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.

General comments:

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.

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 ice-free 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 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.

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.

Detailed comments:

Title: This title doesn't convey any of the actual results of the paper, so consider changing.

Lines 10-12: "…additional factors such as nutrient availability **<u>or top-down controls</u>** limit NPP."

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.

Line 40: I would say "The Weddell **<u>Sea</u>**…" 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.

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.

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

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?

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.

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

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.

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.

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

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.

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.

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.

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.

Lines 236-237: Where does this definition of bloom end come from?

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.

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

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

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

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.

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.

References

Armitage, T.W.K., R. Kwok, A.F. Thompson, & G. Cunningham (2018). Dynamic topography and sea level anomalies of the Southern Ocean: Variability and teleconnections. *Journal of Geophysical Research: Oceans*, **123**, 613-630.