Responses to reviewer 1

;;;;;;;;;;; Review 1, 5/9/25

This study provides novel results on OA in the Mozambique Channel, addressing relevant scientific questions and contributing to the understanding of Global Change impacts in the Indian Ocean. The authors present recent observations from this region, contrasted with existing datasets and supported by climatologies and neural network approaches. The study design, concepts, methods, and data employed give the manuscript appropriate scientific significance.

However, several aspects require major revision to improve the quality of the research and the presentation of the results and conclusions. In particular, the manuscript would benefit from a clearer description of the study area and its main oceanographic features, a more detailed and structured explanation of the applied methodology, and a careful revision of the trend calculations. Addressing this last point is crucial to strengthen the main conclusions of the study.

AR-01: We thank the reviewer for her/his positive and clear report. Our responses are in blue.

Below, I provide my major and minor concerns.

Major comments

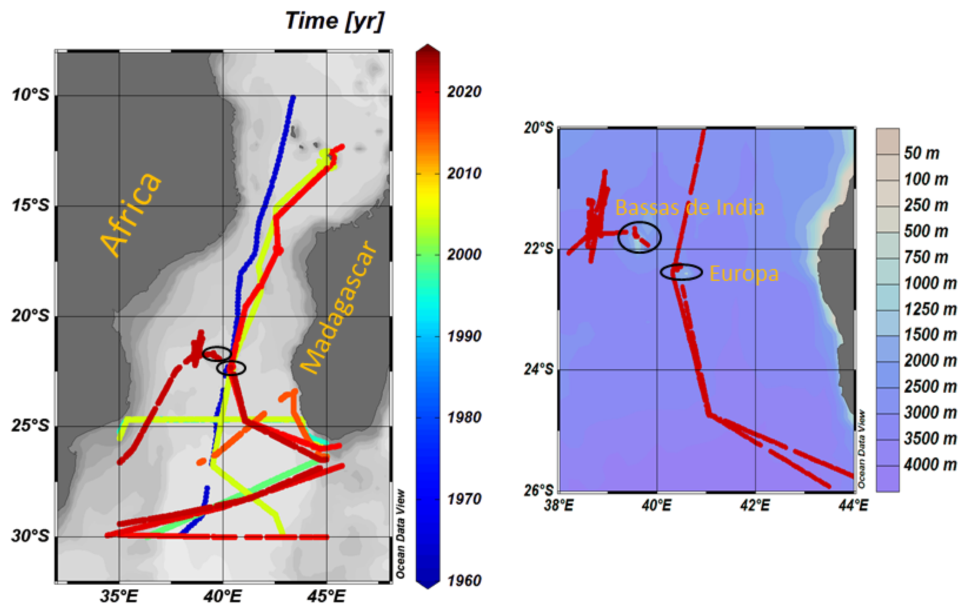
The introduction is concise and well-structured. However, it would be convenient to make explicit reference to the study area and include a description of the most characteristic processes that may act as sources of variability for the CO2 system. Alternatively, a short description could be included in the introduction, with further details provided in a Study Area subsection within the methodology.

AR-02: In the introduction we have referenced to the study area. In short to introduce our study we recalled OA in the global ocean, then in the Indian Ocean and the Mozambique Channel. We are not sure what specific information could be added regarding the processes. At the end of this section we suggest to add: “Both studies concluded that strengthening of acidification trend was mainly driven by ocean CO2 uptake.”

The color code in Figure 1 does not allow interpretation. It is difficult to distinguish the tracks of each cruise, particularly the red ones (from 2010 onwards, when most cruises took place). Please consider using a unique color for each cruise to improve readability.

AR-03: Figure 1 aimed at showing that data exist for different years and identifying locations of crossing when exist. As suggested, we have revised the figure with a different color code. As suggested by reviewer 2 we also add the location of coral reefs (here Bassas de India and Europa in a second map with cruises tracks).

Figure 1 revised: Left: Tracks of cruises in the Mozambique Channel in the SOCAT data-base, version v2024 (Bakker et al., 2016; 2024). This includes recent OISO-31 and RESILIENCE cruises in 2021 and 2022. Color code is for Year. Black circles identified the coral reefs locations. Right: Tracks of cruises near the coral reefs area. Figures produced with ODV (Schlitzer, 2018).



Line 128: How was SST measured? And salinity? What were the instrumental uncertainties? How was the equilibrator temperature corrected relative to in situ values? Even if this information has been described in previous works, it would be helpful to include it in Section 2.2 so that readers can fully understand the methodology.

AR-04: As noted by the reviewer, the methods have been described in many previous studies and we thought it was not needed to recall again these information. Detailed information is available for each cruise in the Metadata file on-line in SOCAT. However, as suggested, we have added information on SST and salinity and mean of Teq-SST for the cruises in section 2.2 as follows:
"The sea surface temperature (SST) and equilibrium temperature were measured using SBE21 and SBE38 probes (accuracy 0.002°C) respectively. During the RESILIENCE cruise the difference of SST and equilibrium temperature was on average +0.088 ±0.066 °C (n= 6416). For all cruises, the sea surface salinity (measured with SBE21) was regularly checked with discrete samples and has been corrected if some drift was observed."

Lines 299–300: "…the climatology (Fay et al., 2024) or the FFNN model (Chau et al., 2024) is coherent compared to the data." Was any intercomparison test performed? It would be appropriate to include results of such an intercomparison, for instance, the mean difference between climatology/NN outputs and observations, and/or provide an additional figure in the Supplementary Material (e.g. time in months on the X-axis and difference on the Y-axis).

AR-05: Thank you. We have compared the values and added a figure in the Supp. Mat. as suggested

New Figure S1: Time-series of fCO_2 (µatm) and pH_T in the southern Mozambique Channel based on observations (black circles) and from the FFNN model (grey diamonds) for the same periods. Standard-deviations are indicated by vertical bars. The differences (FFNN minus Observation) are also shown (Open squares, right axis). In 2018 the fCO_2 from the model is high compared to the observations.

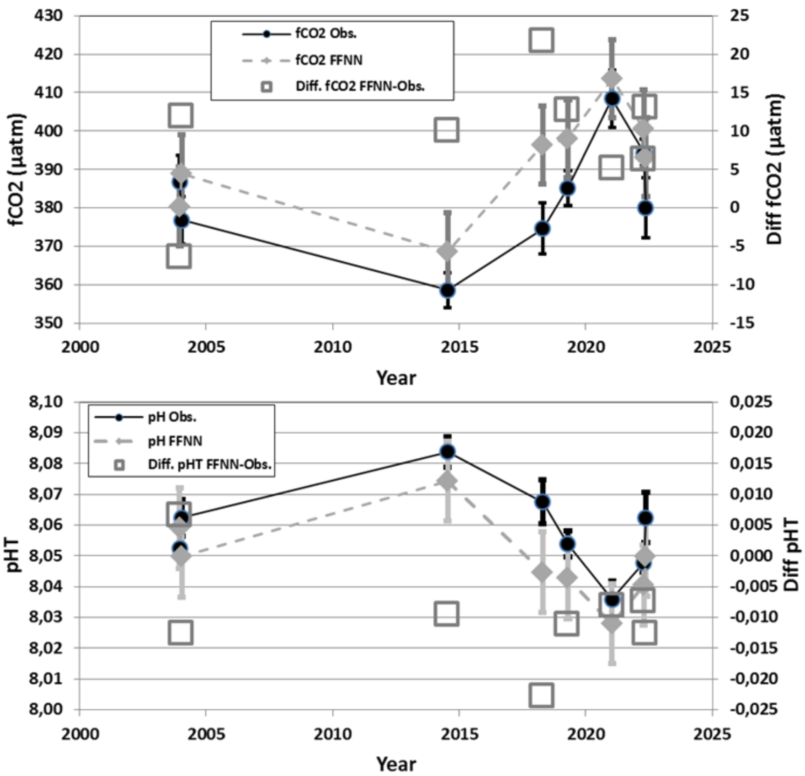


Figure 3: The criteria for selecting these specific periods are not clear, and this choice may introduce bias. The averaging of the 2003, 2004, and 2014 cruises seems to give greater influence to the earlier years. It would be more informative to present the direct observations of each cruise with distinct markers. This approach would facilitate interpretation and enable clearer comparison with FFNN and climatology results. Including error bars would also strengthen the figure.

AR-06: Based on your comment and reviewer 2 we have revised both Figures 3 and 4

Figure 3 revised

"Figure 3: Seasonal cycle of (a) fCO_2 (µatm) and (b) pH_T in the southern Mozambique Channel (24-30°S). Average observations are presented for each cruise (colored circles). The full seasonal cycles are shown for the monthly climatology (reference year 2010, Fay et al, 2024) and for the FFNN model for years 2010 and 2022 with respective error bars."

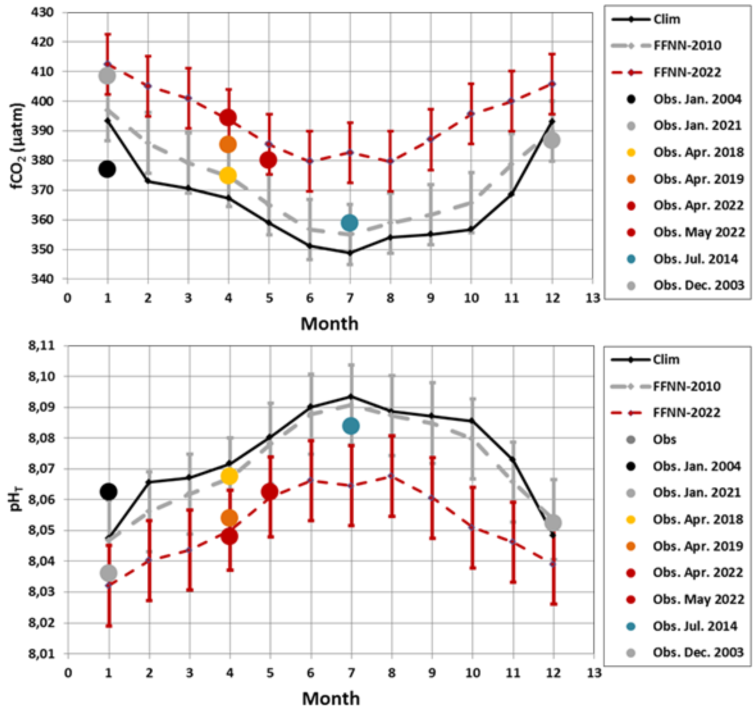
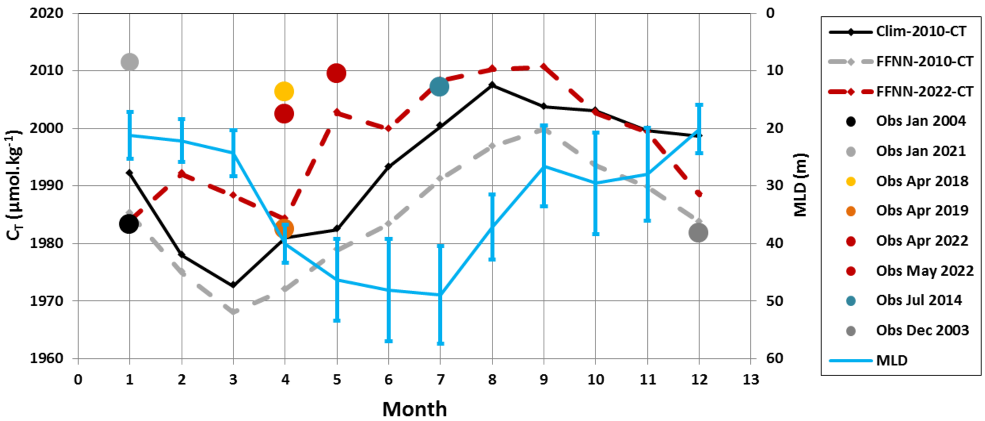


Figure 4 revised

"Figure 4: Seasonal cycle of C_T (µmol/kg) in the southern Mozambique Channel (24-30°S). Average observations are presented for each cruise (colored circles). The full seasonal cycles are shown based on the monthly climatology for a reference year 2010 (Fay et al, 2024) and the FFNN-LSCE model for year 2010 (Chau et al, 2024). The mixed-layer depth (MLD in m, blue line) is averaged in this region (from multi-year reprocessed monthly data, ARMOR3D L4, https://doi.org/10.48670/moi-00052, last access 20/4/2025)."



Section 3.3 does not appear to align well with the overall structure of the paper. Although the main result derived from this section is interesting for the broader discussion, it does not directly fit within the objectives. I therefore suggest moving this section to an Appendix and including the figure as Supplementary Material.

AR-07: We are not sure to understand this comment. Note that as suggested by Reviewer 2 the title of Section 3 has been changed to: "3 Results and discussion"

Section 3.3.2: How were the trends calculated? Were they derived from observational data or from climatology/NN outputs? This is a critical point that needs clarification. For example, between 1963 and 1995 (line 498) a trend is reported, but only two data points exist. In this case, it would be more appropriate to report the change between 1963 and 1995 (-0.040 units) rather than describing it as an interannual trend, which would be biased without intermediate data. The same concern applies to trends reported for 1995–2019 (lines 507–509) and 1995–2022 (lines 514–515). Similarly, in Table 4 the limited number of observations in the second half of the 20th century (only in 1963 and 1995) introduces biases in trend estimation. In addition, the high variability observed since 2018 could influence the calculated trends. Under these circumstances, no interannual or decadal trends can be identified with any statistical significance; instead, it would be more reliable to compare recent values with those from 1995 to estimate the magnitude of change. These changes will require a revision of the main conclusions of the paper, as well as the abstract

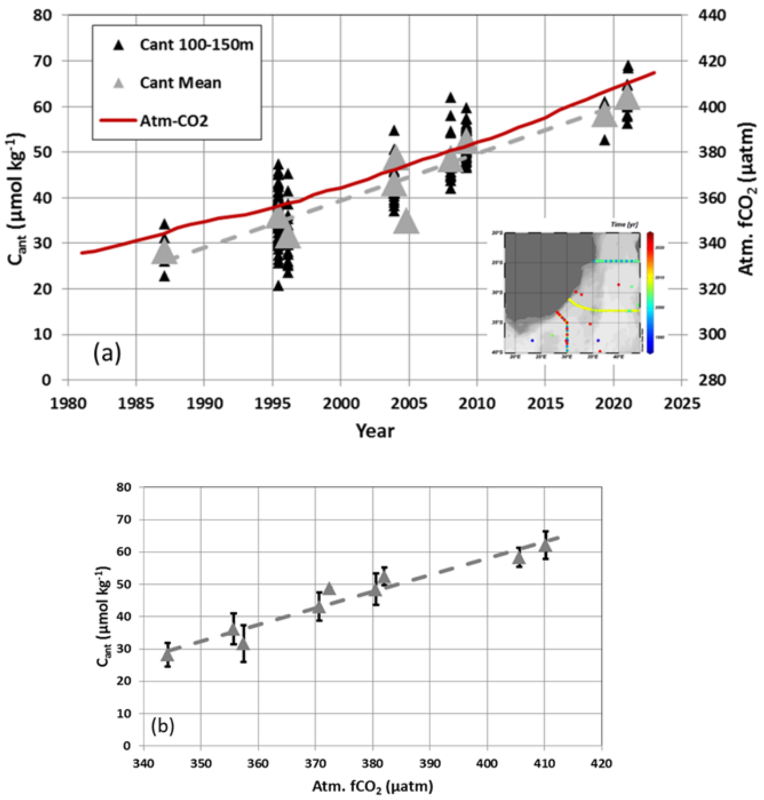
AR-08: We agree with this comment. Text revised on line 498.
AR-08: For the line 507-515 we recall results from Lo Monaco et al 2021) and Chakraborty et al, (2024) No change.
AR-08: This is correct, for the first period, observations are only available in 1963 and 1995. However, we listed all trends deduced from observations or the FFNN model in Table 4. The conclusions of the results for the decadal changes are mainly based on the trends deduced from the FFNN model but we think it is useful to indicate what we learn from observations even if this is only between 2 periods.

Section 3.4 (line 622): The calculation applied in this section may be difficult for readers to follow. The reconstruction of past and future values and the data sources used are not entirely clear. It might be beneficial to expand on the methodological details, either in the methodology section or in a dedicated Appendix, and limit the current section to the discussion of results. Additionally, please clarify the estimation error associated with Eq. 2.

AR-09: The methodology was described in previous work (Metzl et al, 2025b), and we thought it was not useful to recall the details. However, as suggested we have added this information in the Supp. Mat.
The error associated to Eq 2 was somehow indicated on line 625: +0.512 ±0.050 µmol kg^{-1} $µatm^{-1}$
Equation 2 presented the way we calculated C_T for each time step based on the C_{ant} annual rate.

Added in Supp. Mat. new figure:

Figure S8: The relationship between C_{ant} and atmospheric CO_2 used for the reconstruction (Equation 2) was described by Metzl et al (2025b). It was evaluated from the C_{ant} concentrations in subsurface using data in 1987 to 2021 and correlated to the atmospheric CO_2 concentrations. (a) Time-series of anthropogenic CO_2 concentrations (C_{ant}) over 1987-2021 estimated in subsurface (layer 100-150m) from the GLODAP-v2023 data (Lauvset et al, 2024) completed with OISO cruise in 2021 (location of selected stations in the insert map, color code is for year). The figure shows the C_{ant} concentrations calculated for each sample (black) and the C_{ant} averaged in the layer 100-150m for each period (grey triangles). Over the period 1987-2021, the Cant trend is +1.03 ±0.14 µmol kg^{-1} yr^{-1} (dashed grey line). The red curve is the atmospheric fCO_2. (b): same data for C_{ant} versus atmospheric fCO_2 (slope= +0.512 ±0.050 µmol kg^{-1} $µatm^{-1}$).



Minor comments

Lines 41–42: Is this referring to surface measurements? Please clarify.

AR-10: Thank you, yes this is for surface. Lines revised as follows: “the sea surface pH could decrease by 0.4 and aragonite saturation state (Ωar) could be as low as 3 in the tropics by 2100”

Line 47: Replace “fugacity of CO2” with “CO2 fugacity”.

AR-11: Thank you, corrected

Lines 47–50: The inclusion of these trends may appear arbitrary and potentially confuse the reader, since the study region has not yet been specified in the introduction. While the Indian Ocean reference is understandable, the criteria for also including the North Pacific is unclear. As a suggestion, it may be more relevant to mention the decrease in pH using trends from time-series stations (Bates et al., 2014), noting that most are located in the Northern Hemisphere and that knowledge of OA in the Southern Hemisphere remains limited. This could help emphasize the importance of the paper and engage the reader.

AR-12: In the introduction we have selected the range of pH trends previously quantified (slow in the North Pacific and rapid in the Indian Ocean). Here we refer to the results from fCO_2 data (SOCAT), whereas Bates et al (2014) synthetized results derived from A_T-C_T data at several time-series. No change.

Line 49: What does “TS.decade-1” mean? Please use “units decade-1” or simply “decade-1” instead, and apply this consistently throughout.

AR-13: Thank you. We have changed the unit and pH to pH_T in the text, tables and figures.

Line 58: Acceleration with respect to …?

AR-14: corrected as follows: “… it has been shown that the Mozambique Channel experienced an acceleration with respect to the acidification in recent years”.

Line 135: Were only three gases used? Was a 0 ppm gas not included to zero and span the system (Pierrot et al., 2009)? If not, how were the xCO2 measurements corrected?

AR-15: Yes, three gases were used during our cruises. This is what has been done for years during OISO cruises and validated through the quality control in SOCAT. In this manuscript we do not need to detail the instrumental and corrections. No change.

Lines 160–162: Please include statistical information (e.g., RMSE and r2).

AR-16: Thank you, error statistics and R2 added:
A_T ($\mu mol.kg^{-1}$) = 73.841 (± 1.15) * SSS – 291.02 (± 40.4) (n= 548, r^2= 0.88).

Lines 193–194: “For pH, the decrease of -0.005 over three years, i.e., -0.0017 yr-1, is surprisingly close to what is generally observed at global scale and over several decades (-0.017 ± 0.004 per decade).” Please use consistent units when reporting both trends.

AR-17: Thank you. Units changed.

Table 2: Why do the mean SST and SSS values for CLIM-EPARSES fCO2 and CLIM-EPARSES AT-CT not match? They should be identical if only the computation of the carbon system variables differs. The same issue appears in Table 3 with OISO-11 and OISO-31. Please clarify.

AR-18: The mean SST and SSS were evaluated from the data points available for each instrument. For fCO_2 we recorded one data per 5 minutes, for A_T and C_T one data per 20 minutes. This is why we indicated the number of data for each method (294 for fCO2 and 70 for AT-CT). The same applied for Table 3. No Change.

Line 316: Please specify the salinity to which CT is normalized.

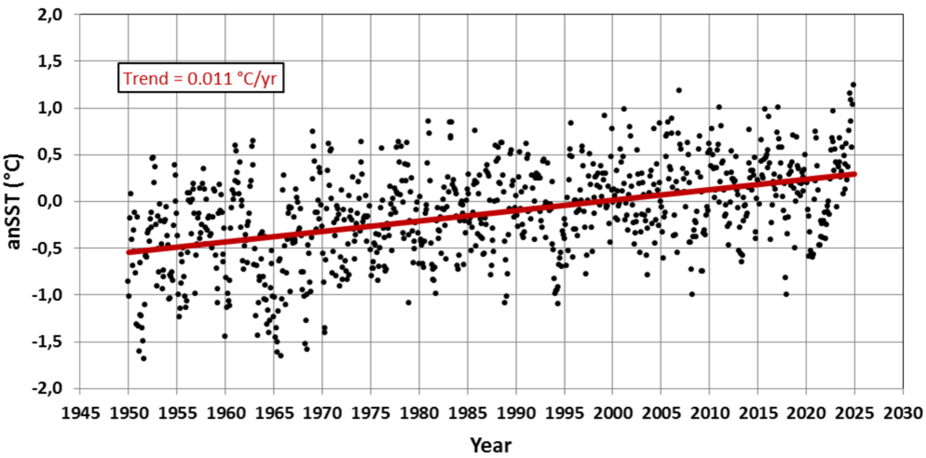
AR-19: C_T is normalized at salinity 35. Information added as follows: ”normalized C_T at salinity 35”

Line 323: Is there a reference supporting the reported trend?

AR-20: Yes, the SST trend of +0.11 ± 0.009 °C per decade since the 1960s was evaluated from data extracted at http://iridl.ldeo.columbia.edu/SOURCES/.NOAA/.NCDC/.ERSST/.version5/.anom/, last access 12 May 2025.

We suggest add a figure in the Supp. Mat.:

Figure R1: Monthly sea surface temperature anomalies (°C) at 25°S-40°E obtained from http://iridl.ldeo.columbia.edu/SOURCES/.NOAA/.NCDC/.ERSST/.version5/.anom/, last access 12 May 2025. The red line is the linear trend of +0.011 °C per year (i.e. +0.11 °C per decade).



Lines 365–366: This statement would be more appropriate in the Results section.

AR-21: Thank you. This was listed in the figure caption: ”CT concentrations are high when MLD is deeper in austral winter.” Now moved in the Result section as follows:
“The progressive C_T increase of about +30 µmol.kg^{-1} from March to August is likely driven by vertical mixing when MLD is deeper in austral winter (Figure 4).”

Line 387: Consider replacing “increased” with “reinforced”.

AR-22: We agree with this suggestion and have corrected: ”The FFNN model also suggests that the sink reinforced over 2016-2021 with a perceptible faster increase of C_T (Figure S3).”

Lines 424–425: How were eddies identified? Were satellite images used, or is there a reference?

Lines 424–425: “We noticed that in 2021, the properties present a high variability along the track linked to the presence of eddies”.

AR-23: As the distribution in 2021 was much more variable than in 2004 (Figure 6), we suggested that the spatial variability in 2021 was linked to the presence of eddies. This was identified from SST and SSS data as well as from reanalysis (Figures R2 and R3, not included in the Manuscript).

Figure R2: Distribution of SST and SSS along the same track in January 2004 (black symbols) and January 2021 (grey symbols). In 2021, the variability of salinity suggested the presence of eddies (see also Figure R3).

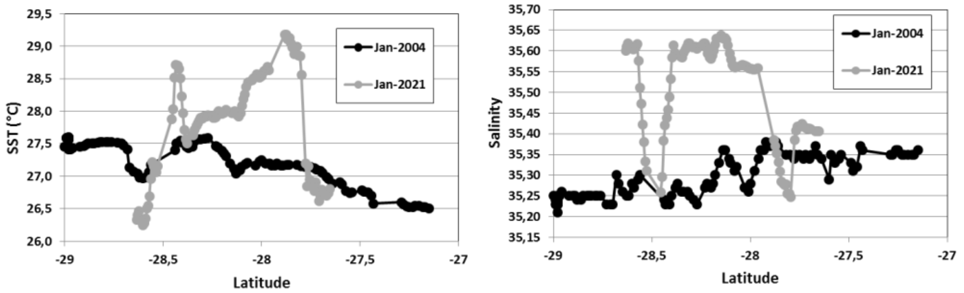
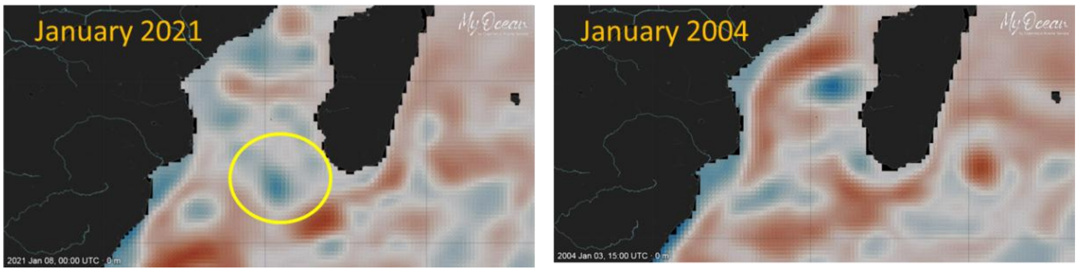


Figure R3: Map of geopotential height in January 2021 (left) and January 2004 (right) highlighting an eddy structure (yellow circle) in 2021. Maps constructed from CMEMS: dataset-armor-3d-rep-monthly (https://doi.org/10.48670/moi-00052), last access 11 Sept. 2025.



Line 428: ΔfCO2 should be defined as the difference between oceanic and atmospheric fCO2 before it is mentioned for the first time.

AR-24: Thank you, ΔfCO2 now defined here for the first time (on line 428): fCO_2^{ocean}-fCO_2^{atm} = ΔfCO2 = -0.04 ±3.11 µatm.

Line 496: What does "difference of ΔfCO2" mean? Please clarify.

AR-25: This was an unclear repetition and we have deleted "difference".
Line 496 revised: "Back in the 1960s, the observations in 1963 indicate that the ocean was a CO2 sink in May (Figure 7a), the value of ΔfCO2 = -32.2 µatm being almost the same as observed in May 2022 (ΔfCO2 = -32.5 µam)."

Line 500: Please specify the units.

AR-26: No unit needed but line 500 "Over 32 years this pH change was driven by the CT increase (effect on pH= -0.045), the AT increase (+0.012) and the warming of 0.95°C (-0.015)."
Revised as follows:
"Over 32 years this pH change was driven by the C_T increase (effect on pH= -0.045), the A_T increase (effect on pH= +0.012) and the warming of 0.95°C (effect on pH=-0.015)."

Line 639: Instead of simply stating "compared well", it would be helpful to report the mean differences between this method and the observations, as well as between this method and the FFNN estimates/climatology data.

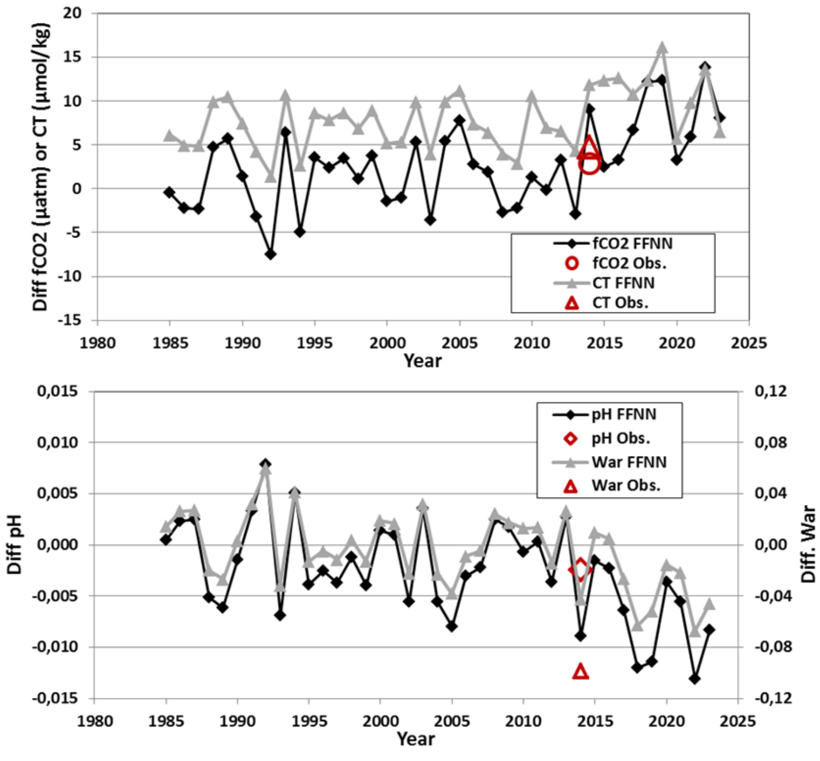
AR-27: Line 639: "The reconstructed C_T, fCO_2, pH and Ωar for August compared well with the observations (in July) and with the FFNN model in August (Figure 8) indicating that the simulation captured the decadal evolution of the properties".

We agree that "compared well" as visualized in Figure 8 need more detail.
We have reported the differences corresponding to the results in Figure 8 (Table R1 and Figure R4) and suggest add the table R1 and Figure R4 in the Supp. Mat.

Table R1: Mean difference between the reconstruction and the FFNN model for August 1985-2022 and with observation in July 2014. SD in brackets.

Method	Year	fCO2 µatm	CT µmol/kg	pH TS	War nu
Sim-FFNN	1985-2022	2.6 (4.9)	7.9 (3.4)	-0.003 (0.005)	-0.005 (0.029)
Sim.-Obs.	2014	2.8	4.8	-0.002	-0.099

Figure R4: Time-series of the difference of (top) oceanic fCO2 and CT concentrations and (bottom) pH and Ωar between the reconstruction using SSP85 scenario and the FFNN-LSCE model over 1985-2023 in August or with observations (July 2014, red). The differences are calculated from data presented in Figure 8.



Lines 679–680: Please provide a reference.

AR-28: Lines 679–680: "Our calculation suggests that for a high emission scenario a risky level for corals ($\Omega ar < 3$) could be reached as soon as year 2034, i.e. in the next 10 years."
We understand that the reviewer asks for a reference regarding the "risky level". As indicated in the manuscript (lines 43 and 562), we refer to Hoegh-Guldberg et al., (2007).
Sentence revised as follows: "Our calculation suggests that for a high emission scenario a risky level for corals ($\Omega ar < 3$, Hoegh-Guldberg et al., 2007) could be reached as soon as year 2034, i.e. in the next 10 years.

Figure 9: Is it possible to add error bars?

AR-29: Figure 9 present the data from the BGC-Argo float select in surface waters (two data point at 7 and 11m) for each period. No need to add error bars on this figure.

;;;;;;; end response to reviewer 1