Review of “Exploring the relationship between sea-ice and primary production in the Weddell Gyre using satellite and Argo-float data”

Douglas et al. (2023)

In this study, Douglas and coauthors use satellite-derived estimates of net primary production (NPP) and chlorophyll data from profiling floats to investigate the link between the seasonal evolution of biological activity in the Weddell Sea and sea-ice dynamics, as well as the ability of satellites to observe this high-latitude region. Over the observational period, the authors find a strong relationship between satellite visible days (reflecting both the absence of sea ice and a solar angle high enough to facilitate satellite observations) and annual NPP on the continental shelf, but not to the same extent in the open ocean, where the relationship weakens once a critical threshold of satellite visible days (~4 months) is reached. This reflects a strong light limitation of phytoplankton on the shelf throughout the ice-free/growing season, while light is no longer the strongest limiting factor in the open ocean towards the end of the season (likely switching to nutrient limitation, but other contributing factors such as grazing cannot be excluded based on the current study).

The paper is well-written overall, and the study is certainly **suitable for publication in *Ocean Science***. However, I see potential for improvement in the current version of the manuscript in the way chlorophyll and NPP data were integrated in the study and in the discussion of implications and shortcomings of the findings. **Overall, I therefore recommend major revisions before publication.** Addressing all major and minor points outlined in the following will hopefully substantially enhance the clarity of the manuscript.

Main comments:

1. In my view, in the current version of the manuscript, the authors do not make it clear enough for the reader what is gained from adding the analysis of float-derived chlorophyll data to the analysis of satellite-based NPP. The way I see it, almost all results could be obtained from using the satellite-derived estimates alone. This is reflected by the abstract of the paper, which mentions the floats in L. 5, but which does otherwise only summarize results of the analysis of satellite-based NPP. The authors should therefore re-consider the title of their paper and/or modify the content of the abstract (and the paper) to better integrate the analysis of the float data. I am not arguing to leave the float-based analysis out but think the paper would benefit from a better integration of this piece into the paper. Further, it should be made much clearer to the reader that the float-based chlorophyll estimates are not estimates of primary production (see e.g., title and L. 240). While their dynamics are certainly similar, their dynamics can be decoupled at times and in some places due to variations in community chlorophyll-to-carbon ratios and/or in loss terms. For example, in the context of this study, I am wondering if one could learn something (about biomass loss terms?) from the overall similar behavior of chlorophyll and NPP in relation to sea-ice dynamics.

2. What is lacking in the current version of the paper is a discussion of the results in the context of possible shortcomings of the underlying data, in particular data gaps in the satellite-derived estimates of NPP. One of the main findings of the authors is the overwhelming contribution of the open ocean to basin-wide estimates of NPP. Yet, at the same time, as the authors state themselves, NPP in highly productive coastal polynyas might be substantially underestimated (L. 152-156 of the submitted paper), leaving the reader wonder what the impact of this bias on all means and trends reported in the

paper might be. Given that chlorophyll appears to be detected for at least some of these polynyas (while NPP is set to zero in the CAFE model), can this information be used to somehow quantify an upper bound of NPP for the shelf region? Is the area affected on the shelves similar for all years or is there interannual variability? I realize that the main findings of the study will remain unaffected and that any attempt to quantify this bias might be associated with its own uncertainty, but the impact of this bias should at least be qualitatively discussed. Only mentioning this once in the method section is not enough in my view and might lead to a false certainty on quantitative estimates reported throughout the paper. I have highlighted several locations in my specific comments below, where I think the paper would benefit from such a discussion.

3. While the overall writing of the authors is clear and easy to follow, the writing could be improved further in several parts of the manuscript. In particular, the discussion section could be written more concisely, as it currently includes some redundancies with other sections of the paper. For example, it sometimes repeats content from the introduction, making it overall too loosely tied to the new findings of the manuscript. I think more clearly focusing on these new results and getting rid of too lengthy repetitions of the motivation of the study will make for a stronger discussion section. Similarly, a revised conclusion section should be less generic and should more clearly highlight the knowledge gain and implications from the study by Douglas and coauthors.

Specific comments:

L. 4: I suggest adding the time period of the analysis to the abstract.

L. 6-8: In my view, a more nuanced statement would be more accurate here given that the continental shelf region is likely affected more by e.g., data gaps, than the open ocean region (see also L. 155 of the manuscript). I realize that the open ocean will remain the dominant area whatever one would do to fill data gaps on the continental shelf but adding some upper/lower bounds (if possible) to the number (95%) or acknowledging the uncertainty of that number some other way would be helpful.

L. 12: From what I have understood in your paper, you have not ruled out grazing, have you? If true, I suggest adding "grazing" to the abstract for a more accurate representation of your discussion.

L. 18: Very minor comment: It is a bit uncommon to see a reference "*b*" before "*a*".

L. 27/30: When first reading this part, I read it as "Here is modeling studies that have looked at the relationship (REF1, REF2), but observational evidence is lacking". I realize that this is *not* what the authors have actually written but want to encourage the authors to rephrase this critical piece to make it clearer to the reader that it is *basin-scale* studies that have been missing until now. I further suggest splitting the cited references to more clearly indicate the ones that are modeling-based, float-based, satellite-based etc. Lastly, I am aware of three more references that might be relevant in this context (Briggs et al., 2017, Uchida et al., 2917, Arteaga et al., 2020; based on a non-exhaustive search).

L. 36/37: The part "will follow in the coming decades" of this sentence is a little unclear to me (will follow what exactly?) – can you rephrase to increase clarity?

L. 40: In the context of deep/bottom water formation, I would find “Weddell Sea” more accurate than “Weddell Gyre”, given that the formation also involves processes other than the Gyre itself.

L. 43: Nissen et al. (2022) could be added here.

L. 48/49: It would be good to be quantitative in your comparison between rates of NPP on the continental shelf/in polynyas and in the open ocean. Can you add numbers?

Fig. 1: Given the strong focus on the role of sea ice seasonality, the authors should consider adding lines for the max/min sea ice extent in the area. From what I could see, the Southern Boundary is not referred to at all in the text and could be deleted (to make room for contours of sea ice extent). Another fairly minor point: Personally, I found it confusing that the contour line denoting the continental shelf is in a color from the colorbar of the chlorophyll map in the background. Please consider changing one of the two to avoid confusion. Lastly, please add a citation to the CAFE model to the figure caption.

L. 62: closing “)” missing

L. 63: From what I understood, “its” should refer to the Gyre here. Grammatically, it refers to “water” I think – please double-check.

L. 64: This is where I first thought that adding sea ice contours to Fig. 1 might be helpful.

L. 83: I am not sure I understand the reasoning behind using parentheses here and would advise against them for section titles.

L. 87: I suggest specifying here what these input data are (not every reader will be familiar with the model).

L. 98: corresponds *to*

L. 99/100: It might be helpful here for the reader to state more explicitly why these don’t (necessarily) agree.

L. 105: I suggest moving this sentence further up to where this quantity is described. It feels out of place here.

L. 124: I suggest citing the paper by Klatt et al. (2007) here, who first suggested the use of such a criterion.

L. 127/128: Have you assessed whether the ice-free period determined this way from the floats corresponds to the ice-free period based on satellite observations in that same region? I would expect some mismatches based on the very different criteria used (see also Hague & Vichi 2021, who have used a revised sea-ice detection algorithm for floats, i.e., also including salinity).

L. 111-130: Just reading up to this section, I do not think it is sufficiently clear yet for the reader what is gained from looking at float-based chlorophyll data in addition to the satellite data. Based on the paper title and e.g., last paragraph of the introduction, the reader was set up for a study on the links between NPP and sea ice, so that the use of chlorophyll might come as a surprise here. In my view, the use of parentheses for chlorophyll in the section title does not help emphasizing why the use of float-based chlorophyll data is useful for the study question, so I suggest clarifying this.

L. 144: I assume that for this assessment, you used the temperature criterion to determine whether a float is under ice. If this is indeed what you did, please mention that here. But coming back to an earlier comment of mine: How sensitive is this to how you define the time “under sea ice”? Given that the temperature criterion is probably a conservative criterion, I would assume a rather small sensitivity. Nonetheless, it could make sense to check this here.

L. 145: Please also mention in the text (not only in the Table) that you’re assessing chlorophyll averaged over 0-20m here.

L. 152: I am not sure I fully understand: How can the smaller spatial coverage of the NPP product than of the chl-a product be due to “satellite observation limitations” if the latter is also a remote-sensing product? Can you clarify this?

L. 153: The number you give here is difficult to put into context. I suggest to also give the number relative to the whole Weddell Sea area in the text (not only in the Table).

L. 155/156: In relation to my comment on L. 152, could you elaborate on what field causes the polynyas to not show up in the NPP products and why?

L. 162: If your intention with Figure A2 was to highlight that satellite-based chlorophyll estimates often underestimate concentrations relative to float-based estimates, I suggest stating that more explicitly in the text. I do not find the current, rather generic statement of “showing similarities and dissimilarities” very helpful. Additionally, please increase the font sizes in this Figure.

L. 169: I would delete the “area-normalized” here for clarity. It took me a moment to realize that the contrast in this sentence was between *daily* and *annual*, not between *integrated* and *area-normalized*.

L. 170: In the text you say “mg C” but the y-axis label of Fig. 2 says “g C” – can you double-check which one is right?

L. 170: How large is the difference in the ice-free season? It might be of interest to the reader, so please consider adding this information.

L. 170-173: This is when I first wondered what the impact of the missing data on the shelf is. Given that you’re missing NPP in coastal polynyas (see your method section), this bias might be worth mentioning, even though it might not change that much on a basin-scale.

L. 172: In the abstract, it says 95%. Should it say 99%?

L. 174-178: For all numbers in this paragraph, I suggest adding the “% of total area” as it is rather difficult to put the numbers into context otherwise.

Fig. 2: I found it a little confusing to have a legend with “IFA” in panel a, even though this quantity is only shown in panel b. Maybe move the legend to outside of panel a, e.g., on top of or below the Figure? In panel b, is the maximum IFA shown for the whole Weddell Sea? Please clarify in the y-axis label and the Figure caption. Looking at the Figure and assuming this is indeed whole-Weddell Sea-IFA, I was wondering why this is not split into open ocean and shelf

as well (for consistency). Lastly, the way NPP is currently plotted, hardly anything can be seen for NPP on the shelf. I realize that this might have been your point, but for any reader interested in the evolution of NPP on the shelf, this would be more easily visible if plotted on two separate y-axes.

L. 190-194: What you write in this paragraph is very hard to see in the Figure. Also, unless I missed something, you do not currently show the temporal evolution of sea ice on the shelf anywhere, do you? Please see my comments on Fig. 2 above for suggestions. Additionally, given your statements in the method section that you might miss NPP in coastal polynyas, have you checked in the chlorophyll and sea ice data whether there are any trends in “possibly missing NPP in polynyas” over time? Can you assume this bias to be constant in time or would it modulate any trends in NPP on the shelf reported here? I think this is an important aspect to check and report to add confidence or uncertainty to the identified trends in NPP.

L. 196: I am not sure I understand what “time” you used in the regression here. Do you mean the total duration of open water area? Please clarify.

L. 197-199: For this statement to hold, it assumes that the maximum sea ice area is far less variable over the years than the maximum ice free aera, doesn’t it? I would assume this is true (and assume that you have checked this), but this could be explicitly stated for clarity.

L. 203-207: Again, the reader is left wondering what role the potential NPP underestimation due to missing NPP in (some) coastal polynyas plays for this finding. It would be helpful if you elaborated on this here (or in the discussion). Do you really think that sea ice is a less strong predictor of NPP in shelf seas or do you think that the caveats associated with the data used here complicate the comparison between the role of sea ice on the continental shelf and in the open ocean?

L. 208-213: I am not sure I find this paragraph particularly useful as it is. I found the per-pixel analysis in the subsequent paragraph much more insightful. I am wondering if the main message of this paragraph (“Over the whole open ocean, the area-normalized annual mean NPP is not correlated with mean IFA, sea-ice retreat or mean visible days, but it is on the shelf”) would not be better embedded into this subsequent paragraph to improve the flow. Unless I misunderstood something, it is the breakdown of the relationship in the open ocean beyond ~120 days that also causes/contributes to the absence of correlation on a basin scale. Do you agree?

L. 211: How does this statement fit to the finding in the previous paragraph, i.e., the low correlation between the maximum ice-free area and NPP?

L. 214: a wide range *was* recorded

L. 215: Whatever you decide to do about my suggestion of the paragraph in L. 208-213, I suggest moving the definition of “visible days” to where these are first mentioned in the result section.

Fig. 5: The y-axis label might be easier to read if given in %.

Fig. 6: In the caption, please describe panel a before panel b and use the panel labels in the caption (instead of “upper panel”, same goes for in the main text). Further, for panel a, please add what the whiskers, the orange line etc. represent (Median? Mean? Which percentiles?) in a

legend and in the caption. I further suggest finding clearer y-axis labels for the current panel a. For example, for “days to 50% 0-200m”, I suggest specifying that you’re referring to chlorophyll and that this is the bloom end. This should also become clear from the figure itself, not only from the caption and the text).

L. 231: I suggest rephrasing “after this much exposure”. Maybe “after waters have been ice-free for more than 4 months”? For me, “exposure” is too unclear.

L. 231: Can you specify in what way the use of floats *deepens* the analysis? It would be helpful for the reader if you stated explicitly what can be gained from additionally analyzing the float data. The current statement is rather vague.

L. 234: I suggest rephrasing “depth restrictions” here. This made me think of the limitation that floats only sample the top 2000m – which is not the shortcoming in the context of your analysis. I assume you’re referring to the fact that because they sample the top 2000m, they are not deployed in regions shallower than 2000m, i.e., the shelf regions. Can you clarify this in the text?

L. 235: If you’re referring to the bottom panel first, I suggest switching the order of the panels in the figure. It is always easier for the reader if you refer to panel a first.

L. 236: It might be helpful here to add “blue lines in Fig. 6a” [or panel b, depending on whether you decide to switch the order or not] and “green lines in Fig. 6a” to the text to guide the reader.

L. 237: Do you have a reference for this definition of the bloom end? This is different from the more typical bloom definition in the literature (see e.g., Siegel et al., 2002 or Soppa et al., 2016, Hague & Vicchi 2018), and I am wondering why you did not use a criterion that makes use of the complete annual information. Please clarify.

L. 240: Please be careful here not to equate chlorophyll concentrations, i.e., a proxy for carbon biomass concentrations, with primary production. Chl:C ratios might vary substantially across the growing season, both in response to a changing light and nutrient environment and changing community composition. Further, as a proxy for biomass, chlorophyll concentrations also integrate loss terms.

L. 242-247: Related to my comment above, I am left wondering how sensitive this assessment is to how you define the bloom end. Have you tested different bloom metrics?

L. 254: What do you consider “long”? As this is subjective to the reader, I suggest being more specific here.

L. 255-262: A lot of this reads more like information for the introduction, not the discussion. Please revise to make this a more concise discussion of your findings. In general, I would always argue for the “one idea/message per paragraph” structure. In section 4.1., I have trouble identifying one message per paragraph – to me, it is the same message in each (see also section title). Maybe combine into a single paragraph?

L. 263-266: As stated above, I think it is very important to at least mention shortcomings related to data coverage in the NPP data set here.

L. 269: Why “likely”?

L. 280: Are you referring to your own work here or somebody else’s with the comment in the parentheses? This is not clear to me, as I do not find this information anywhere in your result section.

L. 281: Weddell *Sea* instead of Weddell *Gyre*

L. 282: *the* instead of *a* dominant driver

L. 285: I find the formulation “provide more space” odd in this context. Can you rephrase?

L. 297: Please add “in review” or “submitted” after this reference.

L. 298: I suggest adding “for satellite detection” after “sufficient available light” to increase clarity here as this is what you show in Figure 5.

L. 305ff: Some of the references mentioned further down (L. 318/319) should be mentioned here already to point out that other authors have reported this transitioning of limiting factors in the high-latitude Southern Ocean.

L. 315-320: This feels repetitive with previous section. Consider deleting/shortening to reduce redundancies.

L. 330-335: While it is very likely that iron is indeed the limiting factor for growth, differences in grazing pressure might also play a role in explaining differences between surface and subsurface bloom dynamics of phytoplankton chlorophyll. I suggest slightly adapting the language here to represent more accurately what you can be sure about and what only appears likely.

L. 336: Do you mean phytoplankton community composition? If so, could you elaborate on how you conclude this from the chlorophyll data?

L. 355: What *warmer* areas of the Gyre are you referring to here? I suggest using geographic descriptors in this context.

L. 364: Grazer populations exert top-down control on phytoplankton communities whenever they are present, not only by late summer. I suggest rephrasing to “may dominantly control phytoplankton biomass/communities” or such.

L. 378: As stated by the authors in the abstract, this statement only holds as long as the other environmental variables do not change (nutrient availability, grazing). I suggest adding this information/assumption here.

L. 380-383: Please check for redundancy with first half of the paragraph.

L. 384: As you only infer this from your results and don’t actually show it, I suggest saying “imply” instead of “indicate”. I further suggest deleting “particularly”, as you only infer this for the open ocean and not at all for the shelf. These changes would reflect your findings more precisely in my opinion.

L. 385: Again: unless nutrient supply changes.

L. 391: The float data do not give estimates of NPP, please be precise. Additionally, since you do not only look at sea ice but also visible days, I suggest rephrasing to something that better synthesizes what you have done.

L. 392: “It is clear” – This makes it sound like it was clear already before your study. I do agree with this interpretation (there was a body of work demonstrating the link before this paper), but I am not sure this is what you actually refer to here.

L. 394: Please add the number here instead of saying “to a high degree”.

L. 397: I disagree with the authors that the float data demonstrate the iron limitation – it might seem plausible (and is probably true), but this has not been explicitly shown in this paper. Please elaborate on this or rephrase.

L. 398-405: I find a lot of these statements rather generic, and as such, they do not represent strong concluding sentences based on the results and discussion presented in this paper. I suggest re-working the conclusion section.

Figure A3: All font sizes are way too small.

Additional references

Arteaga, L. A., Boss, E., Behrenfeld, M. J., Westberry, T. K., & Sarmiento, J. L. (2020). Seasonal modulation of phytoplankton biomass in the Southern Ocean. *Nature Communications*, *11*(1), 5364. https://doi.org/10.1038/s41467-020-19157-2

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Hague, M., & Vichi, M. (2018). A Link Between CMIP5 Phytoplankton Phenology and Sea Ice in the Atlantic Southern Ocean. *Geophysical Research Letters*, *45*(13), 6566–6575. https://doi.org/10.1029/2018GL078061

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