Flynn et al. *Summertime productivity and carbon export potential in the Weddell Sea, with a focus on the waters adjacent to Larsen C Ice Shelf*

Response to Reviewer's comments

We thank the reviewers for their comments, which have definitely improved our manuscript. Both reviewers acknowledged the importance of the observations presented here, particularly those from the under-sampled western Weddell Sea adjacent to Larsen C Ice Shelf, and both are supportive of this work being published once we have addressed their concerns and suggestions. Below, we respond to each of their comments in turn and outline the changes we have made to the manuscript in response. The reviewer comments are in black text and our responses are in blue.

Reviewer 2 also added a number of comments directly to the pdf of the manuscript; we have responded to each of these on the pdf. Within these comments, the reviewer made numerous suggestions related to word choice, style, etc. We have made the suggested changes where they improve the clarity or precision of our argument, but in the case where they are purely stylistic, we have generally chosen not to make the edits.

Reviewer 1: Sebastian Moreau

Review of the paper entitled "*Summertime productivity and carbon export potential in the Weddell Sea, with a focus on the waters adjacent to Larsen C Ice Shelf*" by Flynn et al.

General comments:

I commend the authors on a very interesting paper describing a very large dataset on primary production, carbon export and nutrient dynamics obtained throughout the Weddell Gyre. Of particular interest, the authors estimate the relative contributions of new and regenerated productions by studying the uptake of nitrate and other nitrogen forms. In addition, the authors present very interesting and rare nitrite oxidation data, used to estimate nitrification. They put their results in context of concurrent measurements of water masses, macronutrients concentrations and ratios and phytoplankton community composition.

In addition, their results also provide further insights into the actual debate on the role of cryosphere-ocean interactions in the Southern Ocean. A question will be a very important topic under ongoing climate change in the coming decades.

As pointed by the authors, the Weddell Gyre is a significant region of the World's Oceans, where strong open waters primary production leads to a large CO2 sink. Therefore, this paper is a significant contribution to the study of the biogeochemical cycles of nutrients and carbon in the Weddell Gyre and, more generally, in the Southern Ocean. In addition, the paper is very-well written and a pleasure to read. Congratulations to the authors.

I give below numerous minor comments to improve the manuscript. In addition, a few points deserve more effort from the authors.

For instance, I believe that some equations provided in the Materials and Methods (i.e. for nutrients drawdown and sea ice fraction) are not well written. It seems that the authors' calculations are correct since the figures that present nutrients drawdown are reasonable and in line with other results presented throughout the paper. But I suppose that the authors' calculations were not translated properly into equations. Please see my detailed comments below.
Please see our response below, following the Reviewer's detailed comment. In short, there were some errors in the way that the equations were written in the text, although not in the way they were applied to the data. We thank the reviewer for spotting these mistakes, which have all now been corrected.

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In addition, I think that the authors need to mention throughout the text that the biological carbon pump will be efficient at the Antarctic coast if associated with the formation of bottom waters as it will then be exported to the Ocean's abysses. Biological carbon export onto a continental shelf along Antarctica is otherwise more likely to become part of the benthic food web.

Please see our response below, following the Reviewer's detailed comment. In brief, we have included discussion regarding carbon exported onto the continental shelf as a food source to the benthos in the introduction and discussion.

In addition, the resolution of all figures needs to be improved as the figures are sometimes difficult to read and are very pixelized.

Response: High resolution figures will be submitted along with the final version of the manuscript.

Therefore, I suggest publication in *Biogeosciences* if the authors can improve these few points.I hope these comments will help the authors to strengthen this very interesting manuscript, as it will bring a significant contribution to the understanding of the biogeochemistry of polar oceans. I wish you good luck with the review process.

Specific comments:

Abstract

Line 31: "*with the highest potential export fluxes*". The authors only measured potential exportfluxes, not direct export fluxes.

Response: Agreed; we have updated the sentence with the suggested addition (line 32).

Introduction

Line 70: for a better understanding, please replace "*Weddell Sea surface waters*" by "*Surface waters of the open Weddell Sea*"

Response: Agreed; we have updated the sentence with the suggested addition (line 65).

Line 78: a recent publication by Dinniman et al. (2020) could be referred to in this sentence, as they show that the under ice-shelves meltwater-laden pump drives iron with sedimentary origin upward towards the surface of Antarctic polynyas.
Dinniman, M. S., St-Laurent, P., Arrigo, K. R., Hofmann, E. E., & van Dijken, G. L. (2020). Analysis of iron sources in Antarctic continental shelf waters. *Journal of Geophysical Research:Oceans*, 125, e2019JC015736. https://doi.org/10.1029/2019JC015736

Response: Agreed; the text has been updated to include the reference suggested by the reviewer (line 73).

Line 80: "*supporting high rates of carbon export*". I think that this quote needs to be put in context. My understanding is the biological carbon pump will be efficient at the Antarctic coast as long as it is associated with the formation of AABW as it will then be exported to the Ocean's abysses. Biological carbon export onto a continental shelf along Antarctica is otherwise more likely to become part of the benthic food web.

Response: We agree with the Reviewer's point as it pertains to carbon storage. Carbon export will happen regardless of whether the sinking material is ultimately recycled at the seafloor or incorporated into AABW. Nonetheless, we have clarified that for the large phytoplankton blooms of the CCSZ to equate to a strong biological pump requires that the sinking flux (or more correctly, the CO_2 resulting from the decomposition thereof) be incorporated into bottom waters (e.g., line 75-76).

Line 81: "*due to deeper mixed layer depths (MLD) that lead to light limitation of phytoplankton*" this statement is not always supported. For example, a recent paper by Kauko et al. (2021) suggest that the open waters off the eastern Weddell Sea are not light limited as the depth of the euphotic zone is always deeper than the mixed layer depth. Therefore, I suggest that the authors nuance their statement.

Response: Both Reviewers note that a shallow mixed layer is perhaps not as important as previously thought for phytoplankton growth in the Southern Ocean. We have therefore nuanced the idea of light limitation presented in lines 78-82 to incorporate discussion of sea ice concentration and euphotic zone depth rather than MLD alone. *"In contrast, the OOZ is far less productive due to persistent iron-deplete conditions, along with incidences of light limitation associated with high sea-ice concentrations (particularly in the central WG) and/or deep mixed layers (MLD) (Klunder et al., 2011; De Jong et al., 2012)."*

Line 82-83: "*Here, surface nutrients are never fully consumed and carbon export rates are low*". I have 2 comments regarding this statement. First of all, the upwelling of deep waters contributes to replenishing the open waters of the Weddell Sea with nutrients, which is also apossible reason why nutrients are never fully consumed there. In addition, it has been shown in several studies that primary production at the surface does not scale with carbon export at depth in the Southern Ocean, and more generally in the World's oceans. Thus, stating that carbon export is low because surface nutrients are not fully consumed is somewhat misleading. Two example papers on the topic are:

Maiti, K., Charette, M. A., Buesseler, K. O., and Kahru, M. (2013), An inverse relationship between production and export efficiency in the Southern Ocean, *Geophys. Res. Lett.*, 40, 1557–1561, doi:10.1002/grl.50219.

Le Moigne, F. A. C., S. A. Henson, E. Cavan, C. Georges, K. Pabortsava, E. P. Achterberg, E. Ceballos-Romero, M. Zubkov, and R. J. Sanders (2016), What causes the inverse relationshipbetween primary production and export efficiency in the Southern Ocean?, Geophys. Res. Lett., 43, 4457–4466, doi:10.1002/2016GL068480.

Response: We have edited the sentence to avoid misleading the reader as follows (line 80-82): *"Here, the co-limitation of productivity by light and iron often results in low rates of biological carbon export (Boyd et al., 2008; Boyd and Ellwood, 2010; Klunder et al., 2011; De Jong et al., 2012)."*

Line 91: "the biological carbon pump"
Response: We have updated the sentence with the suggested addition (line 89).

Line 94: "biological carbon pump"
Response: We have updated the sentence with the suggested addition (line 92).

Line 97: "biological carbon pump"
Response: We have updated the sentence with the suggested addition (line 95).

Line 113: I would also add that grazing can also play a strong role in controlling the amplitude and terminating these phytoplankton blooms.
Response: Agreed. We now mention that grazing, in addition to bottom-up controls, plays a role in controlling the amplitude and duration of the blooms (line 109-112): *"The size and duration of these blooms is ultimately controlled by macro- (e.g., nitrate and silicate) and micronutrient (e.g., iron) availability (Martin et al. 1991; Boyd 2004; Boyd and Ellwood 2010; Llort et al. 2015), as well as by zooplankton grazing (Smetack et al. 2004 and references therein; Arteaga et al. 2020)."*

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Line 118-120: this is valid when *Phaeocystis antarctica* do not form large colonies, which are prone to rapid export and less subjected to grazing. See for instance: *DiTullio, G., Grebmeier, J., Arrigo, K. et al. Rapid and early export of Phaeocystis antarctica blooms in the Ross Sea, Antarctica. Nature* **404**, 595–598 (2000). *https://doi.org/10.1038/35007061*

Response: We note that *P. antarctica* in their colonial form are rapid exporters of carbon; indeed, this notion is discussed at length in the *Discussion* section of the manuscript (e.g., line 891-893, line 915-919, line 924-925, line 929-931). We have thus removed lines 118-120 (to which the reviewer refers) from the *Introduction* as the information is not relevant to our findings.

Methods

Figure 1: can the authors increase the resolution of the Figure 1?Line 170: please correct "Filchner-Ronne Ice Shelf (FRIS)"

Response: The figure caption has been updated, and the resolution of the figure has been improved.

Line 172: please indicate what date does the sea ice concentration map correspond to.

Response: The date associated with the sea ice concentration data has been included in the figure caption (line 169).

Line 180: "*Chlorophyll a fluorescence*"

Response: We have updated the sentence with the suggested addition (line 175).

Section 2.2.2. Estimating nutrient depletion
I think that there are problems with the equations presented between lines 231 and 254. If Iwork out equations 1, 2a and 2b, I obtain the following:

X depletion(corrected) = X depletion – X depletion(melt water)
= [X]measured – [X]source – ([X]source – [X]melt water)
= [X]measured – 2 * [X]source + [X]melt water

Therefore, I believe that the nutrient drawdown would be overestimated if following this equation. Perhaps the authors have not translated their calculations correctly into equations? I think that this is the case as the nutrients' drawdown presented below in the paper (Figure 4) are reasonable and in line with the depletion one can visually calculate from Figure 3.

Similarly, I wonder if the equation for the sea ice fraction (fsea-ice) is also correct as some quick examples gives me negative sea ice fractions. For instance, for a measured salinity of 34 (for example), a winter water salinity of 34.4 and a sea ice salinity of 5, I obtain fsea-ice = (34- 34.4)/(34-5) = - 0.014.

Can the authors please verify these equations?
Response: We thank the Reviewer for noticing these errors. While the calculations were not correctly presented in the manuscript (and have since been updated), the correct equations were applied to the data.

The correct calculations are now included in the Methods section of the revised manuscript (lines 223-255; equations 1 and 2a-c in the manuscript):

$$X \text{ depletion} = [X]_{source} - [X]_{measured} \tag{1}$$

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$$X\, depletion_{(corrected)} = X\, depletion - X\, depletion_{(melt\ water)} \qquad (2a)$$

$$X\, depletion_{(melt\ water)} = [X]_{sea\text{-}ice}\,(f_{sea\text{-}ice}) + [X]_{source}(1\text{-}f_{sea\text{-}ice}) \qquad (2b)$$

$$f_{sea-ice} = \frac{salinity_{measured} - salinity_{source}}{salinity_{sea-ice} - salinity_{source}} \qquad (2c)$$

Results

Line 379: add a space "*WW,WDW*"

Response: We have updated the sentence as suggested (line 370).

Figure 2: The first paragraph of the Results section and the Figure 2 are very informative but the inserts of Figure 2, which describe the water masses down to 1500 m depth, should be enlarged and be given as additional sub-panels as most of the information on water masses is drawn from below 150 m depth. In addition, the resolution of Figure 2 is very poor and should be improved.

Response: Reviewers 1 and 2 had the same comment regarding Figure 2; we have thus altered the figure based on their recommendations. Shown below is the updated version of Figure 2 that includes the inserts as their own sub-panels:
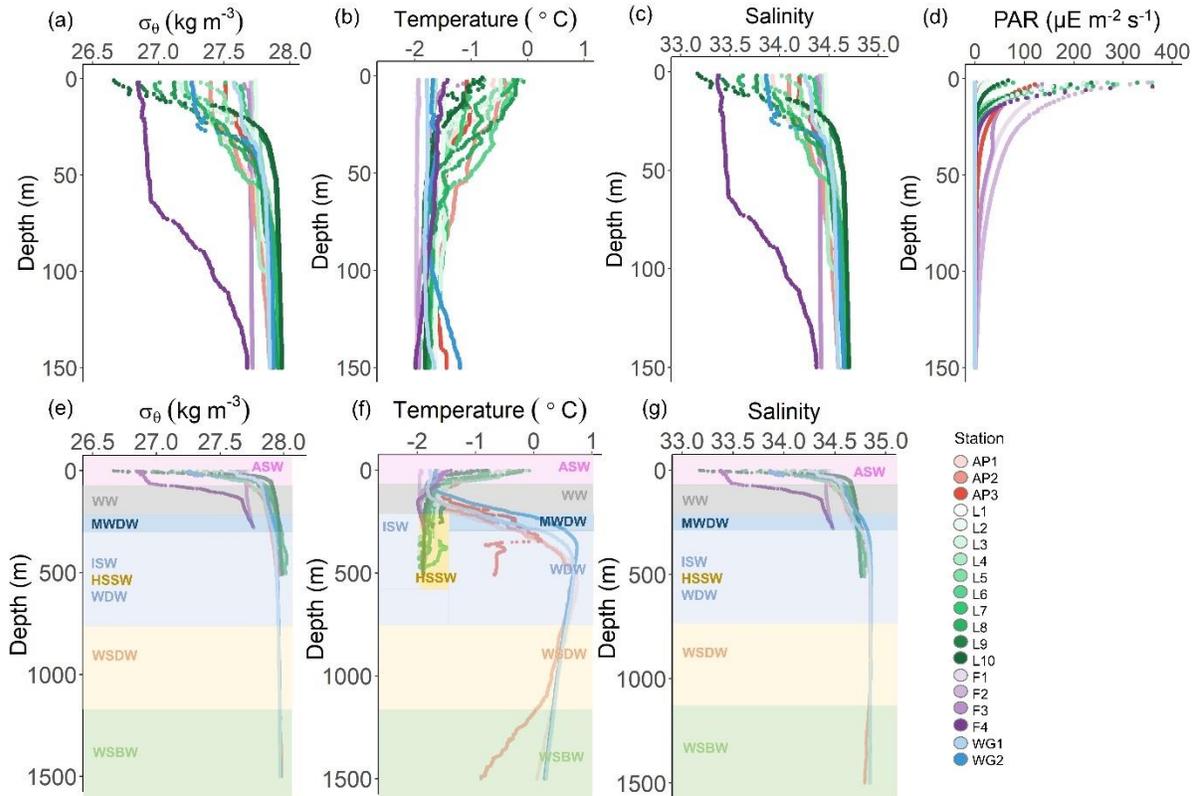
***Figure R1** (Figure 2 in the main manuscript): Depth profiles of (a) potential density (σ_θ), (b) potential temperature, (c) absolute salinity, and (d) photosynthetically active radiation (PAR) in the upper 150 m and (e) σ_θ, (f) potential temperature, and (g) absolute salinity in the upper 1500 m at all stations. The water*

masses present at each station, identified by their temperature and salinity characteristics, are denoted in panels (e-g) as follows: WSBW – Weddell Sea Bottom Water, WSDW – Weddell Sea Deep Water, WDW – Warm Deep Water, MWDW – Modified Warm Deep Water, ISW – Ice Shelf Water, HSSW – High Salinity Shelf Water, WW – Winter Water, ASW – Antarctic Surface Water. In panel (f), the dark yellow rectangle indicates HSSW. The general station locations are indicated by the different marker colours: red shades – Antarctic Peninsula, green shades – Larsen C Ice Shelf, blue shade –Weddell Gyre, light purple shades – early summer Fimbul Ice Shelf, and dark purple – late summer Fimbul Ice Shelf.

Line 399: "*depth profiles (0-150 m)*"
Response: We have corrected the figure caption accordingly (line 391).

Figure 3: perhaps this is because of the low resolution (which needs to be improved) but I find it difficult for the reader to distinguish between the sampling sites (for NH4+ and NO3- for example). Perhaps the symbols are too large? In addition, the error bars are barely visible.

Response: Reviewers 1 and 2 had the same comment regarding Figure 3; we have thus amended all the figures that show data from all stations by i) assigning each station its own colour and ii) representing each region (i.e., Antarctic Peninsula, LCIS, FIS, Weddell Gyre) with a different symbol shape. An example of the updated colour palette is shown below:
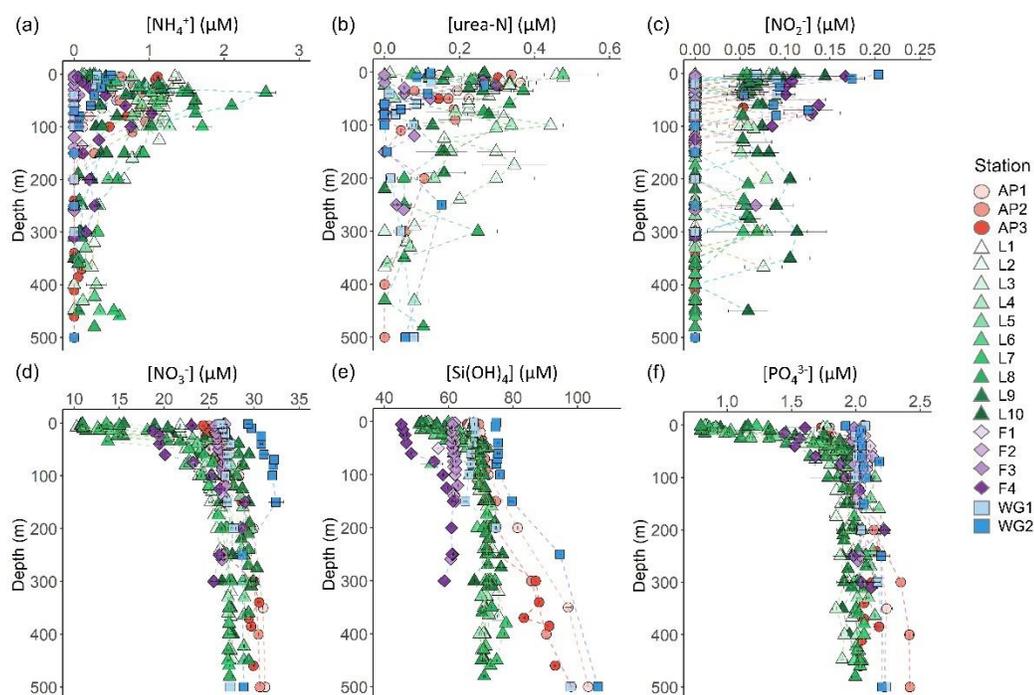
***Figure R2** (Figure 3 in the main manuscript): Depth profiles (0-500 m) of (a) NH_4^+, (b) urea-N, (c) NO_2^-, (d) NO_3^-, (e) $Si(OH)_4$, and (f) PO_4^{3-} concentrations. For all panels, the error bars represent ± 1 SD of replicate samples (n = 2-3). For NO_3^-, which was calculated as $NO_3^- + NO_2^- - NO_2^-$, error has been propagated according to standard statistical practices. Note that the x-axis scales in panels (d-f) do not start at zero.*

Figure 4: I find a little confusing that Figure 3d shows ratios of N depletion to Si depletion (so N:Si) when the figure legend (lines 455-459) and the main text (lines 467-470) discuss Si:N depletion ratios. Perhaps the author could invert the x and y-axes of Figure 4d to avoid any confusion.

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Response: Figure 4 has been updated based on Reviewer 1's suggestion as follows – note in particular the inversion of the axes in panel d:
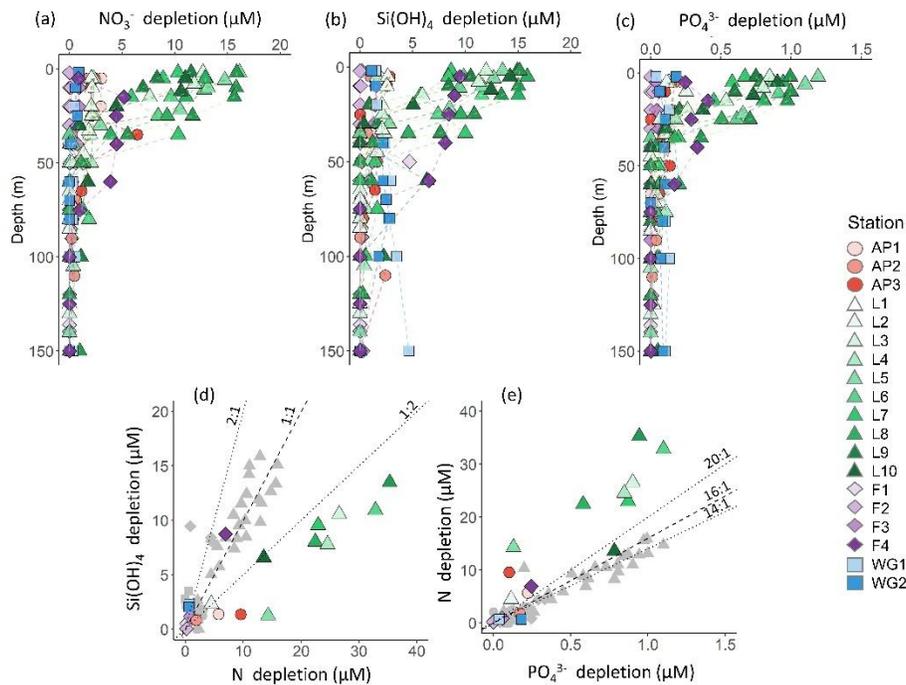
Figure R3 *(Figure 4 in the main manuscript): Depth profiles (0-150 m) of (a) NO_3^- depletion, (b) $Si(OH)_4$ depletion, and (c) PO_4^{3-} depletion at each station. Also shown are scatterplots of (d) $Si(OH)_4$ depletion versus total N depletion (coloured symbols; see text for details) and $Si(OH)_4$ depletion versus NO_3^- depletion (grey symbols) and (e) PO_4^{3-} depletion versus total N depletion (coloured symbols) and PO_4^{3-} depletion versus NO_3^- depletion (grey symbols) at each station. The dashed line in panel (d) represents the 1:1 Si:N depletion ratio, expected for iron-replete diatoms (Ragueneau et al. 2000; Hutchins and Bruland 1998; Takeda 1998; Mosseri et al. 2008), while the dotted lines represent the 2:1 Si:N ratio, expected for iron-limited diatoms (Arrigo et al., 1999; Franck et al., 2000; Brzezinski et al., 2003; Green and Sambrotto, 2006; Mosseri et al., 2008; Weber and Deutsch, 2010a; Martiny et al., 2013), and the 1:2 Si:N ratio, indicative of enhanced activity of non-siliceous phytoplankton. The dashed line in panel (e) represents the 16:1 N:P depletion ratio (the Redfield ratio), while the dotted lines represent the 20:1 N:P ratio, expected for P. antarctica, and the 14:1 N:P ratio, expected for iron-replete diatoms (Hutchins and Bruland 1998; Takeda 1998; Arrigo et al. 1999; Ragueneau et al. 2000; Mosseri et al. 2008).*

Line 476-479: There is a surprising result here with the FIS station showing some of the highest Si:N depletion ratios as well as some of the highest N:P depletion ratios. Looking at Table 1, it looks like this surprising result is mostly influenced by one station (Fimbul F2), where barely any nutrient depletion is observed (only NO3- showing a slight depletion 0.1 uM). So this early spring result seem to have a strong influence over the Fimbul average and might be taken cautiously. Perhaps the authors could mention this.

Response: Thank you for the note. We have included a sentence in the Results section that highlights the low $Si(OH)_4$ and PO_4^{3-} depletions measured at FIS in early summer, particularly at station F2, and have mentioned how this potentially skews the $Si(OH)_4:NO_3^-$ and $NO_3^-:PO_4^{3-}$ depletion ratios (line 453-455): *"In the latter case, the degree of $Si(OH)_4$ and PO_4^{3-} depletion was extremely low (Table 1), which likely accounts for the variable and anomalous $Si(OH)_4:NO_3^-$ and $NO_3^-:PO_4^{3-}$ depletion ratios computed for stations F1-F3."*

Figure 5 and Table 2: Why not present the POC values as well? And so make it a 9-panels figure? The same comment applies to Table 2, why not add a column for POC values? Even though POC values can retrieved from PON values and C:N ratios, it would be easy to just have POC values presented in Figure 5 and reported in Table 2.

Response: We had originally opted not to included POC concentrations in Figure 5 and Table 2 for the sake of concision but are happy to include the data as per the updated version of Figure 5 (shown below) and Table 2 (shown in the manuscript):
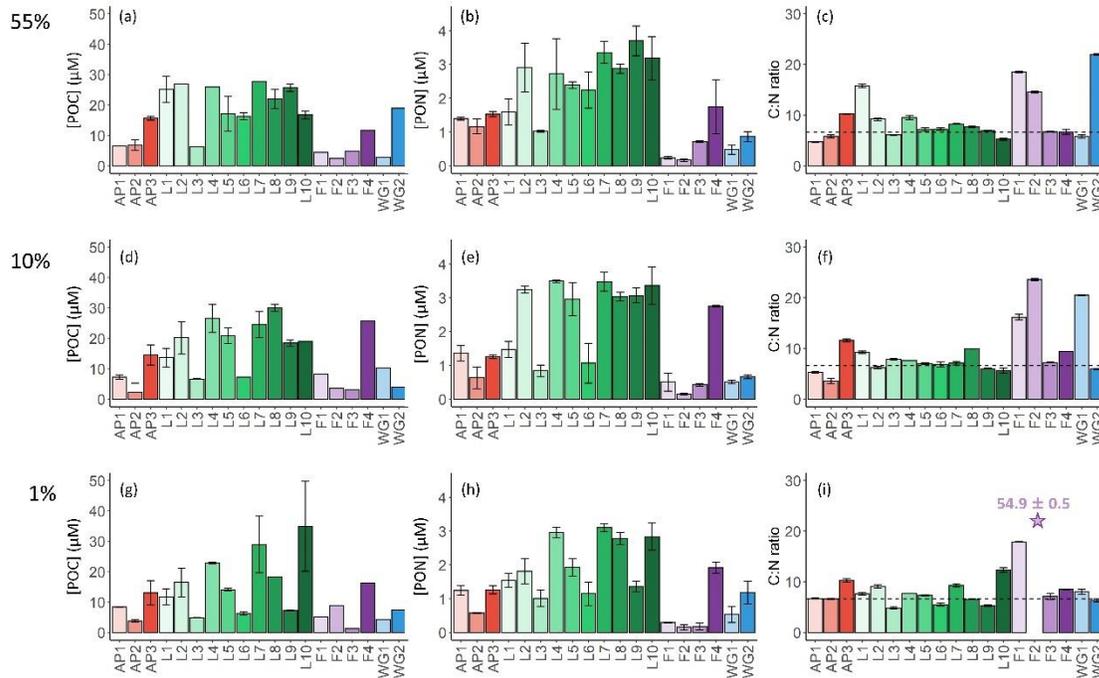
***Figure R4** (Figure 5 in the main manuscript): Bar plots of (a, d, g) POC concentrations, (b, e, h) PON concentrations, and (c, f, i) biomass C:N ratios measured at the 55% (a-c), 10% (d-f), and 1% light levels (g-i). The stations are labelled on the x-axis, and the general stations locations are indicated by the different colours: red shades – Antarctic Peninsula, green shades – Larsen C Ice Shelf, blue shade –Weddell Gyre, light purple shades – early summer Fimbul Ice Shelf, and dark purple – late summer Fimbul Ice Shelf. The dotted black horizontal line in panels (c), (f), and (i) shows the Redfield C:N ratio of 6.63. The purple star in panel (i) indicates the anomalously high C:N ratio estimated for the 1% PAR depth at station F2. The error bars represent ± 1 SD of replicate samples (n = 2-6). Where applicable, the error has been propagated according to standard statistical practices.*

Discussion

Line 637-641: Is it not that biomass plays a major role on light penetration? SST and stratification seem to be better indicator of NPP and $\rho NO3$. So overall, I am not entirely convinced that light plays the strongest role on NPP. In my mind, for such waters of coastal Antarctica or the sea ice zone, NPP is mainly influenced by a combination of factors where seaice melt drives first an increase in dissolved and particulate iron concentration near the surface, together with an enhanced stratification (also helped by ocean-atmosphere heat exchanges) that keeps phytoplankton close to the surface for growth. This is close to what the authors also argue lines 660-661, so line 637 leaves with a strange impression.

Response: We agree that variability in NPP is not well explained by euphotic zone depth alone ($R^2 = 0.1$;

p-value = 0.40). We have updated lines 667-674 of the manuscript to indicate that NPP is better explained by biomass concentration (which affects light penetration in addition to total carbon production) and temperature. However, ρNO_3^- *is* significantly correlated with the euphotic zone depth at the non-LCIS stations ($R^2 = 0.65$; p-value = 0.009), possibly because of the high energy (i.e., light) requirement of NO_3^- reduction (Dortch 1990). Additionally, ρNO_3^- appears less strongly controlled by temperature ($R^2 = 0.32$; p-value = 0.011) and biomass (i.e., PON concentration; $R^2 = 0.29$; p-value = 0.017).

Line 643-650: I have a similar interpretation of the relationship between $\rho NH4$ and the depth of the euphotic zone. I believe that this is an indirect relationship linked to the biomass of phytoplankton present in the water column and which has a strong influence on both the penetration of light and the remineralization of organic matter that leads to the production of regenerated N. The authors state that they conclude "*that $\rho NH4$ and $\rho urea$ were predominantly constrained by the availability of regenerated N rather than by light*". I think that they could mention the potential role of biomass in attenuating the penetration of light to explain these relationships with the depth of the euphotic zone.
Response: We agree that the production of NH_4^+ and urea within the euphotic zone is linked to biomass (as pointed out in the original manuscript). In the amended version, we now discuss the role of biomass in i) attenuating light and ii) as a source of organic matter for the production of DON and NH_4^+ (line 679-685).

Line 672: Oxygen is also typically saturated as a result of phytoplankton and ice algae production.
Response: In the amended version of the manuscript, we state that oxygen in sea-ice is typically saturated due to phytoplankton and sea-ice algae productivity (indeed, this strengthens our argument), in addition to rapid equilibration with the atmosphere (line 711).

Line 673: In fact, sea ice formation leads to an increase in the O2 concentration of the underlying water (through brine rejection and decreased solubility mainly). See the following paper: https://online.ucpress.edu/elementa/article/doi/10.12952/journal.elementa.000080/112740/ Assessing-the-O2-budget-under-sea-ice-An
So I suggest to rephrase the following sentence: "*Sea-ice formation should not, therefore, drive a notable change in the oxygen content of ASW.*"
Response: Agreed. We have updated the sentence to read as follows: "*Sea-ice formation should not, therefore, drive a decrease in the oxygen content of ASW.*" (line 711-712).

Line 687-89: I very much agree with the authors interpretation here. In addition, it might be worth to add that higher SST promote phytoplankton productivity.
Response: We intentionally did not state that higher SST promotes productivity as the specific uptake rates do not increase with increasing SST (Figure R5); however, we have included a section in the Supplemental Information (Section S5) where we discuss the relationship (or lack thereof) between SST and phytoplankton growth, referencing the figure below.
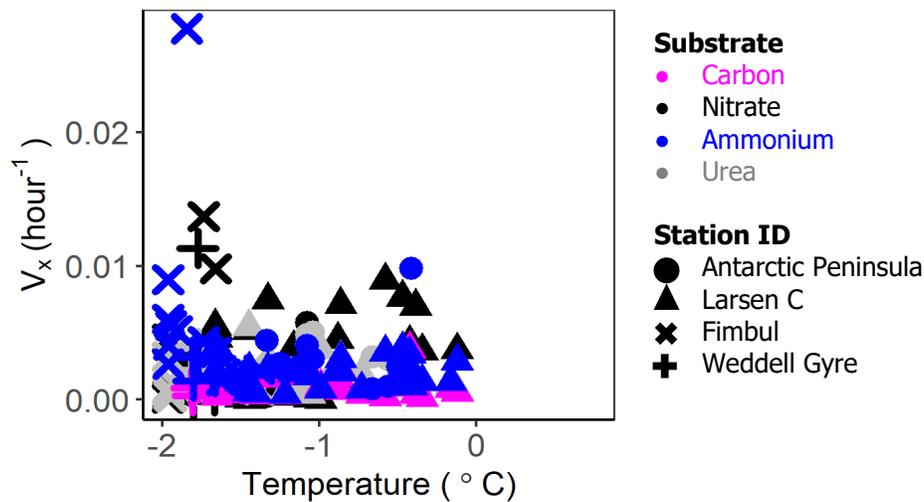
Figure R5. *Specific rates of carbon (pink), nitrate (black), ammonium (blue), and urea (grey) uptake versus sea surface temperature at all depths at each station.*

Line 692: "*and potential organic carbon export*".
Response: We have updated the sentence with the suggested addition (line 730).

Line 699:701: there are some problems with the references here.

Response: The issue with the references (i.e., inadvertent replication) has been corrected. Note, that this figure is now Figure S5 as we included a new figure in the main manuscript pertaining to Si:N and N:P depletion ratios versus % of diatoms and *P. antarctica,* and % of N uptake as either NO_3^- or regenerated N versus % of diatoms and *P. antarctica.*

Line 703: does the grey box in Figure 11d indicate the average MLD plus or minus the standard error?
Response: The grey box does indicate the MLD average ± standard deviation. The figure caption will be updated to make this clear: "***Figure S5.*** *Maps of (a) SST, (b) sea surface salinity, and (c) surface oxygen concentrations and (d) depth profiles of oxygen concentrations in the region of LCIS at the time of sampling. SST and salinity data were acquired from the underway (~7 m inflow) ferrybox, while the oxygen concentrations were measured via the oxygen sensor on the CTD profiler, calibrated against discrete seawater samples measured for dissolved oxygen by Winkler titration (Carpenter, 1965; Grasshoff et al., 1983; Hutchinson et al., 2020). The symbols in panel (d) are coloured by potential density (σ$_θ$), with the circles indicating the "non-upwelling" stations and the triangles the upwelling stations. The grey box in panel (d) indicates the average mixed layer depth (MLD) ± 1 SD across the stations at LCIS (n=10). The bathymetry data in panels (a-c) were taken from ETOPO1 (NOAA National Geophysical Data Center, 2009)." (section S5 in the Supplemental Information)*

Line 731: a word seems to be missing from this sentence: "*due to our results(?) not accounting for regenerated N uptake.*"
Response: We have updated the sentence with the suggested addition (line 759-760).

Line 770: "biological carbon pump"
Response: We have updated the sentence with the suggested addition (line 804).

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Line 852: "*the absolute carbon export flux potential*"
Response: We have updated the sentence with the suggested addition (line 885).

Line 898-901: "*carbon export potential*". Here, while this hypothesis may be true, the authorsmight also want to acknowledge that Biological carbon export is usually stronger later in the season in the Southern Ocean.
Response: From previous research conducted in the Weddell Sea and elsewhere in the Southern Ocean, the idea that biological carbon export is stronger later in the season does not appear to always be true. For example, Goeyens et al. (1995) found that as the summer season progresses, the CCSZ and MIZ of the Weddell Sea switch from being characterized by predominantly nitrate-fueled phytoplankton growth (i.e., high export) to regeneration dominated (i.e., low export). This switch occurs as a result of increased microbial loop activity in surface waters, along with decreased iron (and in some cases silicate) availability. In the revised version of the manuscript, we will reference additional studies that have shown such a seasonal trend (e.g., Goeyens et al. 1995; Lourey et al. 2003; Philibert et al. 2015; Smart et al. 2020) in order to strengthen our argument (line 928-929).
Additionally, we have found no evidence that biological carbon export is usually stronger later in the season and have therefore not included any mention of this idea in the updated version of the manuscript. We do, however, welcome references from the reviewer should they know of any, and would be happy to include them in the manuscript, acknowledging that a "switch" from NO_3^- to NH_4^+ dependence later in the season is not always the case.

Line 927: "*It is projected that these conditions will yield blooms of heavily-silicified diatom species (Deppeler and Davidson 2017) that are known to sink rapidly out of the mixed layer or,if consumed, their frustules are expected to survive the gut passages of copepods, resulting in increased carbon export (Assmy et al. 2013).*"
I disagree with this interpretation from both Deppeler and Davidson (2017) and the authorsfor several reasons. First, the heavily silicified diatoms species that are referred to here are rarely coastal species and more typical of the open Southern Ocean such as the ACC (see Smetacek et al., 2004). In addition, these species are mostly silica exporters and not carbon exporters contrarily to the less silicified diatoms species (Assmy et al., 2013 and Smetacek etal., 2004).

Smetacek, V., P. Assmy and J. Henjes (2004). "The role of grazing in structuring Southern Ocean pelagic ecosystems and biogeochemical cycles." Antarctic Science 16(4): 541-558.
Response: We agree, Deppeler and Davidson (2017) is probably not the most appropriate reference for us to use here, and so we have removed it. We take the Reviewer's point about carbon versus silica export, noting that there is still a lack of consensus on this issue. For instance, Assmy et al. (2013) found that under iron replete conditions, heavily-silicified diatoms were both silicate and carbon exporters: "*Our results suggest that large-scale iron fertilization of the silicate-rich Southern Ocean will not change silicon sequestration but will add carbon to the sinking silica flux.*" (p. 1). At the same time, it has been shown that heavily-silicified diatoms with robust frustules may be grazer-protected, potentially ultimately leading to the sinking of empty shells (i.e., devoid of carbon) (Smetacek et al. 2000; Assmy et al. 2013). In the event that these diatom species come to dominate at LCIS, the shift from *P. antarctica* to these diatoms would further decrease regional carbon export potential, supporting our hypothesis that a shift from a *P. antarctica*- to diatom-dominated community will lead to decreased carbon export. We have nuanced the discussion to include the complexities summarized above, and have incorporated the references suggested by the reviewer. (line 973-978)

In addition, I believe that this section omits to mention the benthic food web. If the falling phytoplankton cells reach the sediments of the continental shelves, they are likely to becomepart of the benthic food web and not contribute to biological carbon export along with the formation of bottom waters. I think that

authors should mention this possibility.

Response: We thank the Reviewer for bringing to our attention the potential role of the benthic food web in the carbon cycle, which is likely to be important in the vicinity of the Larsen C and Fimbul Ice Shelves where the bottom depth is typically <500 m. We have updated this section of the manuscript to include discussion of the role of the benthic food web and its impact(s) on biological carbon export, particularly in regions where no water mass formation occurs (line 942-961,). In brief, organic matter that is exported from the euphotic zone near the Antarctic ice shelves can undergo remineralization, with the resultant CO_2, when associated with deep water mass formation, being stored in these water masses for centuries. Alternately, the organic matter reaches the sediments where a small fraction will be buried and thus removed from the ocean-atmosphere system, while the rest of it is utilized by the benthic community and ultimately converted into CO_2. Nutrients and CO_2 that are recycled by the benthos can be resupplied to the surface during upwelling, providing an opportunity for the remineralized CO_2 to escape back to the atmosphere, thus driving no net carbon removal.

In our discussion, we have relied on the following references (e.g., line 949):

Isla, E., Rossi, S., Palanques, A., Gili, J.-M., Gerdes, D. and Arntz, W. (2006). "Biochemical composition of marine sediments from the eastern Weddell Sea (Antarctica): High nutritive value in a high benthic-biomass environment". Journal of Marine Systems 60: 255-267.

Isla, E., Gerdes, D., Rossi, S., Fiorillo, I., Sañé, E., Gili, J.-M., and Arntz, W. (2011). "Biochemical characteristics of surface sediments on the eastern Weddell Sea continental shelf, Antarctica: is there any evidence of seasonal patterns?". Polar Biology 34: 1125-1133.

Pineda-Metz, S.E.A., Isla, E., and Gerdes, D. (2019). "Benthic communities in the Filchner Region (Weddell Sea, Antarctica)". Marine Ecology Progress Series 628: 37-54.

Line 941: "*potential carbon export*"
Response: We have updated the sentence with the suggested addition (line 988).

Reviewer 2: Anonymous

Comment on bg-2021-122
Anonymous Referee #2

Referee comment on "Summertime productivity and carbon export potential in the Weddell Sea, with a focus on the waters adjacent to Larsen C Ice Shelf" by Raquel Flynnet al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-122-RC2, 2021

In this manuscript the authors present data on nitrogen (NO3, NH4' urea) and carbon uptake and nitrification at different locations in the Southern Ocean, namely, the Antarctic peninsula, Larsen C ice shelf, Weddell Gyre and Fimbul ice shelf. The manuscript presents a large set of valuable data in poorly studied and contrasted systems from the shelves intoopen waters of the Weddell Gyre. The manuscript is well written and the study is well worth publishing in Biogeosciences. However, given the large dataset and scope of the study (compare productivity, macronutrients and regenerated N uptake and assemblage composition and environmental variables), I believe and the results presented here woulddeserve a more thorough and robust analysis (including correlation or multi-correlation statistics) for the discussion points:
Response: In an attempt at a more thorough and robust analysis of our data, we conducted non-metric

Multidimensional Scaling (nMDS) and Canonical Correspondence Analyses (CCA). Detailed explanations of these methods and their results (as provided below) have been included in the Supplemental Information (Section S4) and we have refer the reader to this material when appropriate. However, we feel that it is not beneficial to include this content in the main manuscript given that the results are not particularly informative beyond the conclusions that we have already drawn.

New text (*section S4 of the Supplemental Information)*:
To confirm the relationship of the phytoplankton community to their environment, we performed a Non-metric Multidimensional Scaling (NMDS) and Canonical Correspondence Analysis (CCA) using data from all stations.

NMDS (Figure R6a) is an indirect gradient analysis approach that produces an ordination based on a distance or dissimilarity matrix using Bray-Curtis distance. The NMDS was used to visualise the community composition differences between sites and samples (data were square-root transformed, Bray-Curtis dissimilarity, 999 permutations using Canoco 5). The LCIS and FIS sites were similar to each other and different from the AP and WG sites. AP3 emerged as an outlier, likely due to the very low species diversity and phytoplankton abundance at that station.

The CCA (Figure R6b) summarizes the variation in species composition explained by the environmental variables. Monte Carlo permutation tests (499 permutations) were performed to assess the significance of the canonical axes. The environmental variables considered in the analysis are plotted as arrows originating from the centre of the graph. The origin represents the mean value of each variable, and the arrow represents an increase in the value of that variable. The distance between the symbols approximates the dissimilarity of their species composition as measured by their chi-square distance. Prior to the analysis, co-linearity among possible predictor variables was assessed using variance inflation factors (VIF), with VIFs > 5 deemed co-linear (Zuur et al., 2009, 2010). Therefore, salinity, density, oxygen and PO_4^{3-} were omitted from the analysis. The CCA uses the same species data as plotted in the NMDS but shows the species distribution as constrained by the environmental variables. The AP and WG stations were characterized by higher-than-average temperature (and urea concentrations, although this relationship was less significant). Stations L6, L10, and F3 were characterized by lower-than-average temperature and PAR (consistent with recent sea ice melt and deep MLDs). Station F4 was characterized by higher-than-average PAR and NO_2^- and NO_3^- concentrations, and lower-than-average temperature and NH_4^+ and $Si(OH)_4$ concentrations. The LCIS stations to the left of the centre of Figure R6b (i.e., L1, L2, L4, and L7) were characterized by lower-than-average NO_3^-, NO_2^-, and PAR (likely due to elevated biomass at these stations) and elevated concentrations of NH_4^+ and $Si(OH)_4$.

The NMDS and CCA confirm that the LCIS stations were statistically different from the AP and WG stations, as indicated by the observed variability in the Si:N:P depletion ratios and the dominant phytoplankton groups (i.e., *P. antarctica* at LCIS and diatoms at the AP and WG stations) between the station locations. The LCIS stations were generally characterized by elevated NH_4^+ and urea concentrations (Figures R2a and b), the result of increased heterotrophy and an upregulated microbial loop, and high $Si(OH)_4$ concentrations (Figure R2e), likely the result of low $Si(OH)_4$ uptake by a phytoplankton community consisting of a low abundance of diatoms (Figure 9 in main manuscript). Furthermore, the lowest Si:N and highest N:P depletion ratios were observed at LCIS (consistent with *P. antarctica* dominance), while the highest Si:N and lowest N:P depletion ratios occurred at FIS and in the WG (consistent with diatom dominance; Figures R3 and R8; Table 1 in the main manuscript). The variability in phytoplankton community composition appears to have been largely driven by the mechanisms controlling water column stratification, as discussed at length in the main text.
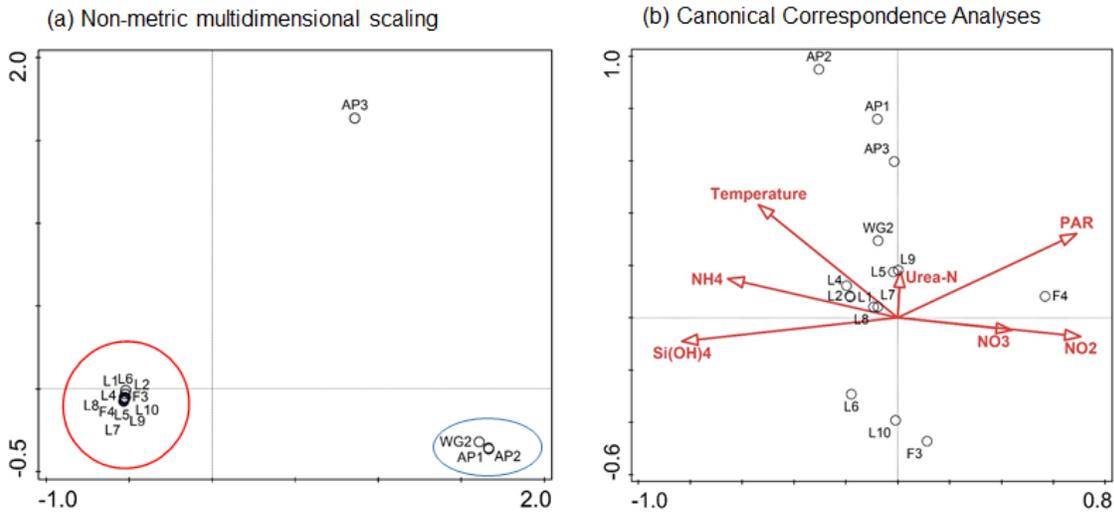
Figure R6: (a) nMDS analysis of the variability in phytoplankton community composition among stations and (b) CCA ordination of phytoplankton species constrained by environmental variables visualized as a biplot with sampling stations labeled.

- In most of the figures presenting profiles it is not possible to distinguish individual profiles. Further, in particular for the LCIS, the location of the different stations is not reported in these figures. This is relevant given that stations sampled were located in areas with very different topography, sea-ice cover, freshwater input, circulation, watermass characteristics, mixed layer depths not to mention plankton biomass and composition.
Response: As per the request of both Reviewers, we have updated the figures and have assigned individual colours to each of the stations (e.g., see Figures R1-3 above). This change should help the reader to differentiate between the stations. We would prefer not to include separate panels for the LCIS stations as this will make the figures very large and potentially unwieldy (e.g., Figure 3 would have 12 panels). That said, if the Editor feels that the LCIS data should be presented separately, we are happy to include such figures in the Supplemental Information section of the manuscript.

- Line 632-641: NPP is compared to Zeu. However, the large variability in Zeu in this study is not explained. Information on irradiance and sea-ice cover would be relevant here. Further, the actual light regime experienced by the plankton is related to the MLD and most probably phytoplankton standing stocks. These are not shown and analyzed in Fig. 10.
Response: We agree that NPP is not well explained by the euphotic zone depth alone ($R^2 = 0.1$; p-value = 0.40). We have updated lines 637-641 to indicate that NPP is better explained by biomass concentration (which affects light penetration in addition to total carbon production; see Figure R7c below) and temperature (see Figure 10d in the main manuscript) (line 667-674).

- Lines 643-661: no graph is presented on the relation between NH4 and urea uptake and substrate concentrations or NPP.
Response: We agree that presenting a figure showing the relationship between NH_4^+ and urea uptake and substrate concentration might be useful to the reader. We have thus included a version of the figure shown below (Figure R7) in the amended manuscript (now updated Figure 10), along with some text detailing the relationship between regenerated nitrogen uptake rates and substrate concentration (Figures R7d and e) (line 679-685).

- Line 652-657: No graph showing the relationship between particulate stocks (POC, PON) and regenerated N concentrations and uptake is shown.

Response: We agree that presenting a figure that shows the relationship between NH_4^+ and urea concentration and biomass might be useful to the reader. We have included a version of Figure R7 in the amended manuscript along with a section of text exploring the relationship between regenerated nitrogen concentrations and uptake rates and substrate concentration (Figures R7c-e) (line 681-685): *"Indeed, the stations with the deepest Z_{eu} were characterized by low concentrations of particulate organic matter and regenerated N (Figures 3a-b and 5), leading us to conclude that ρNH_4^+ and $\rho urea$ were predominantly controlled by the availability of regenerated N (Figures 10d-e and S3b; section S4 in the Supplemental Information). This conclusion is supported by the positive relationship observed between ρNH_4^+ or $\rho urea$ and the coincident NH_4^+ or urea concentrations (Figure 10d-e)."*
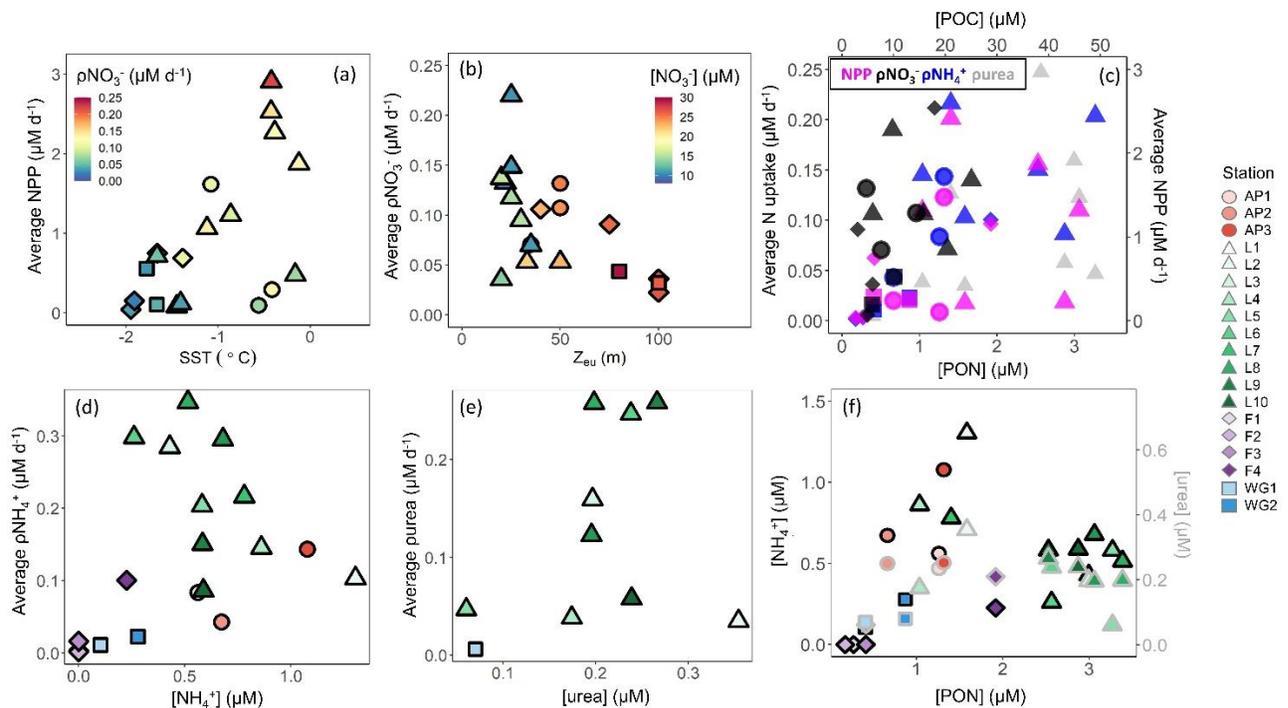
Figure R7 *(Figure 10 in the main manuscript): Euphotic zone-averaged rates of (a) NPP versus SST, (b) ρNO_3^- versus euphotic zone depth (Z_{eu}), (c) N uptake (left y-axis) versus PON (bottom x-axis) and NPP (right y-axis) versus POC (top x-axis), (d) ρNH_4^+ versus NH_4^+ concentration, and (e) $\rho urea$ versus urea concentration, as well as (f) the concentrations of NH_4^+ (black outlined symbols; left y-axis) and urea (grey outlined symbols; right y-axis) versus PON at each station. The symbols in panel (a) are coloured by ρNO_3^-, in panel (b) by NO_3^- concentration, and in panel (c) by NPP (pink), ρNO_3^- (black), ρNH_4^+ (blue), and $\rho urea$ (grey).*

- Lines 706-716: Several studies have shown that floristic composition (diatoms vs otherphytoplankton groups, and diatom assemblage composition) is probably more relevant. How do the uptake ratios change with proportion of diatoms and diatom assemblage composition? (see also comments in the annotated manuscript lines 466-470).

Response: We agree that floristic composition (i.e., not the diatom community alone) will influence the Si:N and N:P depletion ratios. However, due to the dominance of diatoms over other species at the non-

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LCIS stations (i.e., AP, FIS and WG) and *P. antarctica* at LCIS (see Figure 9f in the main manuscript), the depletion ratios are also an indicator of iron availability. In other words, there are examples of stations characterized by near-identical proportions of diatoms but where the depletion ratios vary considerably (e.g., L10 versus F4) – here, something other than floristic composition must be invoked to explain our observations. We have, however, pointed out in the amended manuscript that floristic composition can also influence the depletion ratio, and have included Figure R8 as part of our discussion (see below) (line 791-793).

- Lines 737-750: Why not compare the uptake rates of N species and uptake ratios with assemblage composition? (i.e. vs % diatoms, % non diatom biomass, and vs dominant diatom species types?).

Response: We thank the reviewer for the suggestion – we agree that including some discussion regarding how the depletion ratios and N uptake rates vary with changes in the phytoplankton assemblage would be a useful addition to our analysis. We have updated the manuscript to include discussion of the influence of diatom vs. *P. antarctica* contributions to total phytoplankton biomass on the depletion ratios and dominant source of N taken up by the phytoplankton (see Figure R8 below) (line 768-801). Briefly, at the stations where diatoms dominated, the $Si(OH)_4:NO_3^-$ depletion ratios were elevated and high nitrate uptake rates were measured relative to the LCIS stations where *P. antarctica* was dominant (Figures R8a and c). In contrast, at the stations where *P. antarctica* dominated, the $NO_3^-:PO_4^{3-}$ depletion ratios were elevated, and the highest regenerated nitrogen uptake rates were measured (Figures R8b and d).
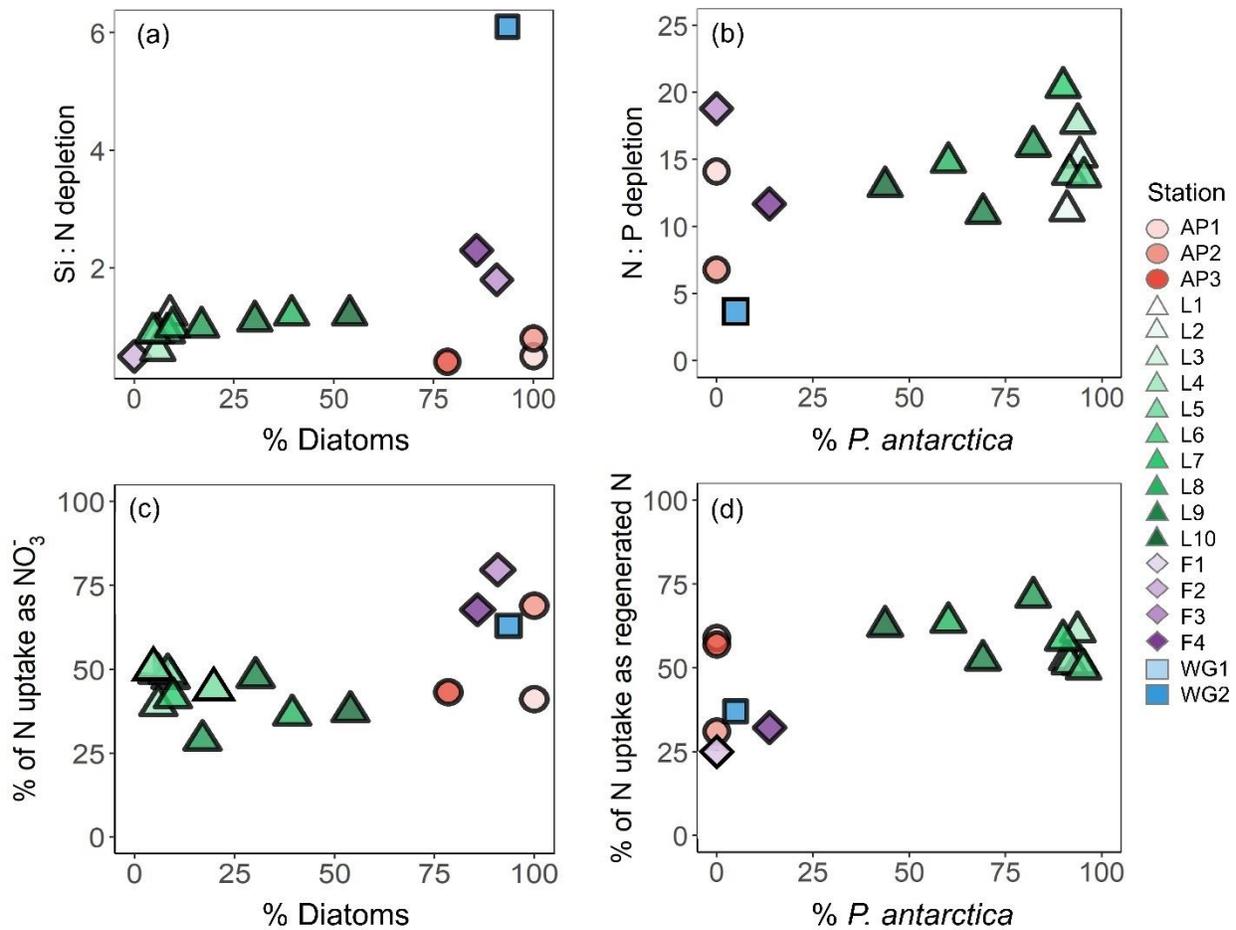
Figure R8 *(Figure 11 in the main manuscript): Scatterplots* of (a) the Si:N depletion ratio (i.e., (i.e.,

$Si(OH)_4$-to-total N depletion) versus the % diatom abundance, (b) N:P depletion ratio (i.e., total N-to-PO_4^{3-} depletion) versus the % *P. antarctica* abundance, (c) % of total N (i.e., $NO_3^- + NH_4^+ +$ urea) assimilated as NO_3^- versus the % diatom abundance, and (d) % of total N assimilated as regenerated N (i.e., $NH_4^+ +$ urea) versus the % *P. antarctica* abundance at each station.

- Lines 885-897: this part is based on the assumption that diatoms do not take up NH4. This is not accurate (see Mosseri et al., 2008 and Smetacek et al., 2012).

Response: We are not arguing that diatoms do not take up NH_4^+ but rather that the high concentrations of NH_4^+ present at LCIS during our sampling may prevent them, and possibly other phytoplankton species too, from (efficiently) consuming the available NO_3^- (i.e., NH_4^+ inhibition of NO_3^- uptake; Dortch 1990; Raey et al. 2001; Philibert et al. 2015; Glibert et al. 2016). Additionally, although diatoms *can* consume NH_4^+, it is possible that they are outcompeted by small cells that tend to be NH_4^+ specialists (i.e., as per Goeyens et al. 1995). We have ensured that these ideas are clear in the amended version of the manuscript, particularly that we are not suggesting diatoms do not take up NH_4^+ (line 908-909).

- Lines 921-935: this is all highly speculative and based on several assumptions (among others that *Phaeocystis antarctica* blooms lead to high C export). I would recommend to base this part on the actual observations of this study. This part of the discussion is also based on the influence of sea-ice cover, but no information on sea-ice cover is given in the manuscript. Water depth might be also relevant here. Further, some stations at LCIS show very low salinity. This has not been analysed and discussed at all (among others, are these low salinities due to sea-ice melt or glacial discharge?).

Response: We agree that we could better integrate our actual findings into our discussion of the potential implications of climate change for the floristic dynamics at LCIS. In the updated manuscript, we have used station L10 as an example of the effect that increased stratification due to sea-ice melt may have on the phytoplankton community and carbon export potential (lines 965-971). At L10, there had recently been sea-ice melt (Figure R1a and 1c), which increased water column stratification. Here, we observed a different phytoplankton community with higher relative diatom abundance compared to the other LCIS stations (Figure 9 in the main manuscript). The idea is that as SST rises and sea-ice melts, a shift from a *P. antarctica*- to a diatom-dominated phytoplankton bloom is expected because diatoms flourish under conditions of high light and elevated water column stratification (i.e., in areas of recent sea-ice melt) (e.g., Boyd and Doney 2002; Arrigo and van Dijken 2003; Petrou et al. 2016; Ferreira et al. 2020). This floristic shift hypothesis is supported by our observations at L10, whereby sea-ice melt induced rapid water column stratification, allowing for the dominance of diatoms over *P. antarctica*. This result contrasts what was observed at the other LCIS stations; here, *P. antarctica* dominated the biomass due to the low light conditions at the onset of the bloom, the result of deep mixed layers that initially characterize coastal polynyas.
Please see below for a response to the question of the potential influence of water depth.

Additional comments are given in the annotated manuscript.
Other comments:
Based on Fig. 1, it seems there are large differences in water depth between stations (i.e. some stations were taken in the continental shelf others in open waters). Information on water depth and how this affects water mass characteristics and MLDs would be helpful, in particular for Larsen C profiles.

Response: The bottom depth of the various stations at LCIS does not appear to influence the water mass characteristics in the surface mixed layer (Figure R9; NO_3^- depletion versus bottom depth; $R^2 = 0.11$ and mixed layer depth versus bottom depth; $R^2 = 0.01$); we would thus prefer not to discuss water depth in the manuscript given that it is already extremely data-rich and lengthy. The variability in productivity and carbon export appears to be influenced most strongly by variability in surface water stratification, which is driven by sea ice melt and/or upwelling.

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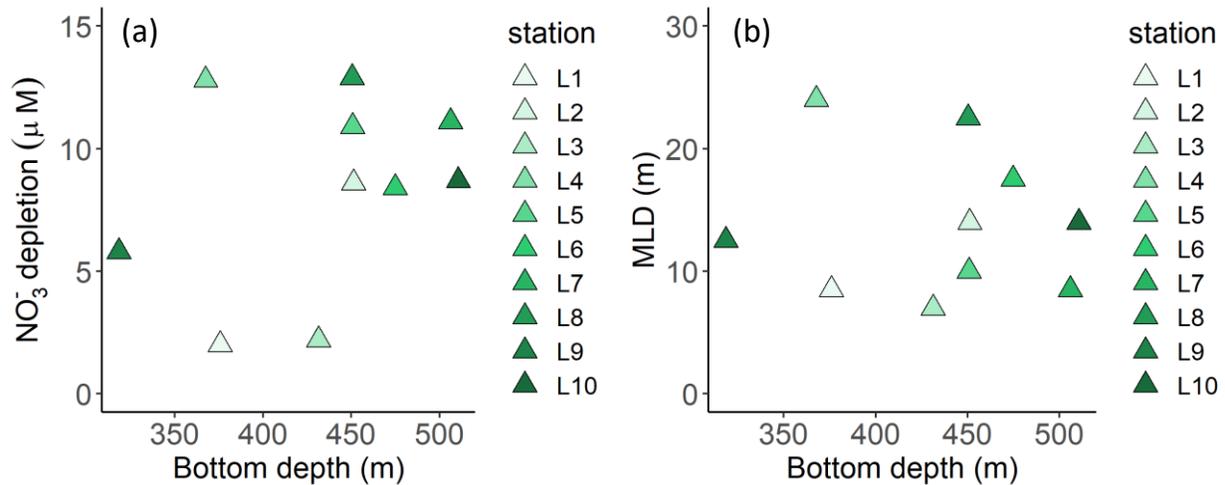
Figure R9: *(a) NO_3^- depletion and (b) mixed layer depth as a function of bottom depth at all LCIS stations.*

Lines 81-82: the importance of shallow MLD for phytoplankton growth in the SO is also more and more contested see Hoppe et al., 2017, Deep-Sea Research II, 138: 63-73. doi:10.1016/j.dsr2.2015.10.005; Smetacek et al., 2012, Nature, 487: 313-319. doi:10.1038/nature11229 and Behrenfeld 2010, Ecology, 91(4), 977–989 for a more general discussion.

Response: Both Reviewers note that a shallow mixed layer may not be as important as previously thought for phytoplankton growth in the Southern Ocean. We have therefore nuanced the text about light limitation (lines 78-82) to incorporate discussion of sea ice concentration and euphotic zone depth rather than MLD alone. We have updated the text and references to include the ideas presented in Hoppe et al. (2017) and Smetacek et al. (2012), including their hypotheses that the main limitations on NPP are nutrient availability (particularly iron) and grazing pressure.

Line 115: The blooming stage in all bloom forming *Phaeocystis* species (including *P. antarctica*) is the colonial form. These are not free living "small cells" but structures in the size range covered by diatoms.

Response: We respectfully disagree. *P. antarctica* have been shown to shift between their single cell- and colonial forms in response to bottom-up and top-down pressures (Lubbers et al. 1990; Hamm et al. 1999; Schoemann et al. 2001; Smith et al. 2003; Rousseau et al. 2007; Bertrand et al. 2007; Feng et al. 2010; Bender et al. 2018). At the onset of a bloom, *P. antarctica* generally exist in their single cell form as they are still building up their biomass (Rousseau et al. 2007; Bender et al. 2018). As they grow, they shift towards their colonial form in order to escape predation and outcompete other phytoplankton groups for the available nutrients (particularly trace metals) (Lubbers et al. 1990; Hamm et al. 1999; Schoemann et al. 2001; Bertrand et al. 2007; Feng et al. 2010). To better support our argument, we have included the following additional references in the amended version of the manuscript (line 116):

Moisan, T. A. and Mitchell, B. G.: Photophysiological acclimation of Phaeocystis antarctica Karsten under light limitation, Limnol. Oceanogr., 44, 247–258, https://doi.org/10.4319/LO.1999.44.2.0247, 1999.

Arrigo, K. R., Robinson, D. H., Worthen, D. L., Dunbar, R. B., DiTullio, G. R., VanWoert, M., and Lizotte, M. P.: Phytoplankton community structure and the drawdown of nutrients and CO2 in the Southern Ocean, Science (80-.)., 283, 365–367, https://doi.org/10.1126/science.283.5400.365, 1999.

Line 123-124: More recent literature strongly indicates that diatoms in the SO also takeup among others ammonia (see: Mosseri, 2008 and Smetacek et al., 2012).

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Response: As outlined above, we are not arguing that diatoms do not take up NH_4^+, and we have made this explicit in the updated version of the manuscript (line 118-120). Instead, we make the point that in the presence of high light, iron and $Si(OH)_4$ concentrations, diatoms will typically consume NO_3^- over NH_4^+ as i) NO_3^- is present in Southern Ocean surface waters at substantially higher concentrations than NH_4^+ and ii) the lower surface area-to-volume ratio of the (larger) diatoms makes it harder for them to compete with smaller cells for a less abundant resource (i.e., NH_4^+) – they more rapidly become diffusion limited.

The study conducted by Smetacek et al. (2012) to which the Reviewer refers shows that under iron-replete conditions, diatoms consume very high quantities of NO_3^-: "*Nitrate uptake until the bloom peak on day 24 accounted for 80% of PON production*" (p. 5). By contrast, the study conducted by Mosseri et al. (2008) on the Kerguelen Plateau found that diatoms actively grew on NH_4^+ (39-77% of total N uptake) produced by the intense heterotrophic activity that followed the phytoplankton bloom. In this case, the measured NH_4^+ concentrations in the mixed layer were always >0.5 μM and thus potentially inhibitory to diatom NO_3^- uptake (Dortch 1990; Raey et al. 2001; Philibert et al. 2015; Glibert et al. 2016) – in other words, diatoms may have relied heavily on NH_4^+ in the Mosseri et al. (2008) study because i) the NH_4^+ flux was large (even if the standing stock was only modest) and ii) these already-dominant phytoplankton could not effectively consume NO_3^- because of the elevated NH_4^+ concentrations (i.e., NH_4^+ inhibition).

Regardless, we have updated the manuscript to clarify that while diatoms can and do consume NH_4^+, they are likely outcompeted by *P. antarctica* and other smaller phytoplankton at LCIS for the available NH_4^+ (line 118-120).

Figures 2, 3, 7: It is impossible to see the profiles properly due to the amount of information. Perhaps Larsen C data could be plotted in a different figure. See alsocomments in the manuscript for Fig. 2.
Response: As per the request of both Reviewers, we have updated the figures and have assigned individual colours to each of the stations (e.g., see Figures R1-3 above). This change should help the reader differentiate between the different stations. However, we would prefer not to include separate panels for the LCIS stations in all our figures as this will make them very large and potentially unwieldy (e.g., Figure 3 would have 12 panels). That said, if the Editor feels that the LCIS data should be presented separately, we are happy to include such figures in the Supplemental Information.

Figure 3 d-f: The x axis range could be reduced (in particular for Si data). This would allow to see differences in profiles for the locations.
Response: We have updated the x-axis ranges for all panels in Figure 3 (please see Figure R2 above), following the Reviewer's suggestion.

Figure 11: It would be helpful (for comparison with the tables) to report the station names(L1...L10) in one of the panels. Further, it is not possible to distinguish the different symbols in the profiles in panel d.
Response: The figure has been updated based on Reviewer 2's suggestion as follows:

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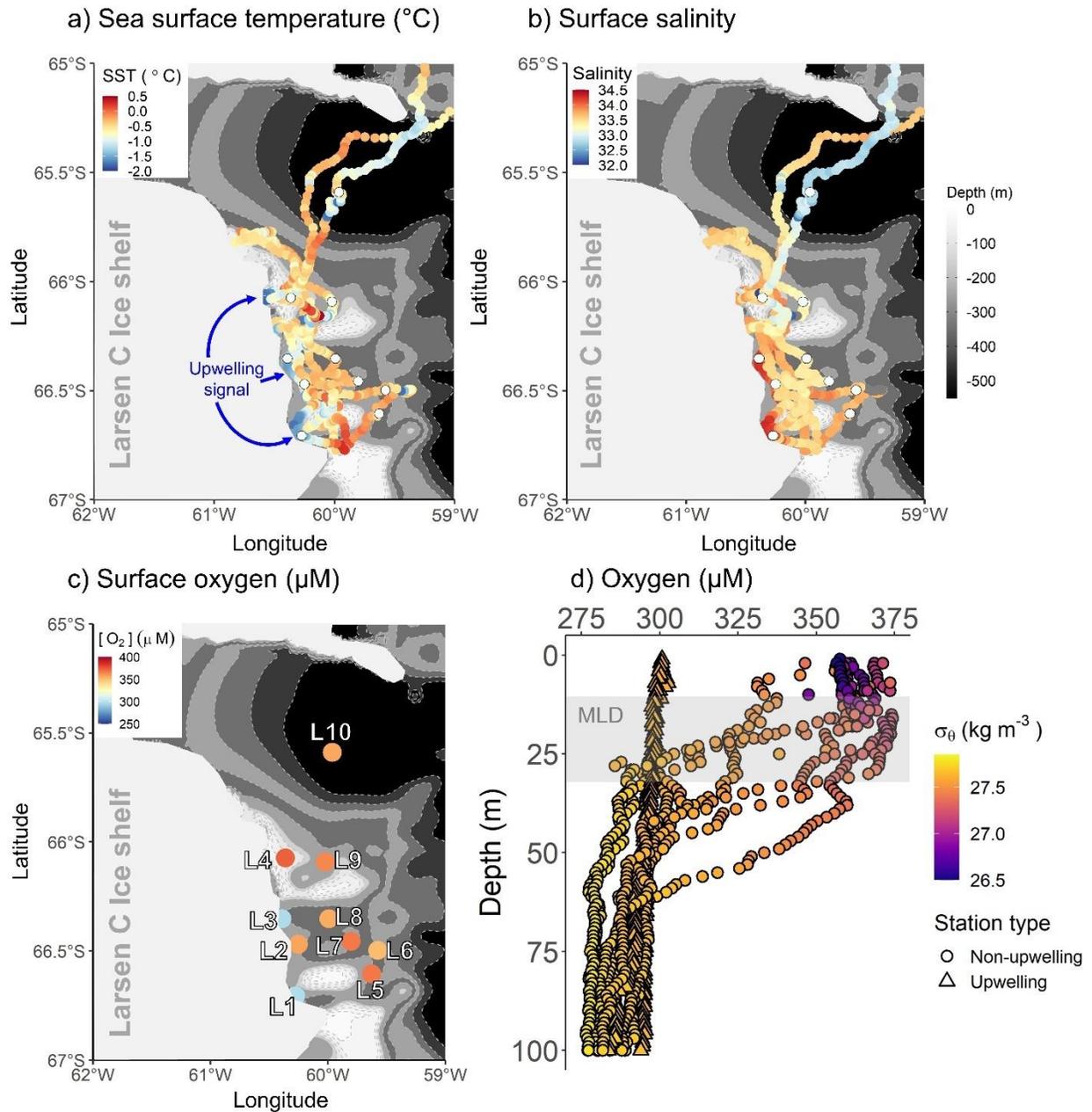
Figure R10 *(Figure S5 in the Supplemental Information): Maps of (a) SST, (b) sea surface salinity, (c) surface oxygen concentrations, and (d) depth profiles of oxygen concentrations in the region of LCIS at the time of sampling. SST and salinity data were acquired from the underway (~7 m inflow) ferrybox, while the oxygen concentrations were measured via the oxygen sensor on the CTD profiler, calibrated against discrete seawater samples measured for dissolved oxygen by Winkler titration (Carpenter 1965; Grasshoff et al. 1983; Hutchinson et al. 2020). The symbols in panel (d) are coloured by potential density (σ_θ), with the circles indicating the non-upwelling stations and the triangles showing the upwelling stations. The grey shaded region in panel (d) indicates the range of mixed layer depths (MLD) derived for the LCIS stations at the time of sampling. The bathymetry data in panels (a-c) were taken from ETOPO1 (NOAA National Geophysical Data Center 2009).*

Table 1 and 2: please specify in the header or legend that all ratios are molar ratios. Is it possible to report % sea-ice cover as well as water depth in either of these tables?

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Response: The headers of Table 1 and 2 have been updated to state that all ratios (except the f-ratio) are molar ratios. In addition, the bottom depth at each station has been listed in Table 1. Unfortunately, due to the close proximity of the stations to each other, we were unable to get high enough sea-ice concentration data to accurately report the sea-ice concentration at each station during our sampling.

Please also note the supplement to this comment: https://bg.copernicus.org/preprints/bg-2021-122/bg-2021-122-RC2-supplement.pdf

Response: Please see our response in the supplement (i.e., the amended pdf version of the manuscript), directly follows each of the Reviewer's comments.