the scales on which we are talking when we say “temporal variability”. Additionally,

as the reviewer suggests, we have added 1) a time series of oxygen solubility to the supplemental, 2) a scatterplot of NCP vs oxygen solubility to the supplemental, and 3) time series of oxygen saturation to Figure 3. These new and amended plots show a lack of strong relationship between patterns of oxygen solubility and the biogeochemical stocks and rates, thus supporting the idea that biologic changes are the largest determinant of changes in oxygen concentration.

See lines 225-227 and the attached figures:

"In all three deployments, NCP was driven by $\frac{\partial O_2}{\partial t}$, reinforcing the notion that biology is the largest driver of changes in upper ocean oxygen content during the bloom period, evidenced by this term being an order of magnitude greater than any of the other terms in the oxygen budget (Fig. 5A; Fig. 6; Fig. S6; Fig. S7)."

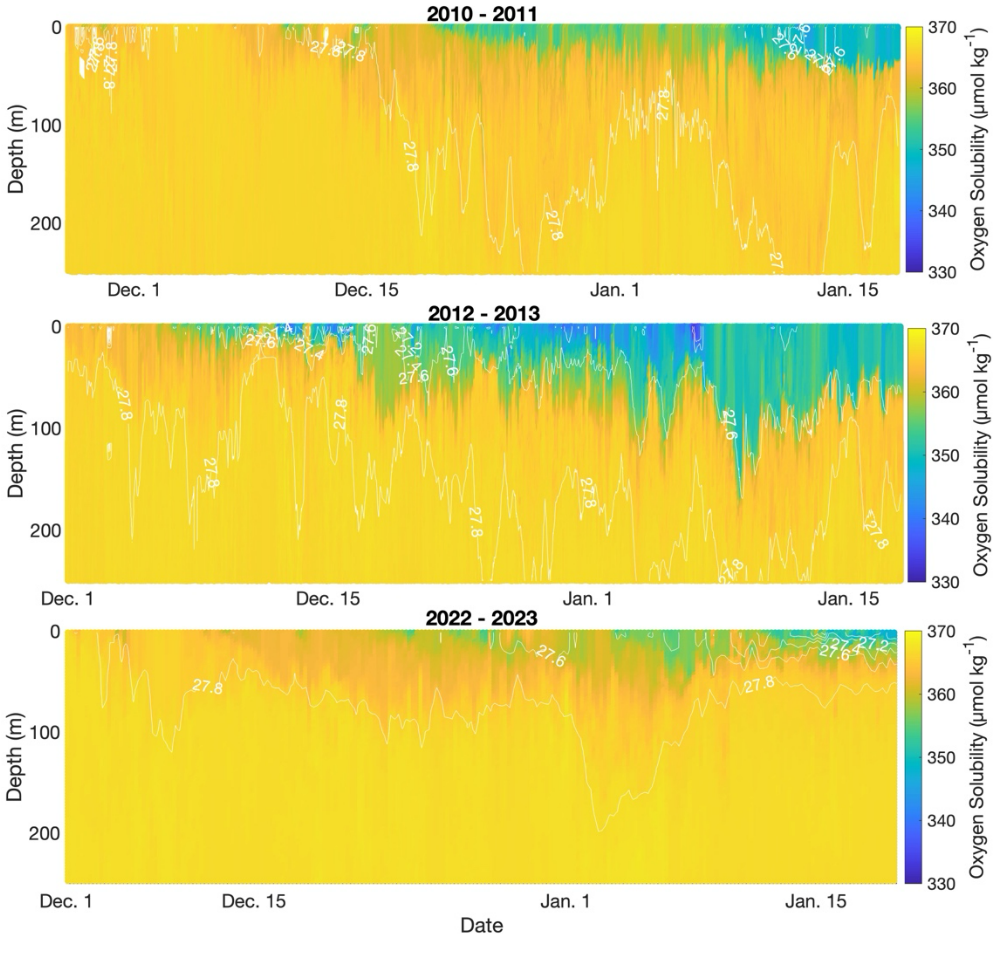

Figure S6. Oxygen solubility (μmol kg^{-1}) from the 2010 – 2011, 2012 – 2013, and 2022 – 2023 glider deployments. Solubility was calculated according to the protocol outlined in the UEA Seaglider Toolbox (https://bitbucket.org/bastienqueste/uea-seaglider-toolbox/src/toolbox/). White lines represent isopycnals.

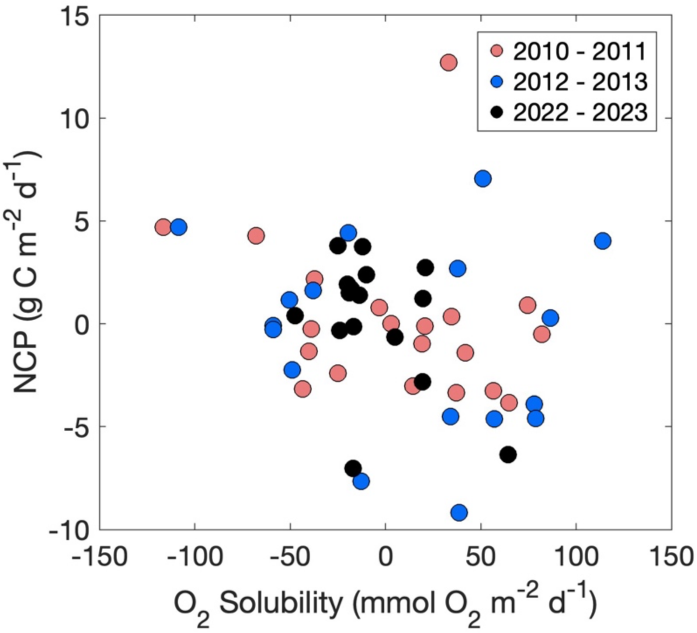

Figure S7. Net community production (g C m^{-2} d^{-1}) versus oxygen solubility (mmol O_2 m^{-2} d^{-1}) per 3-day periods for 2010 – 2011, 2012 – 2013, and 2022 – 2023 glider deployments.

When using the term temporal variability, please clarify whether you mean at the 3-day level, the bloom level, or the interannual level.

Clarifying language added throughout. For example, see lines 228-230:

"Despite the substantial temporal variability between the 3-day periods and negative average NCP observed in 2012-2013, our findings suggest that the Ross Sea is capable of high rates of production during the spring bloom period."

L58: Publications not accessible to the reader should not be cited. Hopefully it's out by publication time.

We apologize for the inconvenience this caused the reviewer. The paper by Portela et al. (2025) is now published and available at: doi.org/10.1029/2024GL111264.

L65-68: This statement might be strengthened by more quantitative metrics. How significant is the difference in export efficiency?

We added the range of flux transfer efficiencies reported by Buesseler et al. (2020). See lines 67-69:

"Despite this, some studies have found reduced carbon transfer efficiencies relative to other regions on seasonal and annual timescales (e.g., Southern Ocean flux transfer efficiencies ranging from 0.2 to 0.8; Buesseler et al., 2020)."

L70 and 82: “spanning 2010-2023” is somewhat misleading, maybe better to list specific years for the 3 deployments?

Clarified as suggested. See lines 72 and 86.

Methods - calibration: were the same fluorescence/chlorophyll ratios used for the whole deployments? Can you elaborate on expected consistency across *Phaeocystis* and diatom communities?

The fluorescence/chlorophyll ratios were internally consistent but varied between deployments. Language has been added to clarify this point. See lines 112 – 114:

“For both chlorophyll and POC samples, single equations converting fluorescence to chlorophyll and backscatter to POC were individually generated per year (Fig. S1).”

Due to potential ecological and environmental differences between years, we feel it is more accurate to employ one consistent fluorescence/chlorophyll relationship per deployment rather than attempting to generate one ratio that describes all three deployments. While we might expect some variability in these ratios between communities dominated by Phaeocystis vs. diatoms, all three glider deployments are constrained by a very limited number of calibration casts. Therefore, we think it is best to not differentiate on how these ratios may vary depending on the dominant communities and the bloom succession.

L114: How accurate is the Morel fit with non-mid-latitude communities? Could this be evaluated using ship-based PAR profiles to provide an error estimate?

We appreciate the reviewer’s concerns about the Morel fit in this region. Indeed, this equation was derived without including polar data. Unfortunately, this is the case with many (if not most) satellite derived products and we are unaware of a similar method of estimating euphotic zone depth from fluorescence (or other available data) that incorporates or is specific to polar data. Ship-based PAR could provide an estimate during the times the glider was co-located with the ship, but this only occurred during the recovery calibration casts. Therefore, we view it is more accurate to apply the Morel fit to the glider-based fluorescence rather than apply a singular PAR value from the ship to the entire glider deployment.

L120: What Kz value is used and why?

Clarified as suggested. See line 131-132:

"F_{K_z} is the vertical eddy diffusion flux of oxygen to the water below 100 m, using the previously published vertical diffusivity coefficient (K_z) of 10^{-3} m^2 s^{-1} for the Ross Sea (Kaufman et al., 2017). “

L122: It would be worth stating that using DAC as an estimate of upper 100m currents when gliders dive deep likely leads to a significant underestimation of upper ocean velocities. Note that I do not think this impacts the study significantly as F_adv is an order of magnitude smaller than NCP.

A sentence describing this consideration has been added to the Uncertainties section. See lines 197-201:

"Values of F_{ADV} may also be influenced by the use of DACs averaged over the entire dive rather than the upper 100 m, but given the low F_{ADV} values relative to NCP, this does not make a substantial difference in the calculation. For example, if supposed F_{ADV} is underestimated by 50%, this only leads to an underestimate of 0.3, 0.7, and 4.0 mg C m^{-2} d^{-1} in our calculations for 2010 – 2011, 2012 – 2013, and 2022 – 2023, respectively."

L134-5: It may be worth specifying that these are assumed minimal *at the 100m boundary*. **Clarified as suggested. See line 147:**

"During the bloom period, we assume entrainment flux, lateral mixing, and vertical advection at the 100 m boundary to all be minimal and are thus omitted."

Fig 3. I really struggle to tell the colours apart. **Altered as suggested. See figure below.**

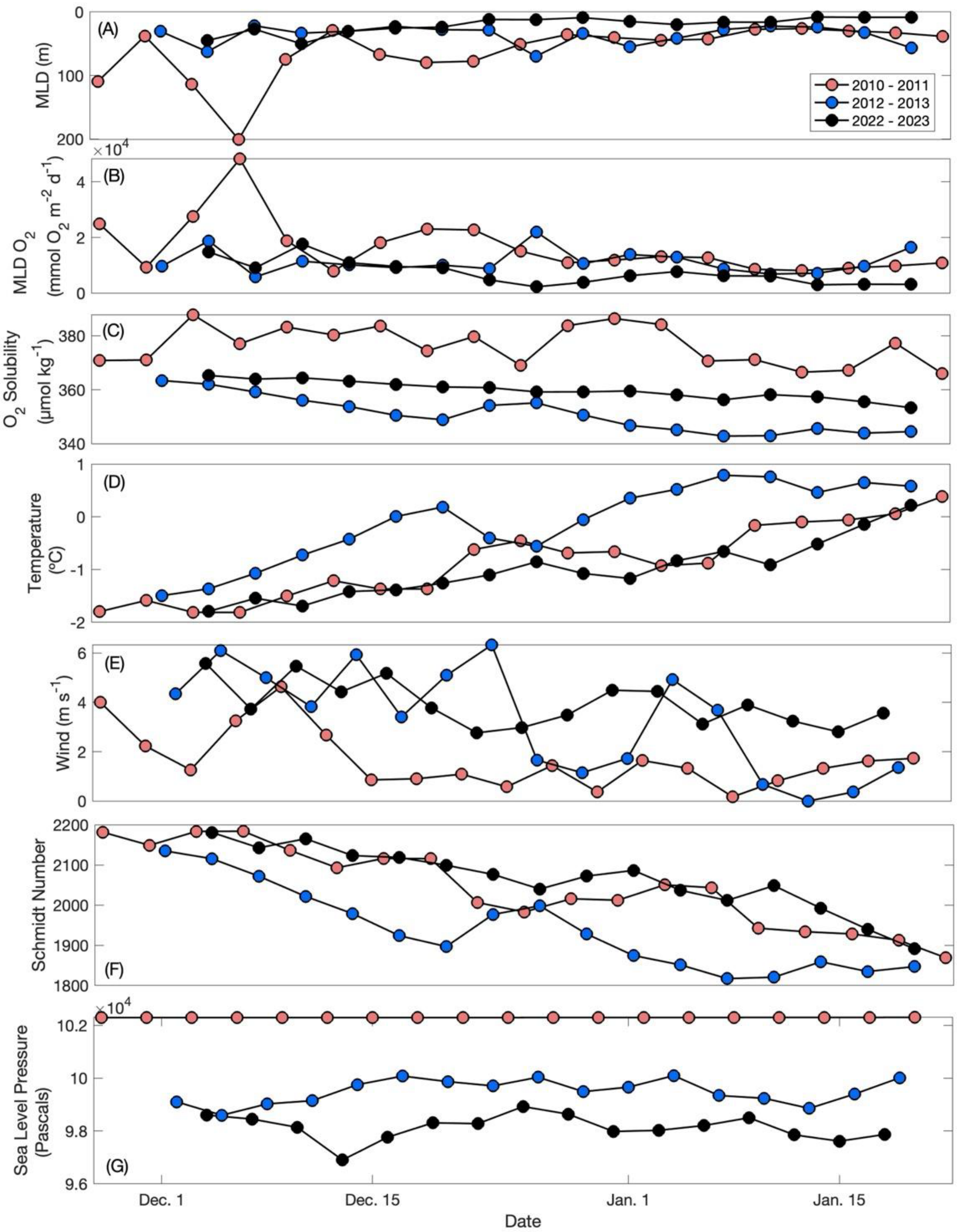

Figure 4. Net community production (A), $\frac{\partial POC}{\partial t}$ (B), and $Export^{*}_{POC}$ (C) through time for glider deployments occurring in the 2010-2011, 2012-2013, and 2022-2023 productive seasons. Units for all rates are g C m^{-2} d^{-1}. Shaded regions represent uncertainty for each rate. For NCP, positive values indicate autotrophy, and negative values indicate heterotrophy. For $\frac{\partial POC}{\partial t}$, positive indicates an increase in POC through time whereas negative indicates a decrease.

L151-154: The convention used here is confusing and it is not immediately clear to me what export potential represents. This warrants further clarification. As I read it, if we assume a scenario with no NCP and a decrease in surface POC (eg. due to an export event) then we have 'export* = 0 – (+ve dPOC/dt)' which leads to a negative export potential. Please explain in more detail.

Correct. If there is no production and a negative dPOC/dt (which means POC concentrations are decreasing through time through a combination of biological (respiration, grazing) and physical (advection, sinking) processes), there would (theoretically) be no POC left in the surface to be exported. Timescales are very important in this discussion. We use the term "POC export potential" because we are unable to capture the full bloom succession. Therefore, we cannot definitively say that our rates are net export because that ignores biological and physical processes occurring later in the bloom when heterotrophy likely begins to outpace autotrophy (Blight et al., 1995; Thomalla et al., 2015). We have added additional text to clarify this point. See lines 170-173:

"POC export potential is the amount of POC available for export in the upper 200 m at the end of the study period. Because each deployment ended before the final bloom succession and biomass export, we measure a potential, rather than actualized export at the end of the deployment period (Hansell and Carlson, 1998; Sweeney et al., 2000). Here, POC export potential was estimated as the difference between the two rates"

L165: One would assume that inter-sensor differences are minimal after calibration to Chl and POC samples anyways? **We agree and have since removed this discussion.**

L179-180: Can it be described as "substantially higher" with such a standard deviation? Would it be beneficial to show the standard error of the mean as well to highlight that your means are indeed significantly different? **We agree and remove the word substantially. For consistency with our other values and so as not to make the results too data intensive, we prefer to continue to present standard deviations, rather than standard error.**

L186: Could a change in dO_2/dt not also be due to crossing a front? Entering a meltwater filament (both colder and fresher) for example could lead to a significant increase in O_2 simply due to greater solubility. **To evaluate the suggestion of solubility, oxygen solubility plots have been added to the supplemental and do not suggest large changes in dO_2/dt due to changes in water masses. These findings support the notion that the majority of dO_2/dt is likely biological, supported by the high chlorophyll concentrations and the small physical terms (F_{adv}, F_{kz}, ASE) in relation to NCP. See new oxygen figures added above (Fig. S6, S7).**

L225: I agree that averaged values over the study period are likely dominated by temporal evolution of the bloom – however I disagree that temporal variability is. I do not think we can rule out that much of the variability observed between successive 3-day segments could be due to spatial variability. I suggest reformulating the statement to highlight that over the study period, spatial variability will be averaged over, revealing the long-term temporal evolution of the bloom. **Altered as suggested. See lines 267-269:**

"When evaluated seasonally, spatial variability will be averaged over and temporal variability is likely to be dominant due to the evolution of the bloom (Fig. S5). Substantial variability of POC and O_2 concentrations themselves is not diagnostic of substantial variability in rate patterns."

L254-255: I agree 100% with this paragraph up to the last statement – maybe as I'm not sure of what temporal variability you refer to. Do you mean the variability exhibited between subsequent 3-day estimates? If so, I'm not sure I agree. Highly variable data (evidenced by high *standard deviation*) does not imply that that variability impacts the *mean*. Rather, undersampling of a highly variable system could lead to an error when estimating the mean (hinted at by, for example, a large *standard error of the mean*). Without further investigating the influence of spatial variability on dO2/dt, I am not convinced that the high variability observed is purely due to rapid changes in NCP. My perception of the real strength of the paper here is the large number of NCP and dPOC/dt estimates which help us identify robust and trustworthy mean values of NCP and dPOC/dt by averaging over any spatial and short term temporal variability, across a well-defined bloom period (within 90% of peak chl). The high variability is the issue that makes prior approaches (eg. incubations) questionable. Could you clarify what you mean by that last sentence in the paragraph?

The reviewer makes a very good point that has changed some of our thinking. We agree that this enhanced resolution should only be really important when evaluating sub-seasonal trends and/or standard deviations. We have altered portions of the discussion to reflect this. See lines 295-298:

"Thus, our results support the classification of the Ross Sea as a high production, low export system (Henson et al., 2019), but the high-resolution data provided by the gliders show substantial 3-day temporal variability during the bloom season that is likely a consistent, yet overlooked, feature of this system. Our results show that this temporal variability impacts rates and our understanding of the relationship between rates when investigated on the sub-seasonal level."

L261: What is the proposed mechanism for the link between low dPOC/dt and variable mixed layer depth? **The proposed mechanism is a changing mixed layer that prevents substantial POC accumulation, leading to low dPOC/dt. Clarifying text has been added; see lines 303-305:**

"Thus, a higher NCP does not necessarily translate to higher $\frac{\partial POC}{\partial t}$. For example, the low rates of $\frac{\partial POC}{\partial t}$ in the 2010-2011 season likely stem from the low POC concentrations (Fig. 3) and a variable mixed layer depth, which could prevent substantial POC accumulation due to mixing induced light limitation, (Fig. 2A) during this period."

L270: Is the big increase in dPOC/dt linked to the inclusion of the earlier or the later data? **This increase in dPOC/dt is due to the inclusion of later data. The sentence was changed to reflect that. See lines 317-318:**

"When late January and early February rates are included, and values averaged over the entire deployment, NCP is low at 0.05 ± 2.75 whereas $\frac{\partial POC}{\partial t}$ is much higher at 0.23 ± 1.24 g C m^{-2} d^{-1} (Meyer et al., 2022a)."

L271: I do not understand how sub-sampling the same population would lead to a different mean considering all terms involved have a linear impact on NCP. Can you explain how you expect 5-day intervals to lead to a different mean to 3-day intervals? **The reviewer's helpful comments have changed our thinking. We clarify that the length of interval will only change sub-seasonal values, not seasonal. See lines 295-297:**

"Additionally, these rates were calculated in 5-day intervals, not 3, so some sub-seasonal variability is likely being smoothed and overlooked due to the longer integration period."

L270-276: I do not follow the argument made here. I do not understand how subsampling would lead to a different mean; my understanding is the difference in values is due to inclusion of the earlier and later portions of the season. So it is how one defines the bloom and the study period (*ie*. 90% of max chl) which is important. From what I understand of the study, this simply shows that we have different phases across a season, but that the core of the bloom (ie. 90% of max chl) is slightly autotrophic. **We agree and have amended the text to reflect this and eliminate confusion. See lines 299-302:**

"Thus, establishing our evaluation timeframe based on an ecological, rather than temporal, metric (such as chlorophyll concentration) is important and should lead to more biogeochemically-reflective rate estimates. These rates are important to compare over the time period when chlorophyll concentrations are greater than 10% of peak bloom concentrations regardless of the exact days of year."

L270-276: I further do not understand how 3-day vs 5-day intervals would change any estimates related to climate change (L273). I feel that throughout the discussion, it would help the reader to be very clear about what scale of temporal variability you refer to (3-day, within the bloom, across a season, interannually). **See above discussion regarding the integration periods of NCP. Text has been added to clarify what we mean by "temporal variability".**

L277-281: Would it be beneficial to fit a martin curve to POC profiles and report the e-folding term as a measure of retention/export efficiency? **While we agree this would be a useful metric, traditional e-ratios are calculated in reference to NPP, not NCP. In an early version of a previous study (Meyer et al., 2022), we calculated an e-ratio and a T100, and reviewers felt strongly that supplementing NCP for NPP made our calculations of these export metrics confusing and not directly comparable to what is reported in the literature. We have to agree and therefore, are choosing to omit them in this analysis.**

L283-286: Fluorescence to chlorophyll ratios and optical backscatter to POC would be expected to change significantly as the community changes. I understand that in the absence of in situ samples it is not possible to assess and calibrate on a per-community basis, but I think it would be important to highlight the assumption made in the calibration and what the effect could be on results. **We agree, and clarifying text has been added. See lines 339-344:**

"The limited availability of POC and Chl calibration samples will not resolve any specific POC and Chl concentration differences between phytoplankton groups which may lead to slight under- or overestimations of POC, Chl, and C:Chl ratios as the bloom evolves. Despite this, the dramatic change in C:Chl ratios through time is typical of the annual bloom and suggests a mixed community with a shift from Phaeocystis to diatom dominance over time (Jones and Smith, 2017; Smith and Kaufman, 2018)."

L287: I still do not understand what is meant by the influence of temporal variability on seasonal means; as this is a recurring point and an element presented as a key takeaway, I would really encourage you to clarify this further. **Text has been added to clarify what we mean by "temporal variability".**

L295-296: This statement would be strengthened by the inclusion of physical diagnostics. **Altered as suggested. See lines 352-354:**

"During this period, NCP is 13.0 g C m^{-2} d^{-1} resulting from an exceptionally high $\frac{\partial O_2}{\partial t}$ (1420 mmol O_2 m^{-2} d^{-1}; Fig. 5A) but moderate F_{ADV}, F_{Kz}, and ASE_{ML} (0.15, 46, -1.9 x 10^{-3} mmol O_2 m^{-2} d^{-1}, respectively)."

L306-315: I would encourage you to wait until Portela et al. is published before final publication of this manuscript. **The paper by Portela et al. (2025) is now published and available at doi.org/10.1029/2024GL111264.**

L321: How does your study relate to sea-ice?? This seems rather a result of Portela et al. which is not available to the reader. **Clarifying text has been added to the Discussion; see lines 374-376:**

"Portela et al. (2025) note substantial differences in Ross Sea sea ice concentration, as is evident in Fig. 1, between these two years with 2022-2023 having more ice and a later opening of the polynya than 2010-2011 and cite this as a possible mechanism leading to the high chlorophyll concentrations observed in 2022-2023."

Additionally, as mentioned above, Portela et al. is now published which should help clarify the referenced points.

L331: Further up (L265-267) you essentially make the statement that export efficiency and POC removal occurs more when there is high biomass – but L331 you state they are uncoupled. I assume this is a question of time scale again (seasonal vs weekly). This should be clarified further. **Clarifying text added. See lines 393-396:**

"Our data highlight temporal uncoupling between biogeochemical stocks (POC, O_2) and rates (NCP, $\frac{\partial POC}{\partial t}$, $Export^*_{POC}$) and between related rates (NCP and $\frac{\partial POC}{\partial t}$) when evaluated on short-term (3-day) and seasonal scales during three spring bloom periods. Much of this uncoupling relates to substantial variability which drives rates and reinforces the need for high-resolution measurements."

Reviewer 2

This manuscript uses several seasonal glider data sets over a few years to derive carbon and export budgets for the Ross Sea in Antarctica. The gliders collected high resolution data sets that are impressive. The main science finding is that net community production (NCP) appears to be decoupled from phytoplankton biomass and export production. The authors also argue for the need for high resolution data to resolve the dynamics in biogeochemistry. The paper is interesting, and the study is conducted in a very important location in terms of Earth's biogeochemistry. There are few things that I think the paper would benefit from.

The main findings of the study are based series of derived parameters. For the derived parameters it would be nice to have some discussion of the sensitivity of the derived parameters. It would be nice have some idea how sensitive are the findings to many of the implicit assumptions in the deriving these products? **A discussion of the implications of our assumptions was added to Section 2.3 Uncertainties (Lines 197-201):**

"Values of F_{ADV} may also be influenced by the use of DACs averaged over the entire dive rather than the upper 100 m, but given the low F_{ADV} values relative to NCP, this does not make a substantial difference in the calculation. For example, if supposed F_{ADV} is underestimated by 50%, this only leads to an underestimate of 0.3, 0.7, and 4.0 mg C m^{-2} d^{-1} in our calculations for 2010 – 2011, 2012 – 2013, and 2022 – 2023, respectively."

The findings of NCP being decoupled from export flux and that the Ross Sea is a high productivity and low export zone is interesting. While referenced in the introduction that this has been seen before but the reasons for this are never not explicitly explored. I think that the discussion of what processes underlie this phenomena, even if it was hypothesized, would be of great interest. **We agree and have added a section hypothesizing some potential reasons for the decoupling between production and export. See lines 305-309:**

"Therefore, $\frac{\partial POC}{\partial t}$ must be kept relatively low by some external factor that was not measured during this study, such as differing lability of P. antarctica versus diatoms (or lack thereof; Misic et al., 2017; Misic et al., 2024) and the role of particle-attached bacteria and remineralization (Becquevort and Smith, 2001) on backscatter measurements. Coupling glider studies with such analyses should be conducted in future studies as they may help elucidate the mechanism behind consistent $\frac{\partial POC}{\partial t}$ between seasons."

Currently the discussion reads a bit like a data report and think more synthesis would be of wide interest. The data is solid, but I think some synthesis would increase interest in this work. **We agree and have worked to provide more synthesis throughout the Discussion.**

References

Blight, S.P., Bentley, T.L., Lefevre, D., Robinson, C., Rodrigues, R., Rowlands, J., and P.J.B. Williams. 1995. Phasing of autotrophic and heterotrophic plankton metabolism in a temperate coastal ecosystem. *Marine Ecology Progress Series*, 128(61-75). doi:10.3354/meps128061.

Cassar, N., Barnett, B.A., Bender, M.L., Kaiser, J., Hamme, R.C., and B. Tilbrook. 2009. Continuous high-frequency dissolved O2/Ar measurements by equilibrator inlet mass spectrometry. *Analytical Chemistry*, 81(5), 1855-1864. doi:10.1021/ac802300u.

Thomalla, S.J., Racault, M.-F., Swaart, S., and P.M.S. Monteiro. 2015. High-resolution view of the spring bloom initiation and net community production in the Subantarctic Southern Ocean using glider data. *ICES Journal of Marine Science*, 72(6), 1999-2020. doi.org/10.1093/icesjms/fsv105.