Leseurre, C., Lo Monaco, C., Reverdin, G., Metzl, N., Fin, J., Mignon, C., and Benito, L.: Trends and drivers of sea surface fCO_2 and pH changes observed in the Southern Indian Ocean over the last two decades (1998–2019), Biogeosciences Discuss. [preprint], https://doi.org/10.5194/bg-2022-22, in review, 2022.

Review 1 (anonymous):

We thank reviewer 1 for her/his review, comments and questions that will be considered when revising the manuscript. Below we list our responses before preparing a revised manuscript.

Summary

"The authors present a detailed analysis of multi-decadal summer trends in fCO2 and pH in the Indian sector of the Southern Ocean. They present a clear, detailed comparison with past works to discuss trends and investigate the drivers behind the changes reported within. This work is valuable to the ocean carbon community, especially given the in depth analysis for a less-observed region such as the Indian Ocean. While I found the results stimulating and well-written, the section explaining the set up of the analysis left me often confused and could use some attention to clarify for the reader."

Response: We agree with the reviewer. The section explaining the set up of the analysis will be rewritten to improve the understanding. This was also emphasized by reviewer 2.

Overarching comments

"You are only evaluating summer trends in this work, which is valuable of course, but still limited in the interpretation of your findings. Your title does not indicate that these are specifically results only for the summer season. You state clearly on Pg 3, Line 11-13 that the pH trend in the Drake Passage varies greatly in the region and by season so the fact that the results discussed and analyzed within are only inferred from summer observations is an important fact to include in the title."

Response: We also agree with the reviewer on this point. In the main part of the manuscript, we use only data collected during summer. The new title could be: "Summer trends and drivers of sea surface fCO_2 and pH changes observed in the Southern Indian Ocean over the last two decades (1998-2019)".

We however also use "winter layer" water data (from stations) to discuss anthropogenic carbon changes.

"While the results of this analysis are valuable and clearly discussed (Section 4), the leadup (Section 2 specifically) left me lost as to understanding what data was being used, where the comparisons were being made, over what regions (how many?), etc. Table 1 (as I mention below) is overwhelming and

needs to be split into multiple tables to truly allow the reader to follow along (scattering these throughout Section 4 so that you can see the trends right there when discussing the comparisons would be helpful)."

Response: We understand your difficulty to understand each of the datasets used. We are rewriting Section 2 and cut Table 1 in several sub-tables which will be successively commented upon in Section 3 (Results). This seems a better place for these tables than in Section 4 (Discussion). We will comment in more detail these new tables in the section on technical comments for Table 1.

Our study area is separated into 2 regions: south of the polar front, in the permanent open ocean zone (POOZ), and north of the polar front in the polar front zone (PFZ). In each region we distinguish high nutrient and low chlorophyll waters (HNLC) from fertilized waters (bloom). As our initial work was centered on stations (because of information on winter layer), we were interested to find how representative the stations were of the surrounding area. To do this, we again divided our study areas:

- PFZ HNLC (Station O7, O8, O9)
- PFZ bloom Crozet (Station O6)
- PFZ bloom Kerguelen (Station O12)
- POOZ bloom Kerguelen (Station A3)
- POOZ north HNLC (Station O10)
- POOZ south HNLC (Station O11)

We chose to separate the HNLC part of the POOZ in two parts because theses 2 stations are very distant. In addition, station O10 (bottom 1650m) was occupied more often than station O11 in open ocean (bottom 4850m).

Thus, we end up with six areas which characteristics are listed in Tables 1 and 2. This is also what is illustrated by the red "boxes" in Figure 2 (we will come back to this figure in more details for the technical comments).

For the two underway datasets, we grouped and averaged data in small homogeneous boxes (for physical and biogeochemical parameters) which are presented in Figure 4. These small boxes correspond to the yellow "boxes" on Figure 2 (you are right, this was very confusing…).

The section 2 will be revised as:

2 Material and methods

2.1 Study area

[…]

Figure 1.

“To investigate the long-term fCO2 and pH trends we thus separate the domain in 6 main sectors (Fig. 2): (i) HNLC waters in the Polar Front Zone (PFZ) between the SAF and the PF, (ii) part north and (iii) part south of HNLC waters south of the PF in the Permanent Open Ocean Zone (POOZ), and the phytoplanktonic bloom regions associated with (iv) the Crozet shelf, (v) the north and (vi) the south of Kerguelen shelf .”

“The HNLC waters in the POOZ have been divided into northern and southern parts because the two stations in this region are very distant (O10: 50.6°S and O11: 56.6°S; Fig. 1). Station O10 is at the edge of the continental shelf of Kerguelen (bottom 1650m) and was occupied more often than station O11 in the open ocean (bottom 4850m).”

2.3 Data selection

[…]

Figure 3.

“In order to estimate the trends from underway datasets, gridded values for each cruise were averaged in boxes of 1° of latitude and 2° of longitude. Some boxes were enlarged if the surrounding boxes were homogeneous both for physical and biogeochemical parameters. Then trends were estimated provides some conditions are fulfilled (as on Figure 4): the box must contain at least 8 cruises (years) and must have been visited at the beginning of the period, in at least one of the years 1998, 1999, 2000, as well at the end of the period, in at least, one of the years 2017, 2018, 2019. Finally, the boxes were grouped into six large regions (Figure 2). As we are interested in separating the anthropogenic signal from natural variability for both fCO_2 and pH trends, and because anthropogenic CO_2 concentrations are not well evaluated in surface waters, we also estimated the trends at each station selecting the data just below the summer mixed layer (a layer referred to as BML). South of the PF, this subsurface layer corresponds to the Winter Water well identified by a subsurface temperature minimum observed in summer at 150-200m (Fig. 3; Metzl et al., 2006; Mackay and Watson, 2021).”

“From the station dataset, the mixed layer was defined for each station and each year. To evaluate the depth of the mixed layer we carefully looked at profiles for each station and each period and identified the layer where properties are homogenous (including O_2, nutrients, A_T and C_T). On average the summer mixed layer depth over the period 1998-2019 is between 50m and 75m for the PFZ region (station O6, O7, O8, O9, O12) and between 75m and 100m for the POOZ region (station A3, O10, O11). Results for each station in the mixed layer will be then compared to those obtained in the corresponding boxes and regions.”

Technical comments

Pg 1, Ln 34: Awkward, consider rewording: "…since the start of the industrialization."

Response: The sentence will be revised as: "CO_2 emissions into the atmosphere have been steadily increasing since the beginning of the industrial age."

Pg 2, Line 6-7: Are the decadal changes in response to climate change (as stated in the sentence) or because of climatic shifts/transitions such as the SAM index (which is mentioned later in the sentence). I do not consider these things the same thing.

Response: We are aware that climate change and climatic shifts/transitions (such as the SAM index) are not exactly similar. Transitions of the SAM index can be either natural or climate change related (but the reverse is not true). We can certainly modify our sentence (see below) but we will not investigate it further, as this is beyond the scope of this study.

"It is now well recognized that the Southern Ocean experienced changes in the carbon uptake at decadal scale in response to natural or climate-induced variability (notably the Southern Annual Mode): a weakening of CO_2 uptake in the 1990s in connection with increasing winds (e.g. Le Quéré et al., 2007; Metzl, 2009; Lenton et al., 2009), followed by a reversal of this trend until the early 2010s (Landschützer et al., 2015), and since 2011 a decrease in the CO_2 sink is detected (Keppler and Landschützer, 2019)."

Pg 2, Line 25-26: consider saying, "…the model-specific/dependent evolution of the Southern Ocean carbon sink."

Response: Thank-you. This will be included.

Pg 2, Ln 29-30: Consider comparing to (or at least referencing) the Ocean-SODA product: Gregor, Luke; Gruber, Nicolas (2020). OceanSODA-ETHZ: A global gridded dataset of the surface ocean carbonate system for seasonal to decadal studies of ocean acidification (v2021) (NCEI Accession 0220059). NOAA National Centers for Environmental Information. Dataset. https://doi.org/10.25921/m5wx-ja34. Version 2021. NOAA National Centers for Environmental Information Dataset. (https://www.ncei.noaa.gov/access/oceancarbon-data-system/oceans/ndp_103/ndp103.html)

Response: Thank you for reminding us of this reference. We choose to cite the corresponding paper (rather than the database): Gregor, L. and Gruber, N.: OceanSODA-ETHZ: a global gridded data set of the surface ocean carbonate system for seasonal to decadal studies of ocean acidification, Earth Syst. Sci. Data, 13, 777–808, https://doi.org/10.5194/essd-13-777-2021, 2021.

The sentence will be revised as: "The long-term decrease in sea surface pH has been revealed from direct observations at regional scale (notably at time-series stations, e.g. Bates et al., 2014) or at global scale using reconstructed pH fields (e.g. Lauvset et al., 2015; Jiang et al., 2019; Chau et al., 2020; Gregor and Gruber, 2021; Iida et al., 2021)."

Pg 2, Line 42: The stated trends here (+1.0 uatm/yr and +4.0 uatm/yr)- are they from literature or from this work? It's unclear since it's the same range stated in the abstract. If it's from other work (which would be more appropriate given that it is in the Introduction) please reference which papers that provide these specific trends.

Response: You are right, this is so confusing. We were talking about all the previous references in the Southern Ocean (fCO_2 data). Indeed, the range of variation of fCO2 trends is not precisely reported in the draft. If you look at Table S1 and the previous references, the range is actually between +0.9 to +4.2 µatm yr^{-1} (Metzl, 2009; Takahashi et al., 2009, 2012; Lourantou and Metzl, 2011; Lenton et al., 2012; Fay and McKinley, 2014; Tjiputra et al., 2014; Lauvset et al., 2015). To clarify this, we will rephrase the sentence as:

"In other regions of the Southern Ocean, the trends of fCO_2 were evaluated from the synthesis of fCO_2 data and vary between +0.9 µatm yr^{-1} and +4.2 µatm yr^{-1} (Metzl, 2009; Takahashi et al., 2009, 2012; Lourantou and Metzl, 2011; Lenton et al., 2012; Fay and McKinley, 2014; Tjiputra et al., 2014; Lauvset et al., 2015). This range corresponds to different regions and periods, but most values in the open ocean are close to the increase in the atmosphere (around +2.0 µatm yr^{-1})."

Pg 3, Ln 4: In the Brown et al. 2019 reference, the analysis is 1993-2017 but then the explanation later in that sentence says it is analysis done "using few years of data". I wouldn't call 24 years "a few".

Response: We fully agree with the reviewer; this sentence is very contradictory. In fact, we wanted to point to Lenton et al. (2012) which uses 8 years of data (2001-2008). We will review this part such as:

"A few results present negative fCO2 trends in summer, but these are not significant, for example -0.8 (±1.0) µatm yr-1 in the Western Antarctic Peninsula over 1993-2017 (Brown et al., 2019) or -0.9 (± 2.5) µatm yr-1 in the Atlantic sector over 2001-2008 (Lenton et al., 2012), highlighting the difficulty to evaluate the trends in summer when using few years of data."

Pg 3, Ln 5: Disconnected transition between these two paragraphs. Consider adding a sentence to guide the reader on this transition.

Response: This will be added.

"Like for fCO_2, the pH trends previously estimated in the Southern Ocean present large range (Table S1). This is not surprising as the observed pH trends in this region were generally deduced from fCO_2 data, i.e. not from direct pH measurements."

Figure 1: I wouldn't say this region is the "South-Western Indian Ocean" but more so the Indian Ocean sector of the Southern Ocean.

Response: We fully agree with the reviewer. The new title of Figure 1 will be: "Map of the Indian sector of the Southern Ocean […]".

Pg 4, Line 11: You say these observations are from the OISO cruises but then in Figure 2 caption (and elsewhere) you state that the fCO2 is from SOCATv2020. I understand that you used the fCO2 values from the cruises that the SOCAT program calculated (as opposed to the measured pCO2 values on the cruise) but perhaps consistency would help to clarify this. Are the observations in Figure 2 from all observations available in SOCATv2020 in this region? Or just from the OISO cruises? Perhaps these are indeed the only obs available in SOCAT for this region but I just find it confusing when you don't mention SOCAT at all in first paragraph of Section 2.2.

Response: Actually, we use all the fCO_2 data available for this region during summer in the SOCAT database. But as you mention, these are mostly data from the OISO cruises. Indeed, we only used one supplementary cruise (expocode 74E320041213 in SOCAT.v2020). This ambiguity will be corrected, and we will also mention it in Section 2.2.

"The surface underway fCO_2, A_T and C_T data (and metadata) are available at the NCEID/OCADS database (www.ncei.noaa.gov/access/ocean-carbon-data-system/oceans/VOS_Program/OISO.html). The oceanic fCO_2 data are also available in the SOCAT data product (Bakker et al., 2016). Note that all the data used come from fCO_2 measurements from OISO cruises, except one in 2004/2005 near Crozet Island (expocode 74E320041213 in SOCAT). Note also, that when added to SOCAT, original fCO_2 data are recomputed (Pfeil et al., 2013) using temperature correction from Takahashi et al. (1993). Given the small difference between sea surface temperature (SST) and equilibrium temperature, the fCO2 data from our cruises are identical (within 1.0 µatm) in SOCAT and NCEI/OCADS. Here we used fCO_2 values as provided by SOCAT."

Figure 2: This is a very confusing figure. First, I don't feel that the black dots marking the cruise lines/fCO2 observations are needed. I do not understand the point of the yellow boxes (labeled in caption as "yellow squares" even though they are not squares and it is unclear their purpose of being labeled on the map. Could you just say that the data within +/- latitude bounds were averaged monthly for trend analysis? The red "squares" (again, not squares and very difficult to identify the boundaries) are used the identify the 6 areas used for trend analysis. But in the abstract you say you do analysis over "three domains". Why such jagged and somewhat random region definition? Why not just N/S of the frontal zones and then define an area around the islands based on the chl bloom or simple lat/lon range? Overall, as a reader, just from figures 1 and 2 im confused and overwhelmed. Is it 3 regions or 6? Is it based on the fronts or not (many red and yellow-defined regions in Fig 2 cross the frontal boundary). Even a statement about why it's so important to have these strange boundary regions would at least allow the reader to understand the need for something as confusing as this.

Response: Thank you for pointing out these problems (as did reviewer 2). We will respond to your comments:

The black dots correspond to all the data used. This figure was originally made to show the amount of observations and secondly the boxes (but that became very confusing).

The yellow boxes (represented by yellow borders) are supposed to represent each grouping by latitude and longitude done to construct Figure 4. We agree with the reviewer, that on Figure 2 this is not at all clear. What was done, is that initially we constructed boxes of 1° of latitude and 2° of longitude (where T, S, A_T, C_T, or fCO_2 are homogeneous). Secondly, we enlarged these boxes when the stations were in a corner of the boxes, or when the longitudinal surrounding boxes were homogeneous between them, to form one larger box. You can see on Figure RC1-1 what the yellow boxes are supposed to be (without the red boxes on top).

Your suggestion to simply say that the data has been averaged into x° latitude and x° longitude boxes for each cruise, seems a much simpler solution, than to show it on a figure, which is too heavy.

The red boxes correspond to the six areas presented above and to estimate the underway trends presented in Table 1. Again, we agree that this is not legible in Figure 2. You can see on Figure RC1-2 what the red boxes are supposed to be (without the yellow borders).

We tried to separate the yellow and red boxes in Figure 2 in order to show in which boxes the different trends were estimated (for Figure 4 or Table 1). But we agree that this is not clear and adds confusion. Instead, we decided for the new Figure 2 to present the Figure RC1-2.

You are right that our definition of regions is not expressed in the same way throughout the article. Finally, our study area was separated from N/S of PF and then 3 areas near the islands (station O6, A3, O12) based on the chl-a bloom. We cannot lump together these 3 stations located in 3 different blooms

which present different results associated with rather different bloom characteristics. But we can certainly group together what we call the POOZ north and POOZ south which present similar tendencies. However, by keeping these two regions separate, we show that even a station close to the Kerguelen plateau (O10) is representative of the POOZ.

Concerning the PFZ HNLC region (Figure RC1.2): yes, this box crosses the frontal boundary. But we represent an average position of the PF which can be very variable west of Kerguelen (Pauthenet et al., 2018).

Pg 5: After reading through your discussion of the observation methods and related uncertainties I think it would be very valuable to have a table (or add it on to another table) where you include these uncertainties due to measurement along with the trends you are seeing. The uncertainty calculation you explain on Pg 8, Ln 32 is outside of the uncertainty in obtaining the measurements themselves so this would be a worthwhile comparison to allow for in a table.

Response: Thank-you for this comment. A table could be interesting. On the other hand, Figure 6 provides already an estimate of what are the uncertainties on each contribution to the trend (see error bars for each contribution). Thus, a table would be redundant with this information.

For each calculated data, an uncertainty related to the calculation is associated (with CO2SYS). But then we make an average of several data in a box. Thus, the uncertainty associated with the average in the box is much higher than the error related to the measurement.

Pg 6, Ln 19-22: Here is where you explain the "three datasets" you will evaluate trends for. This needs to be more prominent, but also it is confusing because it is more than just 3 datasets as it is shown in Table 1. I understand the 3 to be 1 using underway fCO2, 1 using underway At and Ct, and the last using mixed layer values at stations. Perhaps acronyms or abbreviations for these would be helpful to define/assign because Table 1 is absolutely overwhelming in its current format. Which leads me to…

Response: Yes, that's right, there are three datasets as you can see in Figure 4 and in each of the regions in Table 1:

- Data in the mixed layer for the 8 stations (OISO only),
- Data from underway A_T and C_T measurements (OISO only),
- Data from underway fCO_2 measurements (from SOCAT, mostly OISO).

The dataset using the mixed layer values at station, includes 8 stations (which are taken separately). We understand that Table 1 is too crowded for you, but the different data used are well specified in this table

each time in the second column. We will divide table 1 into sub-tables, which we hope will make the results clearer (as you suggest below).

Table 1: There is too much text here in one table and it is too small. Perhaps putting the stations in a separate table? Or at least the bottom 3 that are looking at the island bloom regions. Also, why 6 regions when everywhere else in the manuscript you say you are looking at "3 domains"? How do we now suddenly have 6? Why are we breaking it into "north HNLC" and "south HNLC"? no justification was provided for that. Also, in your Table 1 caption it repeats "the HNLC part of the north POOZ and the HNLC part of the south POOZ" which just adds to the confusion. Why present trends with different start/end years and call them comparable? Why not just truncate all time-series to a common trend? Do you consider if the significant trends are also significant with +/- one year of observations? I.e are they dependent on the specific start/end year or are the trends persistent for the long-term (20-ish) year time period overall?

Response: We will respond to your comments in order (for the ones for which we have not already answered earlier on):

We do not think it wise to put the station trends in a separate table. Indeed, the goal is to show that for the 3 datasets, we have similar trends in each region. But we agree with you to divide this table into 3 smaller tables: the results in the 1) PFZ HNLC region, 2) POOZ HNLC region, 3) blooms regions.

Concerning the 6 regions, we have already answered above and this will be clarified.

We are aware that not all periods used to estimate trends are the same. For example, stations O7 and O8 were not sampled in 2018 and 2019. But station O9 was and we also have underway data. We made the choice to use as much available data as possible to highlight the need to continue these sampling cruises. Here is an example for station O9 if we cut the trends to 2017:

T °C yr^{-1}	S yr^{-1}	A_T µmol kg^{-1} yr^{-1}	C_T	fCO_2 µatm yr^{-1}	pH yr^{-1}	periods
0.02 (0.04)	-0.001 (0.002)	0.0 (0.1)	0.5 (0.4)*	**1.5 (0.8)**	**-0.0016 (0.0008)**	(1998-2017)
0.03 (0.03)	-0.001 (0.002)	0.1 (0.1)	**0.6 (0.3)**	**1.6 (0.6)**	**-0.0016 (0.0008)**	(1998-2019)

As the reviewer mentioned, we did not explain why we separated the POOZ into north and south part. A sentence has been added (see the revised section 2.1, above).

Pg 6, Line 32: Please include your chosen definition of the mixed layer here.

Response: To evaluate the depth of the mixed-layer we carefully looked at profiles for each station and each period and identified the layer where properties are homogenous (including O_2, nutrients, A_T and C_T). For this analysis we prefer this "geochemical view" rather than a purely physical (temperature or density criteria derived from CTD-1db profiles) that is sometimes difficult to interpret in the Southern Ocean (e.g. Park et al, 1998). Sentences have been added in section 2.3 (see above).

Pg 6: How does the standard deviation and or n (number of observations) in underway compare to the values at the stations (given the monthly means you calculate here). Such a comparison would help to provide proof that the box is represented by the station.

Response: We select the underway data in a box around each station (yellow boxes). There is thus only 1 average value in the ML per station/year and for underway this depends on the track of the cruise and time when the ship was operating the casts. On average there is between 30-100 underway fCO_2 data and around 10-30 A_T and C_T underway data in each yellow box per year. The comparisons for each station/period are clearly identified in Figure S1 for the 3 datasets.

Pg 7, Ln 14: How often was At not available? Also, did you compare this to the LIAR algorithm of Carter et al? Carter et al., Limnol. Oceanogr.: Methods 16, 2018, 119–131

Response: There was no A_T data at stations during OISO-4 (Jan-2000) because we found large bias and noisy A_T data (not in C_T) probably due to bottles used for the sampling. This is the only explanation we have for this cruise and we prefer to remove A_T data in the file (including for the CARINA or GLODAP data products for this cruise). We used new bottles in July 2000 and there was no more pb for A_T data after 2000.

On the other hand, we are aware of the LIAR method developed by Carter et al (2016) based on T, S, AOU, Si and N data to reconstruct A_T vertical profiles (e.g. for Bio-ARGO). Here we want to reconstruct surface A_T in order to calculate C_T and pH from fCO_2 underway data. For this we used the A_T/S relationship based on Equation 1 (we have no underway AOU, N and Si data needed to apply the LIAR method). Note that other relationships (e.g. Millero et al, 1998; Jabaud-Jan et al 2004; Lee et al, 2006) might be used, but this did not change the trend analysis and we thus used the relationship based on semi-continuous A_T data as explained in the MS.

Figure 5: I like the display of information here (and color designation for the HNLC/bloom stations) but perhaps include a small map of where these stations are located as a subplot to this figure. It would help with the interpretation immensely.

Response: Good point, thank you. Will we include a small map (as Figure 1) on Figure 5 and Figure 6 (see further down).

Pg 15, Line 21: Does the idea that the summer observed trend is close to the trend in the atmosphere imply that "there is no significant change in the CO2 uptake in the summer"? That seems like a confusing way to state it. It means that the ocean is tracking the atmosphere for these decades for summertime ocean carbon uptake. But to say "no significant change"? Change from what? Change caused by biological uptake in summer months as compared to annual trend? In the following sentences, when comparing to the stronger trend reported by Metzl 2009, that is a fair comparison to make but the word choice in that first sentence is inappropriate.

Response: Thank you for pointing out this confusion. We will change the sentence as: "The averaged fCO_2 trend that we estimated over 1998-2019 in the POOZ ($+2.1 \pm 0.3$ µatm yr^{-1}) is close to the trend in the atmosphere, suggesting that there is no significant deviation in CO_2 uptake from equilibration with the atmosphere in summer."

Pg 22, Ln 21-30: The paragraph discussing GOBMs, while valuable and definitely important, seems out of place here. Could you connect it to the work in the introduction to provide some motivation for this work instead of here in the conclusions?

Response: This will be moved in the introduction just after the 3rd paragraph.

" […]. This however depends on both the anthropogenic CO_2 emission scenario (Bopp et al., 2013; Sasse et al., 2015; Jiang et al., 2019; Kwiatkowski et al., 2020) and the way the Southern Ocean carbon sink will evolve in the future.

Global Ocean Biogeochemical Models (GOBM) attempt to reproduce the ocean CO_2 sink over several decades, since the 1960s and in the future, are generally consistent with data-based methods at global scale, but at regional scale discrepancies are pronounced, especially in the Southern Ocean (Hauck et al., 2020). Comparison of fCO_2 (and air-sea fluxes) in the Southern Ocean between models and observations also shows discrepancies at seasonal scale due to incorrect or missing biophysical processes in models (e.g. Lenton et al., 2013; Kessler 25 and Tjiputra, 2016; Mongwe et al., 2018) leading to large bias in timing and amplitude of the C_T and/or SST cycles (a value of simulated annual ocean CO_2 sink might be correct but for the wrong reasons). This is a problem when using current Earth

System Models (ESM) to project future changes of the ocean CO_2 sink (Kessler and Tjiputra, 2016) or ocean acidification in the Southern Ocean (Sasse et al., 2015). For these reasons, it is important to maintain the continue and understand the observations of carbon cycle to feed the models.

The long-term decrease in sea surface pH has been revealed from direct observations at regional scale (notably at time-series stations, e.g. Bates et al., 2014) or at global scale using reconstructed pH fields (e.g. Lauvset et al., 2015; Jiang et al., 2019; Chau et al., 2020; Iida et al., 2021). […]."

References.

Park, Y., Charriaud, E., Ruiz-Pino, D., Jeandel, C., 1998. Seasonal and interannual variability of the mixed-layer properties and steric height at station KERFIX, southwest off Kerguelen. J. Mar. Syst. 17, 571–586.

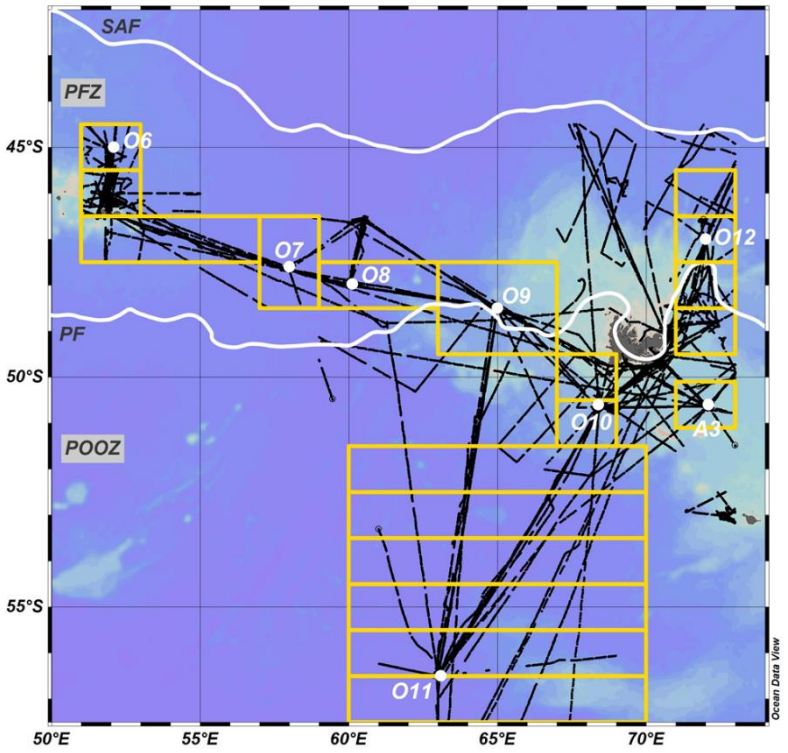

Figure RC1-1. The yellow boxes represent the grouping by latitude and longitude in order to construct Figure 4.

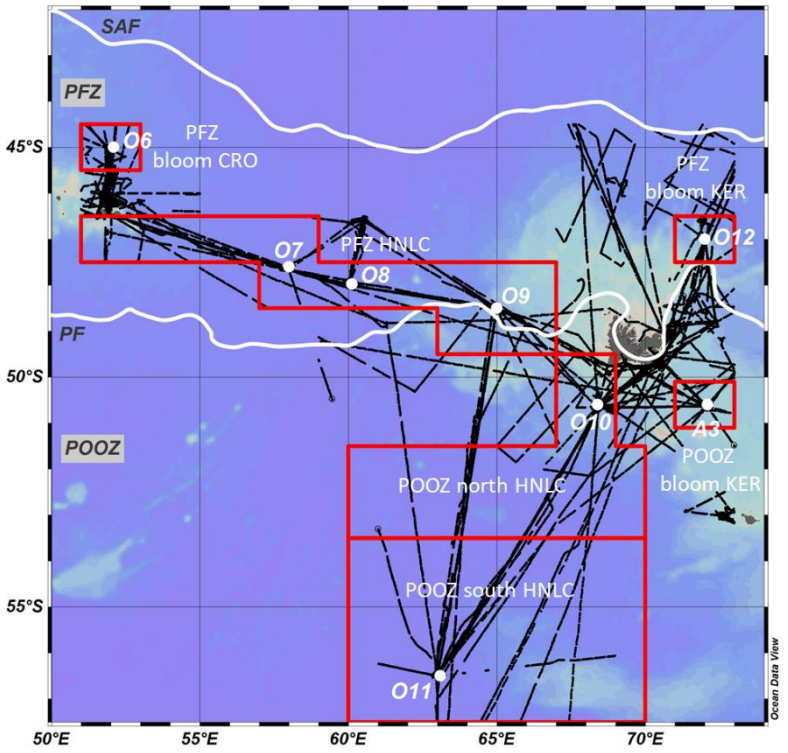

Figure RC1-2. The red boxes correspond to the large regions identified for underway trends presented in Table 1.

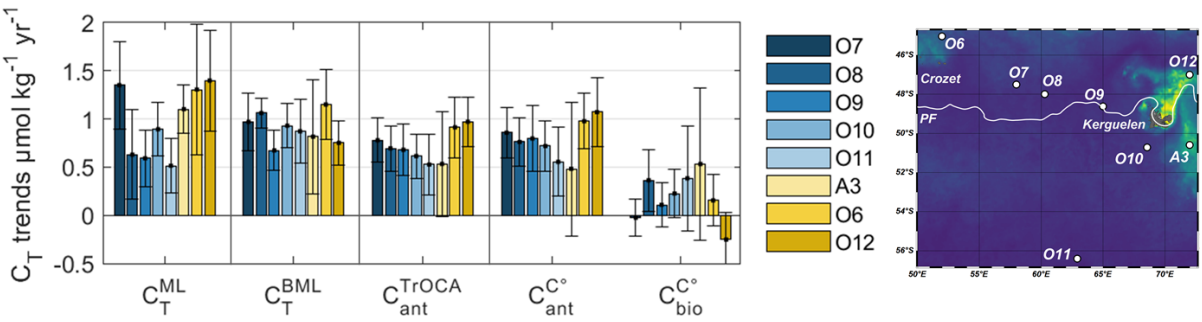

Figure 5. CT trends in mixed layer (ML) and below mixed layer (BML). Decomposition of C_T^{BML} in C_{ant} (TrOCA and C^0 methods) and C_{bio} from C^0 method. The three phytoplanktonic bloom stations are shown in yellow (last) and are separated of the HNLC stations (first five, shown in blue). To help the interpretation, a map with localization of these station is included.

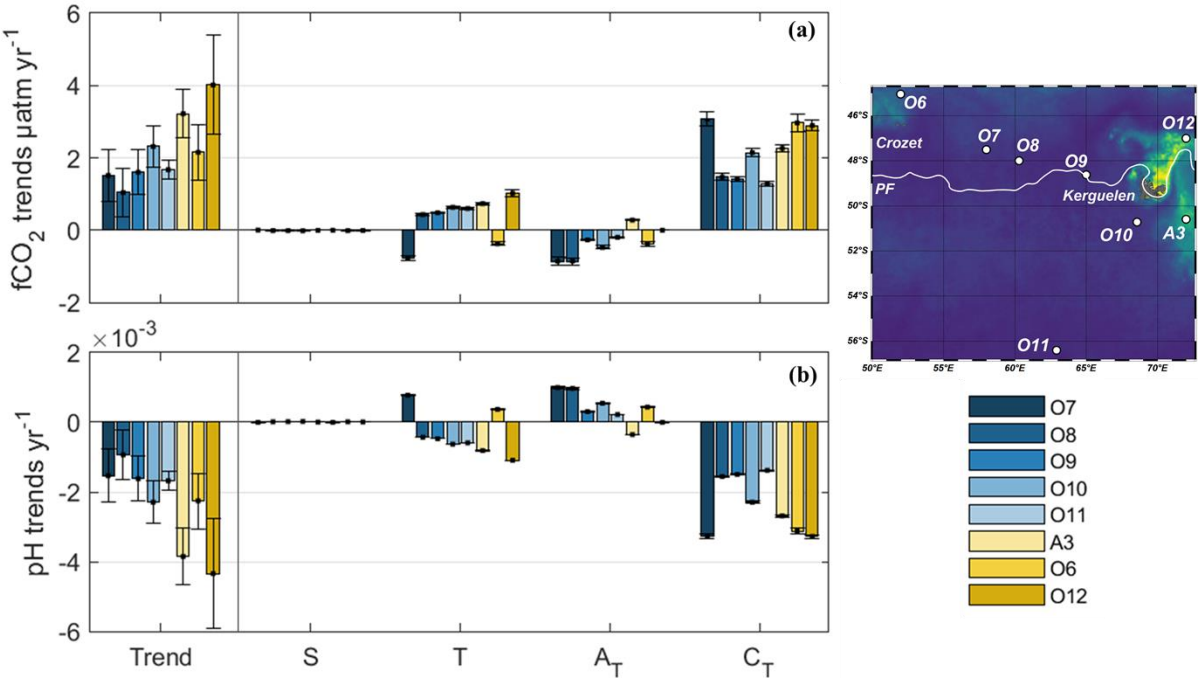

Figure 6. Trends and decomposition of fCO_2 (a) and pH (b) trends in mixed layer, according to Eq. 9. The effect of change in salinity (S), temperature (T), total alkalinity (A_T) and carbon (C_T) is shown. The three phytoplanktonic bloom stations are shown in yellow (last) and are separated of the HNLC stations (first five, shown in blue). To help the interpretation, a map with localization of these station is included.