Review 2:

Review of "Anomalous Summertime CO2 sink in the subpolar Southern Ocean promoted by early 2021 sea ice retreat" by Kirtana Naëck et al.

This is an interesting study, that reveals the importance of sea ice melt or iron to support large phytoplanktonic blooms in the Southern Ocean, even in areas where enrichment or iron from land aeolian transport or hydrothermal vents could have been suspected at first sight. In addition, the authors put the observed bloom in perspective with earlier ice retreat, a feature that is predicted to repeat more frequently in the future. I am on the same line as the authors and think this study is valuable and deserves publication. I have no doubts about that.

Thanks for your positive comment!

Still, I have a few concerns.

The first one does not necessarily need to be addressed. SO-CHIC project proposed a very original combination of Carioca and glider surveys. I'm wondering if more emphasis could have been put on the physical processes. Gliders provide very valuable information about the physical processes underneath the buoy. The concepts of MLD vs XLD have been rapidly addressed in the results, but their impact was not discussed in depth elsewhere.

The study from Pellichero et al., 2020, has demonstrated that a decrease in wind speed causes an increase in stratification, as shown by a shoaling of the MLD, leading to an accumulation of Chl-a at the surface and a decrease in DIC due to increased carbon fixation by phytoplankton. We suggest the same phenomenon for our study case, for the biological events between the 05/02/2022 – 07/02/2022 and between the 04/03/2022 - 07/03/2022. Comparison between MLD and XLD also enables us to identify mixing with waters from the subsurface. (Refer to lines 222-226 of the paper).

The NCP was estimated using the XLD. We have corrected line 125 of the paper: "… following the same methodology as in Merlivat et al. (2015) but using the mixing layer depth as in Merlivat et al. (2022) and Pellichero et al. (2020) (see section 2.3).

We have corrected the title of Section 3.2 by: "NCP and impact of wind on XLD". Moreover, the biological event was between the 5th and the 7th of February, we have corrected the dotted green line on Figure 4 accordingly. We have also corrected the legend of the figure and the dates at line 213.

We have added the XLD on the glider profiles of figure 3:

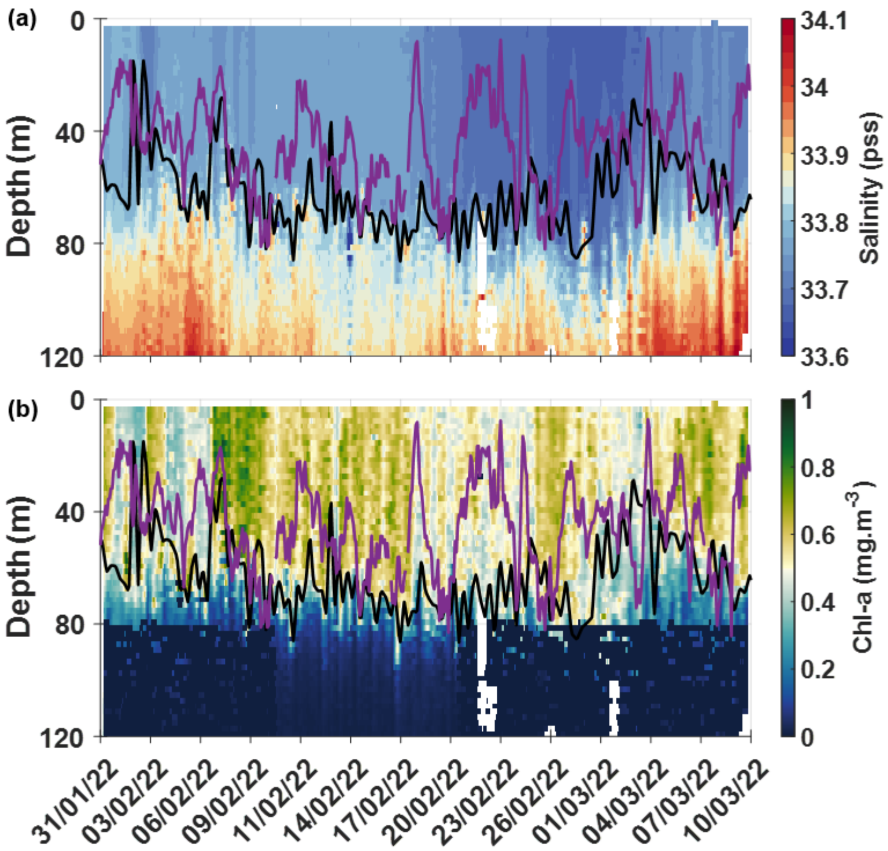


Figure 3: Seaglider profile time series between the 31 January and the 10 March 2022 of (a) salinity, (b) Chl-a, with the MLD indicated by the black line and the XLD indicated by the purple line.

I have the feeling that the authors are doing a bit of cherry-picking, presenting only a part of the data set till 27 June 2022 or some periods where primary production strongly affects the DIC signal (first two weeks of February and first two weeks of march). Personally, I am not encouraging a selective or partial interpretation of the data, but I can understand that you want to deliver one message (the impact on primary production, away from the SIZ) and then not discuss every single aspect.

We decided to present only part of the data set, to focus on summer and autumn 2022 and explain these few months in more detail. The rest of the CARIOCA time series is part of another future study altogether.

We chose to go into more detail for the first two weeks of February and the first two weeks of March, because these are the only periods during which NCP could be estimated. Indeed, the NCP could only be estimated during periods of low wind speed, where we could suppose that there was no mixing with the subsurface and where diurnal cycles of DIC and O_2-O_2 sat in opposition could be observed. (On the right-hand side panel of Figure 4 (a), we have removed the MLD as from the 10th of March 2022, since after that date the glider stopped following the CARIOCA.)

Concerning the rest of the CARIOCA time series:

- There was an undersaturation of about -10 to -15 µatm in winter 2022 (with periods during which the fCO_2 of the ocean was in equilibrium with the atmosphere). This is surprising since this region – the Polar Frontal Zone – is known to be at equilibrium with the atmosphere or should on the contrary show outgassing of CO_2 to the atmosphere (Gray et al. 2018). However, the region in which the CARIOCA was, has almost no winter data available. For the previous years, there were only a few SOCAT data in the winter season, between the Southern Boundary and SACCF, in the whole Southern Ocean, and even less in the Southern Indian Ocean.

Part of the undersaturation (about 5 µatm) observed by the CARIOCA can be explained by solubility changes, due to a negative SST anomaly in that zone in winter 2022. I am currently investigating further to explain the rest of this winter 2022 undersaturation.

- Then, the CARIOCA observed another CO_2 sink (smaller than the summer sink), from November 2022 to February 2023, as it drifted south of Kerguelen Island, in a bloom coming from the Northern Plateau. Further east, as from February 2023, the CARIOCA fCO_2 increased back to values close to equilibrium with the atmosphere. Since the undersaturation wasn't observed the next winter (May-June 2023), this leads us to believe that the CARIOCA's fCO_2 measurements weren't biased.

We have added the figure below, showing the whole CARIOCA time series, to the supplementary material of the paper (the figures have been arranged to follow the order in which they are mentioned in the text).

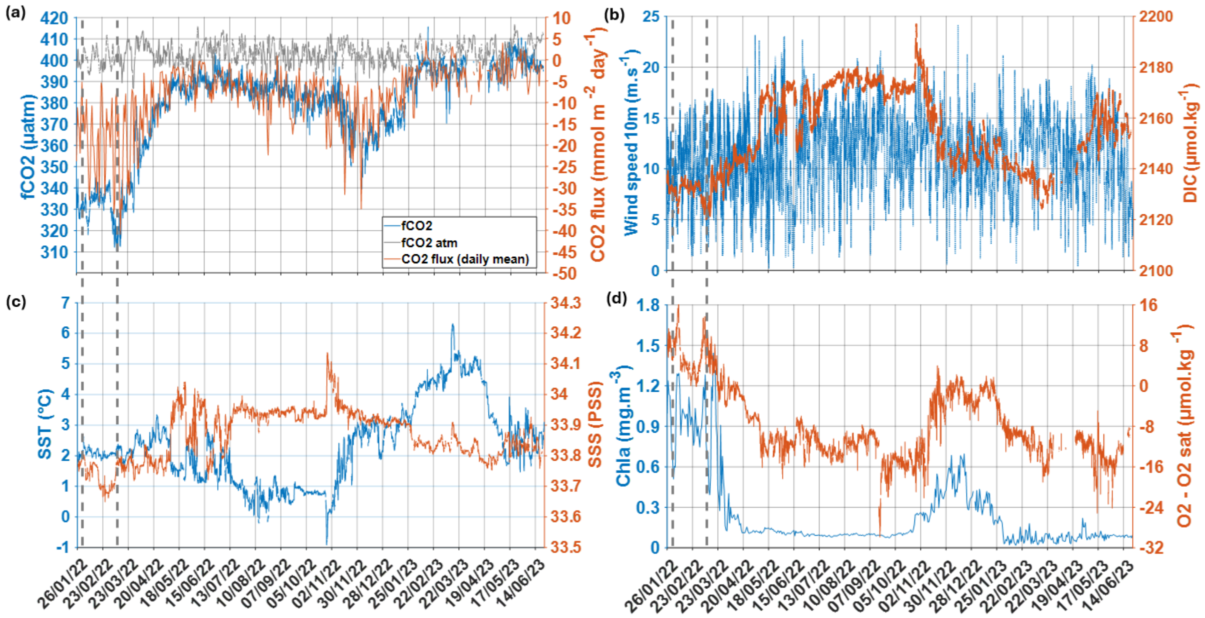


Figure S2 : CARIOCA time series from the 26 January 2022 to the 24 June 2023: (a) Atmospheric and surface ocean fCO_2 (fCO_{2atm} and fCO_2), and daily mean of CO_2 flux (b) DIC and wind speed, (c) SST and SSS, (d) Chl-a and O_2-O_{2sat}, with the period during which the glider followed the buoy (31 January to 10 March 2022) indicated by dotted lines. Since the CARIOCA observed a negative jump in the O_2 measurements on the 17/09/2022, there might be an underestimation of about 10 µmol kg^{-1} for O_2-O_{2sat}, as from that date.

But then you should refrain from writing some sentences like "This suggests that biological activity was the dominant driver of the DIC seasonal variation "line 229, while you are looking into detail only some few weeks, and then during these few weeks, the DIC was actually increasing overall, not decreasing.

The reviewer is correct, the principal driver of the low DIC values in February and March 2022 is not the local biological activity. Although, the DIC slightly decreases during some periods of high local biological activity and low wind speed (when we could estimate the NCP), other processes, such as mixing and air-sea exchanges, are compensating this DIC decrease. The DIC concentrations are overall very low during the whole summer 2022, and we are suggesting that these low values are due to the fresh water mass, already poor in carbon, which came from the ice edge. We have removed line 229.

At the end, the incredible opportunity and amount of energy needed to deploy the glider in conjunction with the buoy was not really necessary. Results from the glider are not addressed in the discussion or in the conclusion.

The glider data were crucial to estimate the depth of the mixed layer (using density profiles, Cf. line 143 of the paper), and to analyse/separate the different contributions (biological activity, air-sea exchanges, mixing) on the DIC variations. Using the wind data, the MLD and XLD, we were able to identify mixing events with waters from the subsurface, and this could not have been done without the glider data. We have insisted on the glider's importance in the discussion.

The glider also enabled us to get in-situ vertical profiles of temperature, salinity and fluorescence, which provide a more comprehensive view, which wouldn't have been possible with only surface data. The glider vertical profiles showed that both the low salinities and high Chl-a concentrations were constrained by the mixed layer (Cf. lines 198 - 200). Moreover, since the CARIOCA Chl-a couldn't be calibrated with the ship CTD during its deployment (not enough points coinciding spatio-temporally), the glider was used to calibrate the CARIOCA Chl-a (and convert the arbitrary fluorescence units to Chl-a concentrations, cf. my comment below for line 141). This was referred to in the section 2.3 of the Materials and Methods, in the paper.

Second, there are more than 80 plots in the paper in three different places (in the text, in the 6 parts of the appendix and in the supplemental material). It's really a massive amount of information, and while it's a rigorous approach, to be honest, it confused me at some point. There is repeatedly the same information with different products (e.g. Figure B2) or the same data appears in different graphs (DIC changes and O2-O2 sat appear in figure 2, 4 and A1). The figures are not appearing in the same order that in the text (e.g. line 251 reads "Fig C1 and fig E1in appendix" – then why there is appendix D between C1 and E1).

There is a long discussion on Figure C1 that is a bit awkward for me and not needed. I mean, the presence of low salinity in the Southern Ocean due to sea ice melting is an information relatively straightforward and widely admitted.

The back trajectories are very useful, one distribution of salinity from remote sensing of reanalysis validated by the onboard thermosalinograph is largely enough for me, but personally, I don't need more proof of that. It is, of course, the authors' responsibility to add more figures, but keep in mind that at some point, the paper is becoming cumbersome with non-essential or repeated information. At some point, reading the results, I was starting to be confused, jumping from one figure to another one, in the main text, the appendix, or the supplemental, going back and forth. I would either remove some figures, like C1, or I would order them better, in only two places, in the order of appearance in the text, with careful references in the text to help the reader to follow your ideas.

The reviewer is right. The information given in figure A1 is indeed already available in figure 4. However, we kept figure 4, it is necessary to show a focus on the periods of high local biological activity, during which NCP was estimated. Figure 4 also shows a direct comparison of the MLD, XLD and wind to the DIC and O_2-O_2 sat, highlighting the diurnal cycles and the mixing events on synoptic time scales. Figure 4 has therefore been moved to the appendix, where it has replaced figure A1. We have simplified figure B2 by removing the ISAS comparison and we have removed figure C1.

We have removed figure E1 from the appendix and we have replaced figure 5 by the figure below:

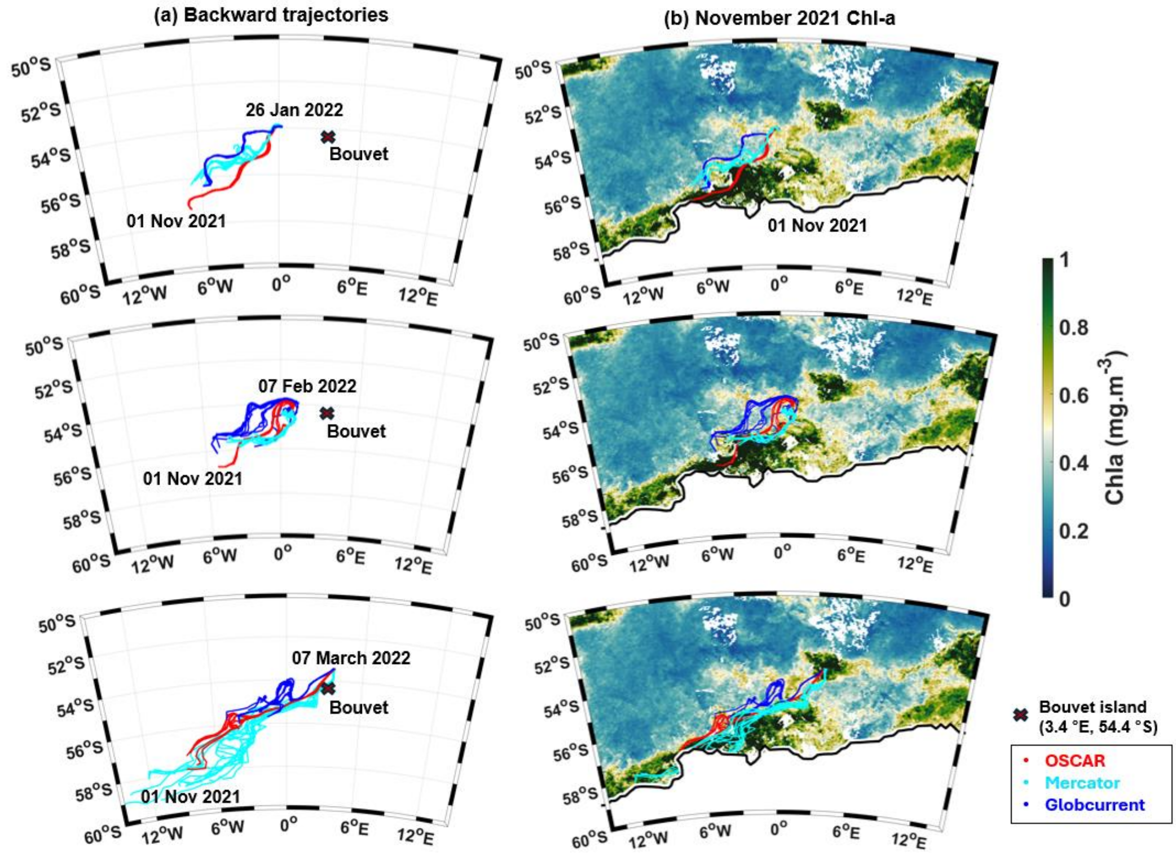


Figure 5: (a) Backward trajectories from CARIOCA's location in January, February, and March 2022 to November 2021, using different current products (Mercator analysis, OSCAR, Globcurrent). (b) CCI Chl-a in November 2021, with the sea ice concentration at 10%, on the 1 November 2021, and backward trajectories superimposed.

We have reordered the figures in the appendix, according to the order of appearance in the text.

Minor comment

Line 79: It's up to you, but personally, I found the sentences "In the next section, the instruments deployed, the different data sets, and the methodology used will be described. There will then be a description of the results followed by a discussion " unnecessary. In each paper, that is roughly what we expect to find in that sequence. I would remove it therefore.

We have removed that part.

Line 84: “It was anchored at 15m”. What does it mean? I presume that the buoy was NOT anchored, at least with a regular anchor, or it might have used a floating anchor, but then it should be precise.

Yes, it’s a floating anchor, following the currents at 15 m depth, in a Lagrangian way. We have corrected this, at line 84, of the paper.

Line 90: Carioca buoys are formidable instruments, able to withstand the rigours of the Southern Ocean and provide precious data at mesoscale or synoptic time scales. Still, there has been a lot of discussion about the accuracy of pCO2 derived from drifters in the Southern Ocean (Long et al., 2021; Williams et al., 2017; Wu et al., 2022; Zhang et al., 2024), especially SOCCOM drifters. I acknowledge that SOCCOM and Carioca sensors are different, but that is still the same principle, and some potential biases are similar (measurement of pH instead of pCO2, assumption on alkalinity, and so on).

Contrary to SOCCOM floats, the CARIOCA sensor deduces the seawater pCO_2 using a spectrophotometer at three wavelengths and there is no assumption on alkalinity. Through a semi-permeable CO_2 membrane, seawater is brought in equilibrium with a dye solution, thymol blue, and the absorption coefficient of the dye is measured by the spectrophotometer (Copin-Montégut et al., 2004, as cited in our paper, Cf. lines 85 – 90). Indeed, while the SOCCOM floats measuring pH, rely on a hypothesis using an alkalinity-salinity relationship, the CARIOCA sensor measures the pH of a dye solution of which we already know the alkalinity (that is measured in the laboratory before deployment and that is independent of the sea water alkalinity). The pCO_2 calibration was done in the lab, using classical infrared pCO_2 measurements.

According to Copin-Montégut et al., 2004, in the methods section, p 172: “Carbon dioxide in a sea water sample equilibrates with a pH indicator solution across a gas permeable (silicon) membrane in an exchanger cell. The pCO_2 in sea water is calculated from pH and alkalinity of the dye solution at known temperature. The pH is measured using the light absorption properties of the thymol blue diluted in sea water with a constant alkalinity.”

Also, refer to lines 88 – 90 of our paper: “The three wavelength measurements enable correction of any modification of the optical path or of the opacity of the optical cell (Copin-Montégut et al., 2004).”

When the error of ICOS measurements, with direct measurements of pCO2 using shower head equilibrator, CO2 CRD analysers, and regular calibration with standard gas is 2 µatm, I think that the absolute precision of 3 µatm claimed for Carioca buoy should be carefully assessed.

It corresponds to the expected accuracy of these instruments, given the accuracy of the laboratory calibration using IR instrument (Copin-Montegut et al. 2004). Previous checks performed at sea in the Southern Ocean confirmed this order of magnitude (Boutin et al. 2008, supporting information available on "https://aslopubs.onlinelibrary.wiley.com/action/downloadSupplement?doi=10.4319%2Flo.2008.53.5_part_2.2062&file=2062a1.pdf".)

I’m sure it has been, but a reference will be very welcome here. And do you mean precision or accuracy? The concept of absolute and relative precision, both in µatm is not clear to me. Is the absolute precision the accuracy? Should the relative precision be in %? I'm sorry for my naïve questions.

The absolute precision mentioned here is the accuracy. Boutin et al., 2008, compared several CARIOCA drifters data in the Southern Ocean to ship measurements and concluded that the accuracy is 3 µatm and the precision is 1 µatm. We have added a reference to Boutin et al., 2008.

Line 102. Why stop on the 27 June 2022? It's somehow uncommon to present only a part of the data and not the full data set. Is there a scientific rationale for that? Was the rest of the data not interesting enough?

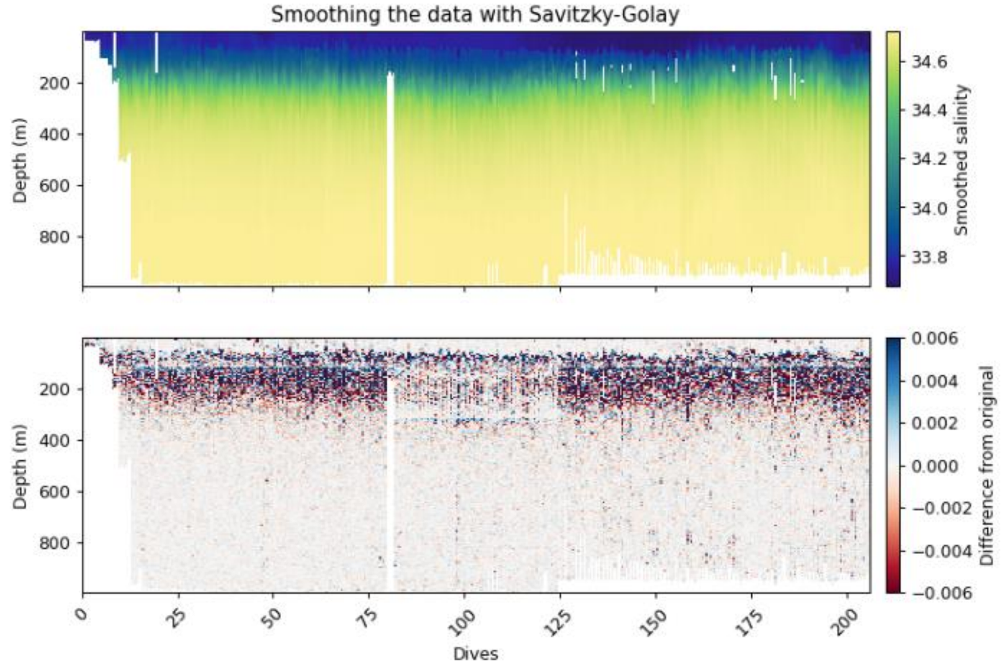
We have added a figure similar to figure 2, but showing the whole time series, to the supplementary materials. (Refer to my comment above, explaining the main points of the CARIOCA whole time series).

Line 128. Could you provide details on that instrument, brand, capability? I've tried to find some information on it, but it was not that easy to find some.

The lines 128-129 will be modified, adding these details: A Kongsberg Seaglider (SG675) was deployed alongside the CARIOCA buoy. A Seaglider is an autonomous underwater vehicle designed to fly through the water column from the sea surface to 1000 m depth following a sawtooth pattern, moving vertically and horizontally at nominal speeds of 0.1 m s^{-1} and 0.3 m s^{-1}, respectively. Upon surfacing roughly every 6 hours, Seagliders communicate to base station via Iridium, thereby transferring data in near-real time. A key characteristic of Seagliders is their ability to be piloted from land, allowing researchers to control the direction of sampling. For this experiment, SG675 followed the CARIOCA buoy for a month and a half, from 31 January 2022 to 10 March 2022 (39 days) (Fig. 1), providing vertical profiles of temperature, salinity, oxygen and fluorescence of the upper 1000 m of the water column.

Line 135. This approach is interesting. Could you provide some details about the Savitzky-Golay filter that would be useful for others, like width and order?

We used a window width of 11 and an order of 2. For more details on this approach, refer to Gregor et al., 2019 and Swart et al., 2024 (cited at line 134 of the paper). On the figure below, the top panel shows the salinity obtained after smoothing the glider data with Savitzky-Golay, and the bottom panel shows the difference (in pss) from the original salinity.



Below are the salinity plots pre and post treatment (after applying all the corrections), with the last panel showing the difference between the two.

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Physics Variable:
        Removing outliers with IQR * 2.5: 0 obs
        Removing spikes with rolling median (spike window=5)
        Removing horizontal outliers (fraction=0.2, multiplier=2.5)
        Smoothing with Savitzky-Golay filter (window=11, order=2)
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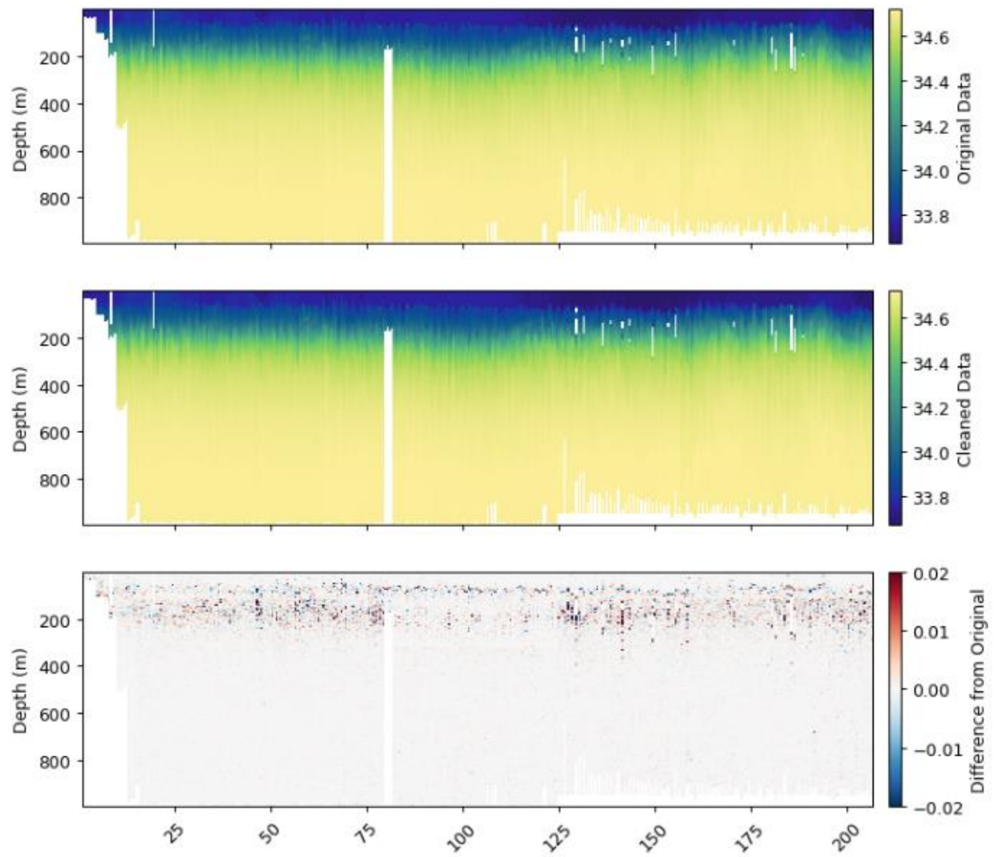

These figures have not been added to the paper but are shown here for reference.

Line141. You're providing details on the calibration of the fluorimeter, but actually, it's calibrated against another fluorimeter (the one from the CTD). But then, how the fluorescence units are converted in chlorophyll (using built-in algorithms)? And why provide details for the sea glider and not from the Carioca buoy? It seems important for me to provide details on converting fluorescence to biomass.

There were no CARIOCA measurements coinciding with the CTD measurements, so we couldn't calibrate the CARIOCA directly using the CTD. On another hand, there were glider fluorescence measurements coincident with CTD fluorescence measurements and some of the latter were also coincident with CTD water samples from which the Chl-a was measured. Hence, since the glider followed the buoy, we calibrated the CARIOCA using the already calibrated glider measurements using CTD information.

Line 144. Remove one parenthesis after "de Boyer Montégut et al., 2004"

We have corrected this, thank you.

Line 144. "mixing" Should it be written "extreme layer depth" instead?

No, refer to Merlivat et al, BG 2022, paragraph 2.4:

"The mixing-layer depth, Z_{mx}, is the upper part of a mixed layer of uniform density where active turbulence occurs (Brainerd and Gregg, 1995)."

Merlivat, L., Hemming, M., Boutin, J., Antoine, D., Vellucci, V., Golbol, M., Lee, G. A., and Beaumont, L.: Physical mechanisms for biological carbon uptake during the onset of the spring phytoplankton bloom in the northwestern Mediterranean Sea (BOUSSOLE site), Biogeosciences, 19, 3911–3920, 2022.

Figure 4. Why did you split the figure and remove the second half of February? It looks like you're cherry-picking and presenting only part of the data set. This approach should be discouraged.

The NCP could be estimated only during periods where we can suppose that the mixing layer is isolated from the rest of the ocean, when the wind is low and there is no mixing with subsurface, and only when there are diurnal cycles of DIC and O_2-O_2 sat in opposition. Using the method from Merlivat et al, 2022, only these two periods of local biological activity could be identified. (We used the same methodology as in Merlivat et al. 2015, but instead of the MLD, we used the mixing layer depth, XLD, to estimate the NCP as in Merlivat et al. 2022. We have added a reference to Merlivat et al., 2022.)

(Also, as per our comment above: On the right-hand side panel of Figure 4 (a), we have removed the MLD as from the 10^{th} of March 2022, since after that date the glider stopped following the CARIOCA.)

Line 229. "This suggests that biological activity was the 230 dominant driver of the DIC seasonal variation". There is something I don't quite understand. You put a lot of emphasis on the impact on biological production. I am not contesting that there was primary production, but, when looking at the DIC, comparing the 01/02/2022 to 13/02/2022 or the 01/03/2022 to 17/03/2022, in both cases, the DIC is increasing overall, NOT decreasing. So there are other processes at work, mixing, air-sea exchange, and water mass change, that outweigh the decrease in DIC. I would not write, therefore, “the dominant driver”. It is not for me.

The reviewer is correct. There were periods of local biological activity, during which the NCP could be estimated, but these were very short periods, spanning only a few days and were rapidly compensated by other events such as mixing. Outside the brief periods of local biological production and of mixing events, the DIC and the fCO_2 remained relatively stable at a low value in January and February. For instance, between the 11/02/2022 and the 03/03/2022, the DIC remained at a mean value of 2133 µmol kg^{-1}. It is very unlikely that the bloom observed locally, west and north-east of Bouvet Island, was what caused the very low values of fCO_2 observed. We put more emphasis on the fresh water mass, already poor in carbon, which came from the ice edge. We removed line 229 and we corrected the text accordingly.

Line 241 to line 245. Could you indicate the relevant figure? I am a bit lost among the 80 plots of the paper.

We added a reference to Figure 5.

Line 247. “The decrease in salinity started near the South Sandwich trench, around 25° W and 60° S, near the sea ice edge in September 2021”. How can you see that? I mean, the data are blanked in the figure. I don’t understand the discussion actually. The ice retreats, but in the west, there is low salinity, and in the East, there is high salinity.

Line 248: and 60° S, near the sea ice edge in September 2021, when the sea ice started to retreat

For simplicity, we removed these sentences at lines 246 – 249: “The formation of this fresh water mass can be seen, from September to December 2021, using salinity maps and sea ice data from OSI SAF (Fig. C1 in Appendix). The decrease in salinity started near the South Sandwich trench, around 25° W and 60° S, near the sea ice edge in September 2021, when the sea ice started to retreat. It continued to develop eastward near the sea ice edge until November 2021 (Fig. C1 in Appendix).”

Part 4.3. I’m not convinced by the rigour of the approach (assuming no mixing), but more importantly, by the interest of such computation subjected to caution, while it’s not clear to me what this computation is bringing to the overall conclusion. It’s not my decision, but I would remove that part.

The local summer bloom in which the CARIOCA entered in 2022, didn’t seem to generate a decrease in fCO_2. Indeed, as the reviewer rightly pointed out, there was no overall decrease in DIC from January to March 2022. However, although there were sometimes slight increases in DIC due to other compensating events such as mixing, the fCO_2 and DIC values remained relatively low in summer 2022. Our hypothesis is that these very low fCO_2 and DIC values are not due to the local summer bloom in itself, but rather due to the fresh water mass, already poor in carbon, advected from the ice edge in November 2021 to the position of the CARIOCA in January 2022. Our computation reinforces our hypothesis, that the summer 2022 carbon sink was caused by early sea ice retreat. Indeed, for our estimations we use an NCP value typical of sea ice retreat starting as early as September, for the spring 2021 bloom (Cf. Line 497 of the paper). Concerning our assumption that there was no mixing; by cross-referencing different salinity products (Mercator / Glorys and SMOS), we were able to determine that the SSS anomaly probably appeared at the same time as sea ice started retreating and then persisted several months at the surface and was advected to the CARIOCA's position in summer 2022 (not shown in the paper). Mercator's vertical salinity profiles (not shown in the paper), also confirmed the hypothesis of a surface fresh layer, which formed during ice melt and then stayed at the surface, and was advected north-east, for several months. For such a surface salinity anomaly to persist for so long, we therefore assume that there was probably very little mixing during that time. We added these details to the paper, putting more emphasis on the contribution of the waters advected from the ice edge.

Supplemental.

There is a poor definition of the figures when zooming in. It's difficult to see the details.

We have removed figures S3 and S4, since the information provided by these figures is already summarised by figure 9.

References

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