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.

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.

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

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.

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<u>Specific comments:</u>

Abstract

<u>Line 31</u>: "*with the highest <u>potential</u> export fluxes*". The authors only measured potential export fluxes, not direct export fluxes.

Introduction

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

<u>Line 78</u>: 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

<u>Line 80:</u> "*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.

<u>Line 81</u>: "*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.

<u>Line 82-83</u>: "*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 a possible 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 relationship between primary production and export efficiency in the Southern Ocean?, Geophys. Res. Lett., 43, 4457–4466, doi:10.1002/2016GL068480.

Line 91: "the biological carbon pump"

Line 94: "biological carbon pump"

Line 97: "biological carbon pump"

Line 113: I would also add that grazing can also play a strong role in controlling the amplitude and terminating these phytoplankton blooms.

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*

Methods

Figure 1: can the authors increase the resolution of the Figure 1?
Line 170: please correct "Filchner-Ronne Ice Shelf (FRIS)"
Line 172: please indicate what date does the sea ice concentration map correspond to.

Line 180: "*Chlorophyll a fluorescence*"

Section 2.2.2. Estimating nutrient depletion
I think that there are problems with the equations presented between lines 231 and 254. If I work 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?

Results

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

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.

Line 399: "*depth profiles (0-150 m)*"

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.

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.

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.

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.

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 ρNO3. 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 sea ice 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.

Line 643-650: I have a similar interpretation of the relationship between ρNH4 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 ρNH4 and ρurea 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.

<u>Line 672</u>: Oxygen is also typically saturated as a result of phytoplankton and ice algae production.

<u>Line 673</u>: 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*."

<u>Line 687-89</u>: I very much agree with the authors interpretation here. In addition, it might be worth to add that higher SST promote phytoplankton productivity.

<u>Line 692</u>: "*and <u>potential</u> organic carbon export*".

<u>Line 699:701</u>: there are some problems with the references here.

<u>Line 703</u>: does the grey box in Figure 11d indicate the average MLD plus or minus the standard error?

<u>Line 731</u>: a word seems to be missing from this sentence: "*due to our <u>results(?)</u> not accounting for regenerated N uptake.*"

<u>Line 770</u>: "biological <u>carbon</u> pump"

<u>Line 852</u>: "*the absolute carbon export flux <u>potential</u>*"

<u>Line 898-901</u>: "*carbon export <u>potential</u>*". Here, while this hypothesis may be true, the authors might also want to acknowledge that Biological carbon export is usually stronger later in the season in the Southern Ocean.

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 authors for 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 et al., 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.

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 become part 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.

Line 941: "*potential carbon export*"