<u>**Author Comment 2 - Responses to Reviewer 2**</u>

"Exploring the relationship between sea-ice and primary production in the Weddell Gyre using satellite and Argo-float data"

We are grateful to the two reviewers for their positive responses and constructive suggestions and comments. In the following, reviewers' comments are in regular typeface *and our responses are in blue italics.*

Douglas et al primarily investigates interannual fluctuations in satellite-derived NPP as they relate to sea ice variability in the Weddell Sea. The main result is that annual NPP and annual maximum ice-free area are correlated at interannual timescales. They also contrast the shelf and open ocean regions. For example, they show that in the open ocean, an increase in satellite visible days corresponds to an increase in annual NPP up to a certain point only. This presumably reflects a shift from light to nutrient limitation over the course of the growing season. I think this paper will be a useful contribution to the community, but some points need clarification before the paper is suitable for publication. In light of this, I'm suggesting major revisions. My general and detailed comments are provided below.

<u>**General comments:**</u>

1. First, I find the usage of gyre confusing in the context of this manuscript. First off, the boundaries of the study region are hydrographic transects that have nothing to do with the actual gyre dynamics. Second, the gyre is typically not thought to extend all the way onto the continental shelf, e.g. see map of mean dynamic ocean topography in Fig. 5a of Armitage et al. (2018). So the division into open ocean and shelf regions seems to apply to the Weddell Sea rather than the Weddell Gyre. I would consider replacing "Weddell Gyre" with "Weddell Sea" in the title and throughout most of the manuscript.

We have changed Weddell Gyre to Weddell Sea in the introduction in the contexts of deep-water formation and when citing work by Arrigo et al., 2008 that referred to the Weddell basin/sector as the Weddell Sea. However in the context of our study region we have continued to refer to the Weddell Gyre. The hydrographic transects, used during ANDREX cruises, have been shown to broadly align with the boundaries of the gyre (Akhoudas et al., 2021, Brown et al., 2014). In Armitage et al., 2018, they show that the Weddell Sea refers to only the western area of our study region and that the gyre extends to ~30 deg E, where our eastern boundary is. For consistency with the terminology used in previous papers that use the hydrographic transects as their study area boundaries (Brown et al., 2015, Jullion et al., 2014, MacGilchrist et al., 2019), we use Weddell Gyre.

2. Second, since satellites cannot see through sea ice, it seems inevitable that the annual NPP over the entire region (as derived from satellites) will necessarily be higher when there's greater icefree area simply because you have the ability to detect the NPP? For example, the ice-free area is correlated with the total annual NPP (Fig. 2b) but not with the area-normalized annual NPP (Fig. 2a). So isn't this suggesting that the greater annual NPP is simply due to there being more ice-free pixels (with non-zero NPP)?

A larger IFA may not always result in greater annual NPP, for instance in an area that is not light-limited and instead primarily controlled by other factors. Instead, the correlation indicates that a larger area over which light limitation from sea-ice cover is alleviated results in more NPP. As the reviewer stated below, the majority of phytoplankton biomass (Chl-a and POC) is seen by floats after the waters are ice-free, supporting the assumptions in place because of the limitations of the ocean-colour satellite that assume no under-ice NPP. Secondly, although the relationship between summer IFA and annual NPP is significant, it does not account for all of the variance in NPP. We have emphasised this and discussed drivers of unexplained variance in the discussion.

A critic might argue that if there were significant under-ice NPP that is undetectable by satellite, the correlation between total annual NPP and ice-free area is an artifact related to the limitations of the satellite data. I actually don't think this is the case, since the floats show that a very small percentage of annual NPP occurs under the ice. But I think this point should still be addressed explicitly, and furthermore, this could help better integrate the float data analysis into the rest of the paper (i.e. if you frame the float analysis as a response to this imagined critic).
In general, in the current version of the manuscript, the float data feels unnecessary to the main results of the paper.
The sentiment here, concerning better integration of the float data in the manuscript, is shared by both reviewers. A detailed description of the changes we have made can be found in response to Main Comment #1 by the first reviewer.

To summarise: The floats are an important component of this work for two reasons: 1) They allow us to assess the uncertainty in the satellite data (namely in quantifying what the satellite misses due to sea-ice cover and low solar angle). 2) They allow us to observe the seasonal progression in Chl-a and calculate a quantitative timeline of activity from ice melt through the growing season, providing a complementary perspective to that gained from the satellite data.

As an overview of the changes we have made:
1) *The abstract has also been modified to integrate the float results more explicitly.*
2) *A sentence explaining the inclusion of float data has been added to the beginning of the autonomous floats methods section.*
3) *The uncertainties section has been modified to improve flow and clarity.*
4) *A paragraph has been added to the satellite results section to emphasise uncertainties in the data and lead into the floats results, emphasising the importance of the addition of float data to this paper.*
5) *The importance of the float data in supporting and expanding on the satellite-based results are also emphasised in the discussion*

More details of how we have justified the inclusion of float data is provided in the response to reviewer 1.

3. Regarding the float data, I also think you need to more explicitly mention the differences between chlorophyll and NPP since it feels like they're used interchangeably at many places in

the manuscript. I'm also wondering why you didn't use the POC estimates derived from the float backscatter? Backscatter-based POC is a somewhat better indicator of biomass than chlorophyll and perhaps more comparable to NPP than chlorophyll.
We chose to look at Chl-a from the floats because we could compare it to the satellite Chl-a observations. We have now made it clearer in the text that Chl-a is used here as a proxy for growth, and that it does not equate to estimates of NPP. In our description of the CAFE model, we state that Chl-a is used in the model to calculate NPP. Additionally, at the start of the "Autonomous Floats" methods section, we state that we are "using Chl-a as a proxy for growth". We have also now added analysis of particulate organic carbon (discussion of results lines 427-447) (estimated from backscatter float data) as an estimate/proxy for biomass. We have calculated Chl-a:POC ratios to improve our interpretation of the seasonal progression in phytoplankton activity.

Detailed comments:
Title: This title doesn't convey any of the actual results of the paper, so consider changing.
Due to the changes recommended in the body of the manuscript that we have undertaken, we feel that the title is now more fully representative of the contents of the paper.
We have changed "primary production" to "phytoplankton growth" to better encompass the use of Chl-a and POC

Lines 10-12: "…additional factors such as nutrient availability or top-down controls limit NPP."
"additional factors such as nutrient availability or top-down controls (e.g. grazing) could be limiting NPP" added

Lines 30-39: These sentences feel repetitive and are a bit hard to follow in terms of the actual writing. Consider condensing/rephrasing for clarity.
We appreciate the request for further clarity. We have rearranged this section, which now reads:

> *Climate models from Coupled Model Intercomparison Project Phase 5 and 6 (CMIP5 and CMIP6) project a decline in Antarctic sea-ice area and concentration as a response to anthropogenic climate change (Casagrande et al., 2023). However, low confidence in projections, due to the complexity of ocean-ice-atmosphere systems, means that exact estimates of decline are uncertain (Casagrande et al., 2023; Meredith et al., 2019). Therefore, in light of these observed and anticipated changes in the climate of the SIZ (Kumar et al., 2021; Ludescher et al., 2019; Casagrande et al., 2023), the need for a deeper understanding of the relationship between sea ice and NPP is pressing, as changes in sea ice will have concomitant impacts on carbon uptake and ecosystem health. However, the crucial gaps in our understanding of the drivers of NPP in the SIZ mean that large uncertainty remains about the nature and extent of these changes (Campbell et al., 2019; Henley et al., 2020; Kim and Kim, 2021; Pinkerton et al., 2021; Séférian et al., 2020; Henson et al., 2022).*

Line 40: I would say “The Weddell Sea…” rather than “The Weddell Gyre…” I think this applies to most of the manuscript (except for some other places in the Introduction that are explicitly related to the actual gyre), but I will not continue to point it out.

We have changed this instance of Weddell Gyre to Weddell Sea to better reflect the area where deep/bottom water formation occurs. We follow our response to the general comment above with regards to the use of Weddell Gyre through the remainder of the manuscript.

Figure 1: The Southern Boundary is hard to see on this map and I don’t think it’s referenced anywhere in the manuscript. Add contours showing the average annual maximum and minimum sea ice extent? There are no maps showing sea ice concentration so it’s hard for the reader to visualize.

Lines to indicate the maximum and minimum sea ice extent have been added, the shelf region line was changed to orange and the open ocean to dark blue and the Southern Boundary line was removed. The figure now appears as:

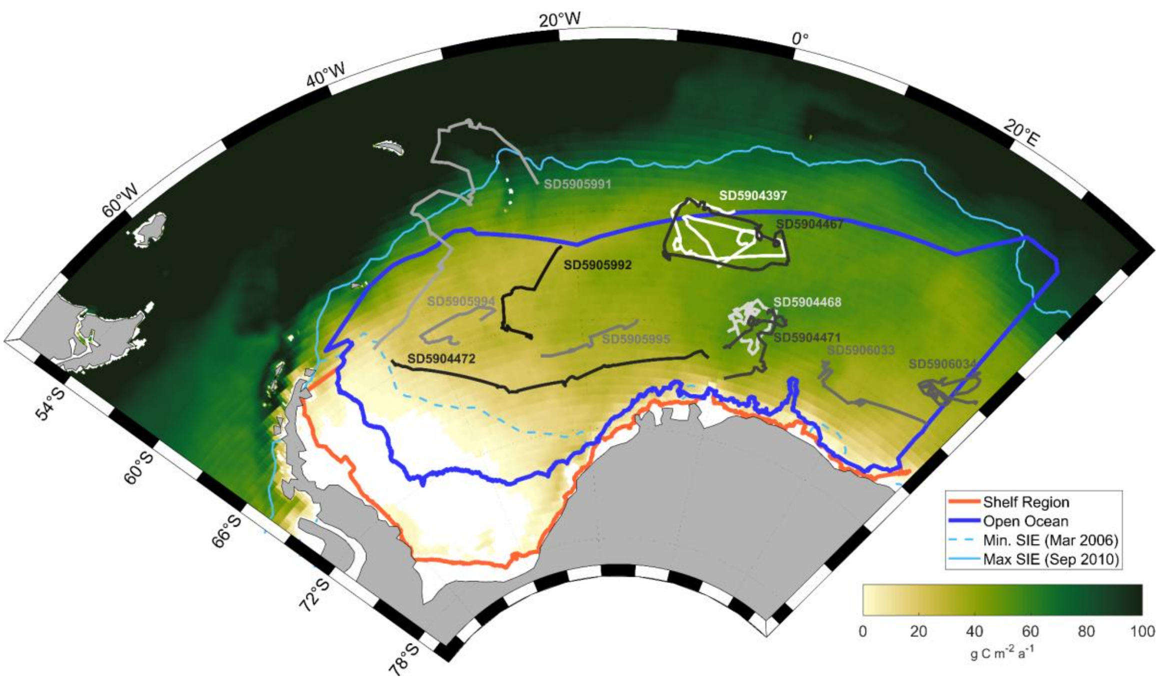

Fig 1: Location of the Weddell Sea and study subregions (dark blue: open ocean; orange: shelf). The 18-year mean area-normalised annual NPP (g C m^2) climatology derived from MODIS-Aqua satellite measurements using the Carbon, Absorption and Fluorescence Euphotic-resolving (CAFE) model (Silsbe et al., 2016) is represented by the yellow to green colourmap. White areas represent no data (permanent sea-ice present). SOCCOM float trajectories from 12 BGC-Argo floats are shown in greyscale and labelled with WMO ID. Profiles from float SD5905991 located north of the study region were not included in the analysis. The dashed and solid light blue lines denote the summer minimum (March 2006) and winter maximum (September 2010) sea ice extent respectively.

Line 64: Missing the closing parenthesis that starts at Line 62.
) added

Lines 116-119: You haven't properly introduced the relationship of chlorophyll and NPP so this subsection feels out of place as written. Also, as I stated in my general comments, why not look at the POC derived from backscatter?
We initially chose to use float Chl-a because, while Chl-a does not directly equal NPP, satellite Chl-a is used in all four of the open-source satellite NPP products, and as such we can compare float and satellite Chl-a products.

- *We have added a more detailed description of the CAFE model in the "NPP and Chlorophyll-a" section of the methods, stating that NPP is derived from Chl-a (among other variables) and that:*

  > *Cloud-filled MODIS-Aqua Chl-a concentration data were also obtained at the same resolution as the NPP data to later compare to BGC-Argo float data as proxies for growth.*

- *In the Autonomous Floats methods section, we now start the section with:*

  > *We use BGC-Argo float data to evaluate the data recovery attributes of satellite data, estimate associated uncertainties (Section 2.4), and also to assess the seasonal progression of phytoplankton growth in the water column, using Chl-a as a proxy for photosynthetic potential and particulate organic carbon (POC) as a proxy for biomass.*

- *Analysis of POC data (derived from float backscatter) has now been added to this study. The methods are described in the Autonomous Floats methods section:*

  > *POC concentrations were estimated from optical backscattering data after the removal of spikes due to large particles following Briggs et al., 2011. "De-spiked" backscattering were averaged in 10 m bins in the upper 50 m and then at 50 m intervals to 200 m. As with the Chl-a data, missing surface/shallow backscatter values were extrapolated (nearest neighbor) from the shallowest data available for each profile. Backscatter was converted to POC concentrations using the conversion co-efficient 3.12×10^4 as proposed in Johnson et al., 2017. The mean and depth-integrated POC in the 0-20m and 0-200m bins are reported here.*

- *Interpretation of these results have been added to the discussion to support the interpretation that increases in Chl-a indicate occurrent of primary production:*

  > *Our hypothesis of iron limitation at the end of the growing season is supported by the sub-surface Chl-a and POC observed by floats (Appendix Figures A1 and A2). Changes in Chl-a concentrations can arise from several situations aside*

from growth/accumulation of biomass: photo-acclimation, nutrient limitation and changes in phytoplankton community composition (Thomalla et al., 2017). Comparing Chl-a to POC, we can assess what may be causing changes in Chl-a. The presence of elevated Chl-a concentrations close to or below the base of the mixed layer, often (but not always) after the cessation of the initial surface bloom (Appendix Figure A1), suggests that phytoplankton are benefiting from replenishment of nutrients from below the mixed layer through diapycnal mixing (Arrigo et al., 2015; Taylor et al., 2013). Elevated POC signals coincide with increased Chl-a in the majority of these cases, providing evidence that active production is taking place at depth (Appendix Fig. A2). Surface nutrient concentrations are thus likely to be limiting phytoplankton growth in many areas of the ice-free Weddell gyre, although float data do not allow us to quantify its net impact on NPP. Grazing pressures may also be important in driving the differences in surface and sub-surface phytoplankton dynamics (Baldry et al., 2020, also see Section 4.2.3).

The complexity of the relationship between light and nutrient limitations – and their implications for inter-annual variability in annual NPP – is highlighted by the occasional occurrence of a secondary (temporally separated) late-summer bloom (Appendix Figure A1. e.g. panels a) 5904397: 2018, 2019; b) 5904467: 2018; c) 5904468: 2018, 2019; d) 5904471: 2018; g) 5905992 2020). As seen in the matching Chl-a and POC signals at depth, the second peaks in surface and depth integrated Chl-a that suggests a late-summer bloom are matched by simultaneous POC increases at these times, implying active growth within the phytoplankton community (Appendix Fig. A2). There are four float years (Appendix Fig. A2 h) 5905994: 2020; j) 5906033: 2020; and k) 5906034: 2020, 2021) that saw small increases in Chl-a at the end of the ice-free season without a concurrent increase in POC. We conclude that the increase in Chl-a in these cases may be a result of phytoplankton photoacclimating to the decreasing light conditions.

Lines 125-127: It's worth mentioning that float timeseries reflect both temporal and spatial variability. The language here implies that the floats can be treated as the timeseries of a bloom at a particular location, but this may or may not be the case given the small decorrelation length scales for chlorophyll.

We have added this caveat to the Uncertainties section:

Floats data have their own limitations - floats are Lagrangian autonomous observing platforms, so observations reflect both temporal and spatial variability. Additionally, sensor calibrations may vary and sensors sometimes drift towards the end of the float deployment. We did not attempt to estimate water-column integrated NPP from float data, as the floats in the study region lacked PAR (Photosynthetically Active Radiation) sensors, and, as far as we are aware, there are not yet methods for calculating NPP from float data that have been robustly validated for widespread use.

Lines 150-152: I don't understand this statement, which input data have less extensive spatial coverage than Chl-a?

Reviewer 1 also commented on the input data biases. A more detailed response can be found in the response to reviewer 1. In relation to this specific section, we have modified text to say:

> *In addition to this, there is also a disparity in the spatial coverage of the NPP products and the Chl-a input data used to derive CAFE NPP (Table 1). Some of the input data (absorption due to gelbstoff and detritus, absorption due to phytoplankton and backscatter spectral slope parameter) used in the CAFE algorithm to derive NPP have less extensive spatial coverage than the Chl-a input data. This means that there are some areas in the NPP product that imply there is no NPP occurring despite Chl-a being observed by the satellite.*

Lines 154-156: This seems like a limitation to the partitioning of total NPP on the shelf vs open ocean, which is framed as one of the main results of the paper. Obviously there's not much that can be done to address this, but it feels like it should at least be discussed later on in the paper.

Despite this limitation, when NPP data gaps are imputed using regional timepoint tendencies, we find that our result indicating the dominance of the open ocean to Weddell Gyre NPP still stands (93-96% contribution). The imputed values are reported in the text.

Lines 170-173: You should mention explicitly that the area of the open ocean is significantly larger than the shelf, which seems to be dominating the partitioning of the total annual NPP between the two regions.

This is mentioned in the discussion, but we have added the following sentence here as well:

> *The open ocean also has a far greater area than the shelf region (50.32 x 10^5 km^2 compared to 8.81 x 10^5 km^2, such that the open ocean represents 85% of the Weddell study region).*

Line 172: The abstract says 95%, but here it says 99%.

Thank you for picking up on this, the value should have been 99%. Although, the main text and abstract now reflect the updated values (93-96%) following calculation of uncertainties.

L173: mention missing data bias

Following the comments in the 'general' section, we calculated an estimate of NPP had the areas with data unavailable experienced the mean rates of NPP. These results have been included after the first paragraph of this results section:

> *Total annual NPP integrated over the entire Weddell Gyre between 2003 and 2020 averaged (± standard deviation) 172±34 Tg C a^{-1} before gap-filling and 269±39 Tg C a^{-1} after gap-filling (adjusting for the missed IFA; see Section. \ref{sec:uncertainty}). Annual area-normalised production was on average 97±8 g C m^{-2} a^{-1}. While the open ocean experiences lower daily rates of productivity*

compared to the shelf region (376±33 mg C m^{-2} d^{-1} compared to 582±99 mg C m^{-2} d^{-1}; Figure 3.a), annual NPP is in fact higher per unit area in the open ocean than in the shelf region (97±8 mg C m^{-2} a^{-1}, 68±23 mg C m^{-2} a^{-1} respectively; Fig. 3.b). This is due to a longer mean visible ice-free season: The sea-ice product shows that areas in the outer North-East edge of the open ocean are at the outer extent of the SIZ and can be ice-free for entire years, while on average, the whole open ocean region is ice-free for 139±13 days per year. The longest any of the shelf region is ice-free is 157 days, while the mean is 37±13 days.
The open ocean also has a far greater area than the shelf region (50.32 x 10^5 km^2 compared to 8.81 x 10^5 km^2, such that the open ocean represents 85% of the Weddell study region). As a result, when integrated over time and area, the open ocean accounts for a significant majority of the total carbon taken up by phytoplankton in the Weddell Gyre and dominates the inter-annual variability of NPP seen in the region (Fig. 3.b and c). Before imputation, the total annual NPP in the open ocean is 170±33 Tg C compared to 2±2 Tg C in the shelf region (such that the open ocean accounts for 99±1\% of the total NPP in the Weddell Gyre). After imputation, annual NPP rises to 255±38 Tg C a^{-1} in the open ocean and 11±5 Tg C a^{-1} in the shelf region. Despite seeing a large increase in shelf estimates following the use of the gap-filling approach, the open ocean still accounts for 96%±2% of the imputed Weddell NPP (ranging between 93-96% depending on the NPP model chosen).

Lines 186-188: Consider discussing some of the relevant forcings that drive interannual variability of NPP? This entire subsection is very descriptive, and you don't really discuss any of the mechanisms at play. I realize that you go into depth on the drivers in the Discussion section, but at least a sentence or two mentioning some of the controls on NPP might help the reader.
We have added the following to provide context for the reader and point to the discussion:

Potential causes of variability are multiple, including ice-free area, ice-free days, timing of ice retreat, cloudiness, wind speed and direction, sea surface temperature, vertical nutrient supply, glacial contribution. We investigate a number of these in the discussion below.

Lines 190-191: As I said above, some discussion of the mechanisms feels absent. Why might NPP on the shelf be declining? Speculation is fine, but I think some mention of the underlying dynamics is helpful. Otherwise the reader is left wondering whether this trend is just due to aliasing associated with the limitations of the satellite data on the shelf.
Thank you for the request for more investigation and speculation on this trend. Following this and comments from the other reviewer, we had a closer examination of the spatial coverage of the satellite NPP products and found that there is a significant decline in the spatial coverage of the CAFE NPP product compared to the IFA reported. This implies a degradation in the CAFE data. The trend is not reflected in the Chl-a data used in the CAFE model, and so we can assume that it is a decline in the coverage of the absorption and backscatter variables (as elaborated on in the uncertainties section). Despite this, we continue to use the results from the

CAFE model since it is the most comprehensive open-source model available and has been shown to best match the Southern Ocean in Silsbe et al., 2016. However, to support the robustness of our main conclusions, we repeat several components of our analysis with other NPP products (VGPM, VGPM-Eppley and CbPM; added to the supplementary figures) to support the CAFE results.

The results paragraph describing the trends in shelf NPP has been adapted to include interpretation of the gap-filled data and other NPP models (below). A weak, but still statistically significant trend is seen in all gap-filled shelf NPP data, but this trend is not seen when the first year/data point is removed.

> *In contrast, a trend in NPP is seen in the shelf region. In the CAFE model, imputed NPP declined by 3% per year,* $p=0.02$ *(Fig. 3.b). A similar rate of decline, although less statistically significant, is seen in the other NPP models. The directly-observed CAFE estimates of NPP decreased more rapidly (average decrease of 7% per year,* $p=0.001$*), underscoring the large influence of missing NPP data in the shelf region. Westberry et al.,2023 describe other potential causes for trends seen in NPP products (e.g. physiological changes in phytoplankton and decoupling of Chl-a and NPP), and emphasise the difficulty in identifying trends in NPP data and inferring drivers of trends. The trends seen here are sensitive to the occurrence of extremes in the early part of the time-period when there was a collapse of the Larsen B ice shelf along the Antarctic Peninsula (Peck et al., 2010). No trend is seen in the shelf NPP when the first data point (year 2003) is removed.*

Lines 203-204: Is this the yearly maximum IFA over the entire region or over the sub-regions separately? Because the area of the open ocean is so much larger than the area of the shelf, so the yearly maximum IFA over the entire region will be dominated by changes in the open ocean. In other words, if you're considering the yearly maximum IFA over the whole region, this could lead to a smaller correlation with the NPP on the shelf (compared to if you used the yearly maximum IFA on the shelf). It just seems strange to me that sea ice would be less important on the shelf.
Thank you for your request for clarity, we have amended the sentence to clarify that it is the Shelf IFA vs Shelf NPP and Open ocean IFA vs Open ocean NPP. It now reads as:

> *In the Weddell Gyre and open ocean sub-region, 42% of the inter-annual variability in total annual NPP can be explained by variability in the summer maximum IFA in each region (*$p=0.002$*, Fig. 5.a and b). This relationship was strongest in the shelf region, with 55% of the variability in total NPP being explained by the yearly maximum IFA over the shelf (*$p<0.001$*, Fig. 5.c).*

Lines 229-232: I think more could be done to introduce the objectives of the float data analysis so that it feels better integrated with the rest of the paper.

First paragraph in the "Aligning satellite and subsurface perspectives" section has been changed to:

> *Satellite observations indicate that, in the open ocean, the strong positive correlation between visible days and NPP degrades after around 130 visible days, indicating that other processes (e.g. grazing, nutrient availability) potentially begin to limit NPP after waters have been ice-free for more than 4 months. However, as described in Section 2.4, the ocean-color satellite loses coverage in late summer, when the solar angle decreases below 20°. As a result, it is uncertain whether further NPP is occurring, and therefore, missed after this point. Assessment of float Chl-a and POC (as proxies for phytoplankton growth/biomass) can reduce this uncertainty by indicating whether phytoplankton are still present in the surface ocean and/or whether growth may still be occurring beyond the date when satellites lose visual coverage. Therefore we seek to address if significant growth is missed after loss of satellite coverage in late summer and whether the same relationship between ice-free days and phytoplankton growth is seen in the available float observations. Although these data come from drifting platforms, rather than fixed points, we can enquire how the seasonal cycles of Chl-a and POC unfold in each year, and specifically how they evolve relative to light availability. It is worth noting that these data all represent open ocean conditions as floats are not deployed in regions shallower than 2000m.*

Lines 236-237: Where does this definition of bloom end come from?
Hague & Vichi, 2021 used the time derivative/rate of change in Chl-a within their definition of growth initiation. Our definition was informed by this prior use of time derivatives to define bloom dynamics.We noted the plateauing nature of the cumulative increase in Chl-a, and aimed to define the timing of that plateau. Notably, the values of Chl-a observed by the floats varied considerably, and it was not possible to use an absolute value (such as 1 mg m^{-3}) as a threshold from blooms and bloom termination. The "bloom-end" term used here is a subjective definition in order to quantify the slowing/decline in growth/increase in Chl-a.

Lines 284-285: Larger areas of ice-free water also provide more space for satellites to detect NPP. As I said in my general comment, I think you should use the float data as evidence that there is not significant NPP occurring underneath the ice, so that you can rule out the possibility that the correlation between ice-free area and NPP is not simply due to the greater number of pixels with non-zero NPP since satellite can't see through the ice.
The discussion has been condensed and reworked to improve clarity and flow. The following has be written in response to this comment:

> *The float data showing that 2-23% of integrated Chl-a (and 7-30% of surface POC; Table. 1) is present potentially before sea-ice retreat suggests that our the satellite analysis may over-estimate somewhat the correlation between IFA and NPP. Recent studies (Bisson and Cael, 2021; Hague and Vichi, 2021; McClish and Bushinsky, 2023) have also reported the presence of considerable amounts of Chl-a under sea ice as well as highlighting the onset of growth prior to*

complete sea-ice retreat (Hague and Vichi, 2021; McClish 375 and Bushinsky, 2023). However, while our float observations also indicate that biomass tends to increase before complete ice retreat, our results still clearly show IFA as a major productivity driver. Strong phytoplankton growth follows ice melt (Fig. 7) and the majority of phytoplankton biomass is found in ice-free conditions (Figs. 2, A1 and Table 1). Similarly, in McClish and Bushinsky (2023), the break-up of sea ice initiates the increase in Chl-a and POC, highlighting the light limiting control of sea ice on phytoplankton growth.

Line 286: I know you cite it later on, but some discussion of Moreau et al. (2023) seems warranted in this paragraph.

A citation for Moreau et al., 2023 was added to the sentence:

These hotspots are thought to be set by comparatively high levels of nutrient supply (e.g. Vernet et al., 2019; Geibert et al., 2010; Arrigo et al., 2015; Moreau et al., 2023).

Later in the paragraph, we have added a sentence to supplement discussion on the physical drivers of variability in sea ice-cover:

Moreau et al. (2023) found that strong winds transport sea ice towards the shelf, potentially removing light limitation to the surface waters as a result.

Line 297: add “in review” for this reference and also link to the preprint in the References section at the end of the paper.

This paper has now been accepted, so citation and reference have been updated.

Lines 335-337: Can you elaborate on how float data show differences in type/composition?

In their review of the occurrence of SO sub-surface Chl-a maxima, Baldry et al., 2020 describe the dominance of diatoms in sub-surface Chl-a maxima and a shift/difference in phytoplankton community between the surface and sub-surface. We have carried out an assessment of float POC. While Chl-a and POC (and Chl:POC) do vary with depth, we acknowledge that we cannot say for sure whether these changes are due to community composition differences, or physiological changes due to photo-acclimation, or nutrient limitation. We have removed the statement and added a reference to Baldry et al., 2020 in relation to surface and subsurface differences.

Lines 378-380: I don’t understand this statement? Why would a region becoming permanently ice-free cause NPP to decrease? Are you suggesting that the sea ice is an important source of iron to the system? Or that freshwater fluxes associated with sea ice melt/refreeze are important in setting the stratification that favors growth? Give some possible mechanism because “by analogy to the permanently open ocean regions in the present-day Southern Ocean” is not very convincing since it’s not clear what regions you’re even referring to. There are many sources of variability besides just ice vs. no ice that lead to heterogeneity in NPP.

We acknowledge the weak statement and have added some sentences later to highlight the potential impact of warming on stratification and nutrient mixing in the future. This now reads:

In future warming conditions, increased stratification, combined with freshening from melting ice could act to cut off biological productivity by reducing the vertical nutrient supply (Bronselaer et al., 2020). This will be particularly apparent in the open ocean, given its greater distance from terrestrial micro-nutrient sources. Noh et al., 2023 recently showed that, within CMIP models, Chl-a in the Arctic declines as a result of reduced nutrient supply when regions become ice-free. Despite being based in the Arctic, and thus differing physically and ecologically from the SO, this result in Noh et al., 2023 could point to a less productive Weddell Gyre in the future, should any of it become permanently ice-free.

Lines 390-405: I found the Conclusions section to be a bit weak and I suggest rewriting. Some of the statements are well-known from existing literature (e.g. it is clear that sea-ice dynamics are important in driving NPP in this region), while other statements are speculative and don't stem from the actual analysis conducted (e.g. substantial spatial variability undoubtedly contributes to the variance in NPP…). As a result, the reader is left feeling uncertain about what contribution has been made by this study

This sentiment was shared by both reviewers. As such we have amended the conclusions section to better highlight the findings, implications of this study and suggest avenues for future research. It now reads:

This study used a complement of satellite-derived sea ice and NPP products as well as BGC-Argo float observations of Chl-a and POC as proxies for phytoplankton biomass to assess the basin-scale relationship between sea ice and phytoplankton growth. We find that sea ice is the primary control on Weddell Gyre NPP in areas that experience fewer than 70-130 ice-free days per year. Beyond ~ 130 ice-free days, float Chl-a and POC observations suggest that nutrients (likely iron) emerge as an important limit to growth, possibly co-limiting with top-down grazing control. We find that while the shelf region sustains higher instantaneous NPP during its ice-free window, the open ocean sustains 93-96\% of the annual NPP of the Weddell Gyre, due to its larger area and longer ice-free season. Furthermore, while sea ice is a primary driver of inter-annual variability in total annual NPP in the Weddell Gyre, nearly half of NPP variability is still unexplained, motivating further study. We found no long-term trends in the Weddell Gyre sea-ice extent or NPP during the study period. However, our results suggest that NPP will increase if sea-ice extent decreases in the future, at least until the Weddell Gyre is ice-free for longer than 130 days, at which point, controls other than sea ice may dominate. Finally, this work has highlighted the importance of using BGC-Argo float data to complement and corroborate satellite data analysis. The study highlights the need for development of quantitative float-based NPP measurements in the region, which would likely benefit from inclusion of PAR sensors on more floats.

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