Replies to reviewer 1 (Her/his Report published 22/12/23)

Article : Metzl, N., Lo Monaco, C., Leseurre, C., Ridame, C., Reverdin, G., Chau, T. T. T., Chevallier, F., and Gehlen, M.: Anthropogenic CO2, air-sea CO2 fluxes and acidification in the Southern Ocean: results from a time-series analysis at station OISO-KERFIX (51°S-68°E), EGUsphere [preprint], https://doi.org/10.5194/egusphere-2023-2537, 2023.

The reviewer's comments and questions are written in black; our replies are in red.

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Nicolas Metzl et al. present 36 years of measurements of carbonate system properties in the Indian Southern Ocean, an area important for oceanic CO2 uptake. They investigate multi-decadal trends and their causes. Among other results, they find the accumulation of anthropogenic CO2 to be the dominant control on carbon and acidification trends in their study area, modulated by natural decadal variability. Metzl et al. use the observed trends to extrapolate into the future, including estimates of the time of crossing undersaturation thresholds important for marine life. The findings are presented clearly and convincingly. I certainly recommend to publish this work. I would like to add that the sustained efforts to maintain high-quality oceanic measurements over such a long time frame already represent a great value for the research community in itself, far beyond what can be discussed in a single paper.

Response: We warmly thank the reviewer for her/his supportive report.

Minor comments meant to further improve clarity:

Title: consider mentioning "Kerguelen" as a more easily recognizable geographic term
Response: Thank you for the suggestion, but we prefer not to mention "Kerguelen" in the title, because the analysis focused on a region SW of the Islands (in the POOZ). These data correspond to an HNLC area, and are not in the north-east or the south-east of Kerguelen Islands (fertilized areas with sustained blooms) that are more commonly associated with the Islands.

Line 59: Rödenbeck et al (2022) note that interannual variability before the 1990s may be underestimated because there are hardly any pCO2 data to add variations not captured by the extrapolation based on relationships to predictor variables
Response: We agree with this comment. Here we wanted to refer to recent data-products that evaluate the CO2 sink back to 1957. No change.

Lines 252-253: Why would the lower salinity speak against being representative?
Response: The change in salinity might inform on different water masses (occasionally) or variation in E/P budget (for decadal and long-term trend, as recently observed, Akhoudas et al, 2023 or for future scenario as discussed in the section 3.4.2). This would potentially impact N-AT, N-CT or pH trends. Here we find that salinity surface data showed some low values in 2011-2013, but such signal has no impact on our long-term analysis (i.e. the period 2011-2013 was thus not filtered in the trend calculations). We also tested the impact of salinity change in the future and conclude (line 1138) that "the impact of AT decrease has a minor effect on the future change for pH".

Line 258: Just to clarify: Will the data not be present in any upcoming GLODAPv2.20xx release?
Response: The recent OISO stations data are not part of the current GLODAP version (Lauvset et al, 2024, ESSD submitted), but will be part of the following one.

Line 433: The seasonal cycle is actually difficult to see from Fig 2a

Response: Figure 2a aimed at presenting all data available and results from the FFNN model; this indicates that data exist for different seasons, but for trend analysis, only summer data could be used. The seasonal cycle is more clearly seen in Supp Mat Figure S4 for 1985. No change.

Line 435: Somewhat unclear: do you mean the annual CO_2 flux 1985-1998, or the winter one?
Response: Thank you for this comment. Yes, this is for the annual flux as shown in figure 3. The sentence will be revised as follows: "The model also indicates that between in 1985 and the mid-1990s the fCO_2 during austral winter (May-September) was always higher than the atmospheric fCO_2 leading to an annual CO_2 source during this period (Figure 3)."

Line 494: It sounds as if this rate has been calculated from just the difference of 2 values. If so, why not from a linear fit?
Response: The reviewer is correct, here we calculated the trend from only 2 values (the first in Feb-1991 and the last in Feb-2021): 394.9-344.4=50.5 µatm, i.e. +1.68 µatm/yr over 30 years. This very simple calculation was provided as a hint on how the trend would compare to the atmospheric CO_2 trend. We have changed the sentence as follows: "From the first underway measurements obtained at the OISO-KERFIX site in February 2021 to the last measurements used in this study in February 1991, the average oceanic fCO_2 increased by +50.5 µatm (from 344.4 ±1.5 µatm to 394.9 ±1.5 µatm, Figure 2a). During the same period, the atmospheric CO_2 increased by 57 µatm in this region (recorded at Crozet Island, Dlugokencky and Tans, 2022). This first comparison of two cruises 30 years apart indicates that the oceanic fCO_2 increase was close to that of the atmosphere."

Fig 5: I was wondering whether the break point between the 2 trends (orange) wasn't mainly induced by the large values in 1998 and 2000? Visually, the dip in 2008-2010 actually seems to be in the range of the general variability since 2002. Is is really justified to suggest a break in trends?
Response: We explored the Chl-a variability to see if one can interpret the change of the decadal fCO_2, C_T trends due to biological processes. Remote sensing observations (Seawifs) are only available since 1998. During the first period (1998-2010), summer Chl-a appeared higher in 1998, 2000, 2001, 2005, while it did not exceed 0.2 mg/m3 over a relatively long period after that (2006-2013), and since 2008-2010 there is a progressive increase of Chl-a, with some interannual variations but lower than the large anomalies as detected in 1998 or in 2000 at that location. Other analyses at larger scale (basin scale in the SO or HNLC biomes) also suggest a change of Chl-a/productivity before and after 2010 (Basterretxea et al., 2023). We thus think that it is useful to separate the information into two periods: 1998-2010 and 2010-2021.

Lines 682-683: The change is termed "very small", but isn't it actually outside the given uncertainty range?
Response: The reviewer is correct; the difference of 2.72 µmol.kg-1 between 1985 and 2021 is slightly higher than the observed variability (±1.27 and ±0.62 µmol.kg-1 in 1985 and 2021 respectively); this is however a small difference compared to the uncertainty of the TrOCA method used to calculate C_{ant} (±6 µmol.kg-1). We thus estimate that the derived C_{ant} trend in the deep waters is "very small" (Figure R1, no significant decrease or increase). No change.

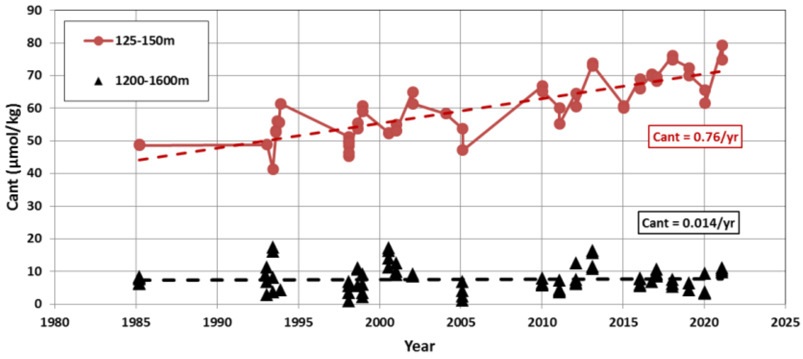

Figure R1: Cant in layers 125-150m and below 1200m. At depth, the signal of the trend is "very small".

Line 694: can you briefly indicate why lower O2 explains lower Cant?

Response: To calculate C_{ant} concentrations in the water column, one separates the contribution of C_T change due to organic matter remineralization. This is achieved using O2 data with an adapted Redfield ratio; following the concept of a quasi-conservative tracer such as NO (Broecker, 1974), TrOCA is a tracer based on a combination of CT, AT and O2:

TrOCA = O2 + a (C_T- A_T/2) and C_{ant} = (TrOCA-TrOCA0)/a

When O2 is lower, TrOCA is lower, indicating higher remineralization that has to be taken into account to correct C_T data. If one observes the same C_T at year 1 and at year 2, but lower O2 at year 2, then C_{nat} (C_T-C_{ant}) at year 2 is higher (i.e. C_{ant} is lower).

Line 716: Maybe replace "filtered" by "excluded" or "discarded"

Response: Thank you, this will be corrected: (anomalies in 1998, 2005 and 2020 excluded).

Line 764: missing "uatm"

Response: Thank you, µatm added on line 764.

Line 765: If I understand correctly, a correlation between Chl-a and fCO2 is built into FFNN. Is such a correlation also seen directly in the data? I'm asking because if not, how do you know it is a real feature and not an extrapolation artifact?

Response: The relations presented in figure 8 are constructed from mean summer Chl-a (from SeaWIFS and MODIS) and fCO_2 or C_T from the FFNN results. This is a real feature, as in the FFNN model, Chl-a, as well as SST or mixed-layer, is a predictor used to reproduce fCO2 (when fCO2 is not available (i.e. no direct correlation is built in the FFNN). Here the link is presented for the seasonal fCO_2 or C_T amplitude. Unfortunately, there is no data each year both in summer and winter for a more direct investigation. Thus, we are not able to evaluate the seasonal amplitude (winter-summer) based on observations, instead using FFNN outputs to explore the link between fCO_2 and C_T seasonality and the Chl-a records. Based on the FFNN model, we observed the inter-annual variability of C_T in summer whereas in winter the C_T follows the C_{ant} uptake (figure S12). Therefore we interpreted the change of C_{nat} (C_T-C_{ant}) seasonality due in part to biological processes

Lines 965-981: It took me quite a while to understand how to read Fig 12, though in the end everything makes good sense. Maybe there is a way to further help readers? I cannot offer a good suggestion either, unfortunately.

Response: Thank you for highlighting this figure; here we aimed at showing on the same plot the properties versus depth or versus time; such a plot was also used in another publication (Metzl et al.

2010, see Figure R4 below) and we thought it was the best way to describe and link the changes in time, in season, and at depth based on observations and FFNN model.

Line 1066: Consider writing "CT(1962)+Cant(t)" in both caption and figure legends, because otherwise the sum "CT+Cant" is quite confusing

Response: Thank you for the suggestion. The caption will be revised:

"The red line is the atmospheric fCO2 and red dashed-lines in each plot are the evolution of properties since 1960 corrected to Cant where fCO2, pH, Ω-ar and Ω-ca were recalculated using CT(1962)+Cant(t)". We also changed the legend in figures.

Line 1096-1097: Couldn't this hypothesis be checked by the FFNN, by calculating the FFNN response to a counterfactually constant SST predictor?

Response: Here we discussed the fCO2 change for the October-November season (same season for the 1962 data). It appeared that fCO2 computed with Cant trend (dashed red line in figure 13) was coherent with observations and FFNN model before 2014 but not after 2015, suggesting that another process explained the more recent fCO2 variability (at least for this season). We suggested that this was due to warming taking place during the recent period. The Reviewer suggests testing the impact of the warming after applying constant SST on FFNN. We have tested this hypothesis by calculating fCO2 with constant SST (at 2°C); this is presented in this review in Figures R2 and R3. When SST is constant, fCO2-2C (orange lines) is close to fCO2 deduced from Cant accumulation after 2016; note also a large anomaly of fCO2-2C in November 2014 when there was a cooling (Figure S9 in the paper) that might be the signal of deeper mixing as suggested from the maximum CT from FFNN (Figure 13 in the paper).

As mentioned in the article, the differences between fCO2 and fCO2 computed from CT+Cant are not very large given the uncertainty in the CO2sys calculation when using AT-CT pairs (+/-13 µatm); we thus cannot interpret the signals beyond what was indicated in the paper. Cruise sampling is marginal for the interpretation of 2-5 years signals, but not for long-term variability (here since 1962) for which our conclusions hold.

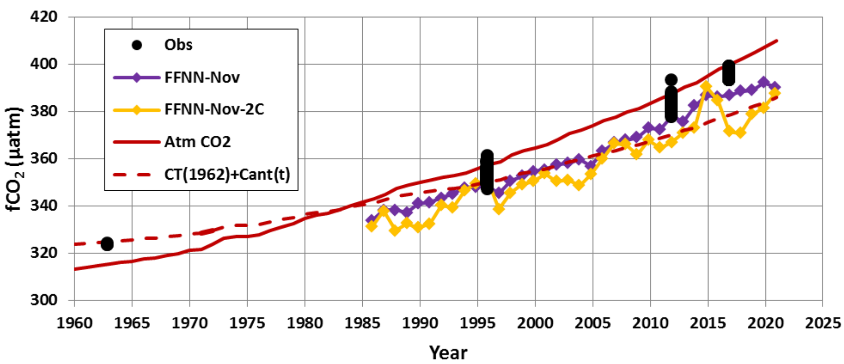

Figure R2: Same as for Figure 13 in the paper with fCO2 from FFNN also at constant SST (2°C) in orange.

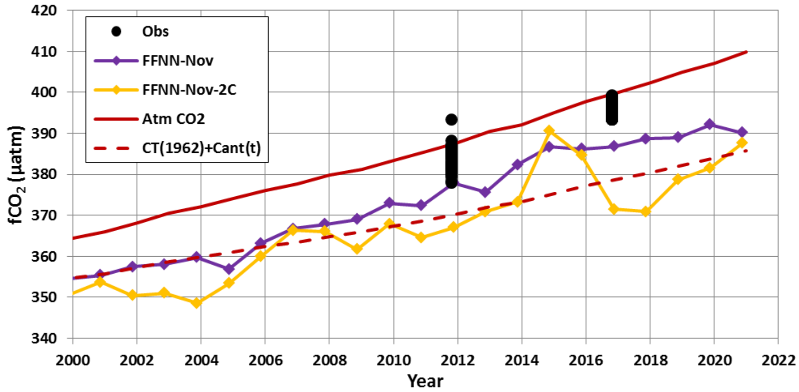

Figure R3: Same as for Figure R2 for years 2000-2021. In 2017-2021 the warming explains why fCO2 was higher than fCO2 due to Cant uptake only.

Line 1110: "undersaturation"?

Response: Thank you, corrected

Line 1312: "BGC-Argo"

Response: Thank you, corrected

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Lauvset, S. K., Lange, N., Tanhua, T., Bittig, H. C., Olsen, A., Kozyr, A., Álvarez, M., Azetsu-Scott, K., Brown, P. J., Carter, B. R., Cotrim da Cunha, L., Hoppema, M., Humphreys, M. P., Ishii, M., Jeansson, E., Murata, A., Müller, J. D., Perez, F. F., Schirnick, C., Steinfeldt, R., Suzuki, T., Ulfsbo, A., Velo, A., Woosley, R. J., and Key, R.: The annual update GLODAPv2.2023: the global interior ocean biogeochemical data product, Earth Syst. Sci. Data Discuss. [preprint], https://doi.org/10.5194/essd-2023-468, in review, 2024.

Metzl, N., A. Corbière, G. Reverdin, A. Lenton, T. Takahashi, A. Olsen, T. Johannessen, D. Pierrot, R. Wanninkhof , S. R. Ólafsdóttir, J. Olafsson and M. Ramonet, 2010. Recent acceleration of the sea surface fCO2 growth rate in the North Atlantic subpolar gyre (1993-2008) revealed by winter observations, Global Biogeochem. Cycles, 24, GB4004, doi:10.1029/2009GB003658.

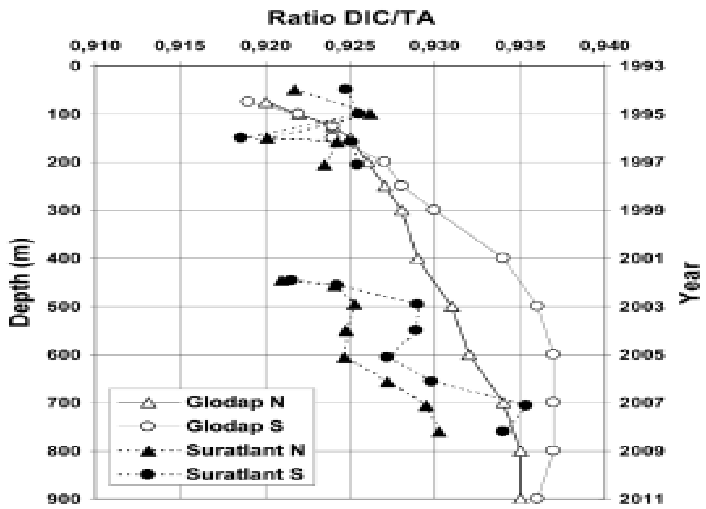

Figure 5. DIC/TA ratio as a function of depth for the northern and southern region in the NASG (mean of GLODAP data along Suratlant line, open symbols, left axis) and DIC/TA ratio as a function of time for the same regions (mean of Suratlant data, filled symbols, right axis). The DIC/TA ratio are higher in the south (in both data sets), and in recent years this ratio has increased in surface waters.

Figure R4: Figure 5 in Metzl et al 2010, for the plot of properties as a function of depth or time (somehow like Figure 12 in the paper).[DIC/TA ratio as a function of depth for the northern and southern region in the NASG (mean of GLODAP data along Suratlant line, open symbols, left axis) and DIC/TA ratio as a function of time for the same regions (mean of Suratlant data, filled symbols, right axis). The DIC/TA ratio are higher in the south (in both data sets), and in recent years this ratio has increased in surface waters.]

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