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Review

Title: Acidification in coastal waters of Adélie Land, Antarctica (1985–2025)

Author(s): Nicolas Metzl et al.

MS No.: egusphere-2026-1664

MS type: Research article

The paper entitled "Acidification in coastal waters of Adélie Land, Antarctica (1985-2025)" uses cruise observations and neural network approaches to estimate the carbonate chemistry in the upper levels of Adélie Land over the last 40 years. It highlights the temporal variability of carbon dioxide fugacity, total dissolved inorganic carbon, total alkalinity, pH on a total scale, anthropogenic carbon, and the saturation state. Furthermore, the authors estimate the variability trends for most of these parameters and extend these trends into the future, correlating the expected saturation states with respect to aragonite with the critical conditions for precipitation associated with the estimated anthropogenic carbon levels over the course of the century.

The paper's approach is innovative for the region and includes new data from summer 2024-2025. Another innovative perspective is the application of neural-network outputs to the carbonate system in high latitudes.

There are a few points in the paper that need to be addressed before it can be properly published:

AR-01: We thank the reviewer for her/his useful questions and positive comments. We reply below (in blue) to each point (recalled in black).

The reproducibility of the neural-network model in the dataset. The steps for validation and testing are lacking, which means that reproducibility is also lacking. This may be justified by the relatively good representation of fCO2 and CT, but not so much for pH and Omega aragonite. Although the provided link guided us to the L4 time series of monthly global reconstructed surface ocean pCO2, air-sea fluxes of CO2, pH, total alkalinity, dissolved inorganic carbon, and the saturation state with respect to calcite and aragonite, I could not identify any validation that would (1) corroborate the potential representation of real carbonate chemistry in the target region, or (2) justify annual variability based solely on data from summer cruises (there is a few years with winter data). I suggest performing comparisons to indicate the representation of the in situ data and the neural network model before expanding it back in time, especially for winter scenarios using the BGC-Argo data. The robustness of the model can be improved by testing more equations for AT reconstructions beyond the global ones mentioned in the paper. There are a few other papers that develop such approaches in the coastal zones surrounding Antarctica, which may or may not be applicable to the Adélie region, but this has not been tested. The point here is that equations derived from global datasets may miss regional features; therefore, comparisons with equations derived in coastal areas around Antarctica are likely to be more representative. I was hoping to see at least one Taylor diagram (https://doi.org/10.1029/2000JD900719) when I read the abstract.

Reviewer: "I could not identify any validation that would (1) corroborate the potential representation of real carbonate chemistry in the target region, or (2) justify annual variability based solely on data from summer cruises (there is a few years with winter data)."

AR-02: Concerning the reproducibility of the neural-network model in the dataset. The FFNN model developed by Chau et al (2024) was validated against several time-series data-sets in the global ocean and coastal zones but, as identified by the reviewer, not specifically in the Adélie Land region investigated here. The aim of our analysis was not to validate the FFNN model (long-term time-series not available in the Adélie Land region), but use this model to complement seasons and years not available from in-situ data and explore the annual CO2 fluxes (Figure 2 in the submitted manuscript).

As the FFNN dataset is publicly available, it has been used for comparison in other regions and periods (e.g. Abe et al, 2026 for AT and CT reconstructions; Da et al, 2025 or Wu et al, 2026b for fluxes in coastal zones including the Southern Ocean; Wu et al, 2026a for pCO2, CT in the China Sea during cyclones; Savelli et al 2026 to validate models; Tan et al, 2025 for pH Time of emergence analysis; Hsiao et al, 2025 in corals reefs; Findlay et al, 2025, 2026 for synthesis and review on acidification; Hunter et al, 2024 for microplastics, pH trend and stress in the SO over 1985-2021; Wu et al, 2024 for the Gulf of Mexico to compare models mean and trends in 2003-2017; Kang et al, 2026 in the Indian for fluxes and CT anomaly). Notice that the FFNN model, as the SNAPO-CO2, SOCAT or GLODAP datasets, is now included in the first synthesis of ocean CO2 datasets (Jiang et al, 2026; https://oceanco2.github.io/co2-products/). We think there is enough information in the manuscript and suggest readers interested to read details of the method in Chau et al (2024) or in Jang et al (2026).

AR-03: Concerning the AT reconstructions, the FFNN model used one relation-ship, LIAR from Carter et al, (2016, 2017). This was indicated line 142 for the BGC-Argo float, but not recalled in "section 2.4" dedicated to inform the use of the CMEMS-LSCE-FFNN model. The method to calculate AT is thus coherent for FFNN and BGC-Argo. Although a full description of the model is presented in Chau et al (2024) we suggest add this information as follows: "To estimate surface ocean CT and pH fields from reconstructed fCO2, the FFNN model use AT derived from the multivariate linear regression model LIAR (Carter et al., 2016; 2017) based on sea surface temperature, salinity, and nutrients concentration, i.e. the same as for BGC-Argo floats."

Reviewer: "The robustness of the model can be improved by testing more equations for AT reconstructions beyond the global ones mentioned in the paper."

AR-04: Thanks for this comment. As suggested by the reviewer we have tested the reconstruction of A_T for FFNN using the relationship described in Eq1 and recalculate C_T and pH. As expected, we notice some differences, larger in austral summer (November-February) and at specific periods (e.g. February 2009, March 2022), but the trends of C_T and pH are identical (Table R1). Figure R1 shows the Ωar calculated with the original AT and AT derived from Eq1.

Table R1: Trend of C_T and pH over 1985-2025 deduced from the FFNN model when using AT from FFNN (LIAR) or AT from Eq1

Period, Method	CT µmol/kg/yr	pH /yr
January Using FFNN LIAR	0.080	-0.000675
January Using AT Eq 1	0.097	-0.000650
August Using FFNN LIAR	0.443	-0.00131
August Using AT Eq 1	0.447	-0.001315

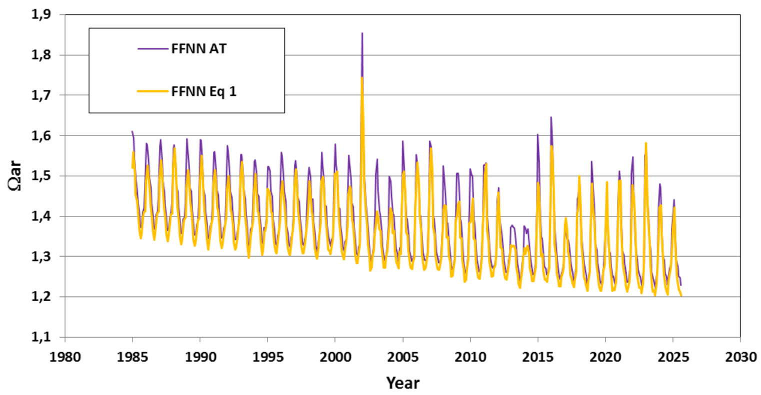

Figure R1: Times series of Ωar from the FFNN model when using original AT (purple) or AT/SSS relationship deduced from equation 1 (orange).

Reviewer: "I was hoping to see at least one Taylor diagram (https://doi.org/10.1029/2000JD900719) when I read the abstract."
AR-05: We are not sure why when reading the abstract a Taylor diagram should be included somewhere in the manuscript. Such Taylor diagram is generally dedicated to compare large data-sets (e.g. CMIP model versus data) at global (e.g. Eyring et al, 2016) or at regional scale (recent paper by Bahamondes et al, 2026).

The hidden mechanisms behind the trends. Most of the trends observed in the paper are posed on two points (e.g. year 1 and year 2), divided by the interval between them (i.e. the inclination from year 1 to year 2 / [year 2 – year 1]). The study area is close to Antarctica, where the Southern Annular Mode (and the El Niño Southern Oscillation, which has a minor impact) and all the impacts stated in the introduction (lines 39 - 41) affect the CO2-carbonate chemistry in the surface layer. Interannual variability is important here, but trends based on extreme values (i.e. the first and last concentrations within the time series) could obscure this important aspect of the carbon cycle. I suggest two options: (i) in situ data supporting deeper reconstructions of long time series (1985 – 2025), which might highlight the role of anthropogenic carbon over the other drivers, especially for more detailed interannual variability; or (ii) combining time scales to deconvolve the trends in more detail, paying special attention to other drivers over time. My criticism here is based on the absence of parameters other than fCO2 from R2R3R4/L'Astrolabe fCO2 showing a more consistent time dependence, whereas the other data does not clearly demonstrate this (see Fig 6a).

Reviewer: "Most of the trends observed in the paper are posed on two points (e.g. year 1 and year 2),"
AR-06: This is correct for winter because data are available only for two years (1996 and 2024, Table 2) but not for summer (28 points were used for the trends).

Reviewer: "The study area is close to Antarctica, where the Southern Annular Mode (and the El Niño Southern Oscillation, which has a minor impact) and all the impacts stated in the introduction (lines 39 - 41) affect the CO2-carbonate chemistry in the surface layer."
AR-07: We agree that the SAM probably impact the CO2 system in the Southern Ocean (e.g. Le Quéré et al, 2007; Metzl, 2009; Lenton et al, 2009; Xue et al, 2018; Metzl et al, 2024), but the response of Antarctic continental shelf regions to climate variability and climate change is poorly understood and the impact of the SAM in coastal AA regions is still an open question (e.g. Tagliabue and Arrigo, 2016; Zhang et al, 2023, Deng et al, 2025; Mo et al 2026). We noticed that when the SAM was positive the CO2 sink was stronger in summer (lines-187-189), but for the pH or Ωar trend we did not identified direct link with the SAM.

Reviewer: “My criticism here is based on the absence of parameters other than fCO_2 from R2R3R4/L’Astrolabe fCO_2 showing a more consistent time dependence, whereas the other data does not clearly demonstrate this (see Fig 6a).”
AR-08: Not sure to understand this comment. Figure 6a and b presents all measured and calculated fCO_2 and CT. This includes fCO_2 and CT measurements from L’Astrolabe in 2025 as indicated in the caption

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General comments:

line 18: standardise the subscript for CO2 and omega aragonite throughout the paper.
AR-09: Thank you, corrected except for acronyms SNAPO-CO2 and CO2sys.

line 21: remove the space before “Here we present …”.
AR-10: Thank you, corrected

line 24: add a space between ± and 0.01, and remove “. “ in the units, both throughout the paper.
AR-11: Thank you, corrected as suggested.

line 38: suggest replacing “offshore ocean” with “open ocean”, as cited in line 43
AR-12: Thank you, corrected as suggested.

lines 44-45: several other papers in the coastal zone of the Northern Antarctic Peninsula that are not cited, but should be. These could be useful for the discussion (especially comparisons between Adélie and other sensitive areas to climate change) and cover aspects of CO_2 fluxes (Monteiro et al 2020, https://doi.org/10.1038/s41598-020-71814-0) and anthropogenic carbon (Kerr et al 2018, https://doi.org/10.1016/j.dsr2.2017.07.007). These are just a few examples.
AR-13: Recall lines 44-45: “Apart from the Drake Passage to the north of the Antarctic Peninsula (Takahashi et al, 2014; Hauri et al 2015; Munro et al, 2015) no long term time-series have been used to evaluate OA in coastal Antarctic zones.” Thank you to recall theses references. We are aware the suggested papers that used cruise data in 2015 to estimate Cant (Kerr et al 2018) or used SOCAT pCO_2 data to evaluate the variability over 1999-2017 (Monteiro et al, 2020) in the coastal Antarctic Peninsula. In the introduction we refer to studies that were based on long term observations of AT CT. No change in the introduction but, as suggested, we add the references listed by the reviewer in the discussion as follows:

In section 3.1: “This demonstrates that biology drives most spatial and temporal variability making CT or pHT trend detection difficult in Antarctic coastal summer data (Monteiro et al, 2020; Metzl et al, 2025a).”

In section 3.3: “To estimate the anthropogenic CO_2 concentrations (Cant) in the water column we used the TrOCA method developed by Touratier et al. (2007). This method was successfully used in the southern Ocean (Shadwick et al, 2014; Kerr et al, 2018; Mahieu et al., 2020; Zhang et al, 2023; Metzl et al, 2024b).”

In section 3.4: “Low Ωar of 1.03 ± 0.10 has been observed in summer 2015 in the Gerlache Strait, Antarctic Peninsula (Kerr et al, 2018).”

line 47: add a space between ± and std in the first trend.
AR-14: Thank you, all corrected in the paper

line 123: add space between ± and 2
AR-15: Thank you, all corrected in the paper

line 123: I did not identify the description of the flags used. Either describe it or reference it in the bridging papers and method.
AR-16: Thank you. Recall line 123: "The precision of the fCO_2 data are estimated to be ±2 µatm, with cruises flagged B in SOCAT (Table S1)." Here we indicated that the unpublished cruises in 2024-2025 received a flag B during the secondary QC SOCAT process (Flags listed in table S1). Flag B corresponds to data validated with an uncertainty of calculated fCO_2 at SST is less than 2 µatm. For readers interested to know the QC details, we suggest to add reference to the SOCAT Quality Control Cookbook as follows: "The precision of the fCO_2 data are estimated to be ±2 µatm, with cruises flagged B in SOCAT (Table S1, Gkritzalis et al, 2024)". Notice that the 2024-2025 cruises were submitted to SOCAT and now published in version v2026 released last June (Bakker et al, 2026).

lines 131-133: To standardise quality control flag system, mentioned the one used for AT and CT here (correct me if I am wrong, but I believe you are referring to the one by Boyer, T. P., Baranova, O. K., Coleman, C., Garcia, H. E., Grodsky, A., Locarnini, R. A., et al. (2018), World Ocean Database 2018).
AR-17: Thank you. Recall line 131-133: "A total of 31 samples were analyzed for the R2 cruise. Two anomalous values were identified for both AT and CT either due to a sampling or analytical issues and were not considered further (Supp. Mat. Figure S2)." The reviewer is correct we used WOCE Flags in the SNAPO-CO_2 dataset. Revised as follows: "Two anomalous values were identified for both AT and CT (WOCE flag 4 assigned) either due to a sampling or analytical issues and were not considered further (Supp. Mat. Figure S2)."

line 134: remove "." between µmol and kg.
AR-18: Thank you corrected

line 139: O2 with subscript 2.
AR-19: Thank you corrected

line 144: remove "." Between µmol and kg. Please correct it throughout the manuscript; there are many of these typos.
AR-20: Thank you corrected in the MS

line 151: Why did the authors use the dissociation constants of Lueker et al (2000) instead of those of Goyet and Poisson (1989), the latter of which are known to perform well in high latitudes? If based on Dickson (2007) and Sulpis et al (2020), then cite them, please.
AR-21: For coherence, we used constants of Lueker et al (2000) because it is also used to derive fCO_2 and CT from BGC-Argo data and used in the FFNN model to calculate pH and CT. For this reply, we compared the results using K1, K2 form Goyet and Poisson (1989) or Lueker et al (2000). Results are presented in Table R2 for a cruise conducted in 2003. The calculated fCO_2, pH and Ωar are similar. As we are interested in evaluating the trends, the results would be the same although the values are slightly different and within the errors in calculation (2.5 µatm).

Table R2: Mean values and standard deviations (in bracket) of fCO_2, pH and Ωar calculated using 79 AT and CT data south of 60°S in 2003. Here we compare the calculation using K1, K2 from Goyet and Poisson (1989) or Lueker et al (2000).

	fCO2 µatm	pH TS	Ωar
GP 1989	373.6 (12.2)	8.060 (0.013)	1.532 (0.066)
Lueker 2000	371.1 (11.1)	8.062 (0,012)	1.526 (0,064)

lines 163-165: beyond the open-ocean AT-salinity equations, there are a few others for coastal regions in Antarctica. Have the authors compared these, too? It would be useful to test the open-ocean and coastal region equations and plot the results as a Taylor diagram (https://doi.org/10.1029/2000JD900719).
AR-22: Recall line 163-165: "This relation compared well with those previously estimated in the southern ocean (Lee et al 2006; Shadwick et al, 2014; Stark et al, 2018) as well as when derived from GLODAP data only (Figure S4)." As shown in figure S4 the four selected relations lead to the same AT reconstruction. As an example, the figure R2 below presents the Ωar values calculated using different AT/S relations versus Ωar from measured AT CT data. Using any of this relationship would lead to similar trends of pH and Ωar we are interested with. The same is observed when selecting data south of 63°S as suggested by reviewer 2. No change.

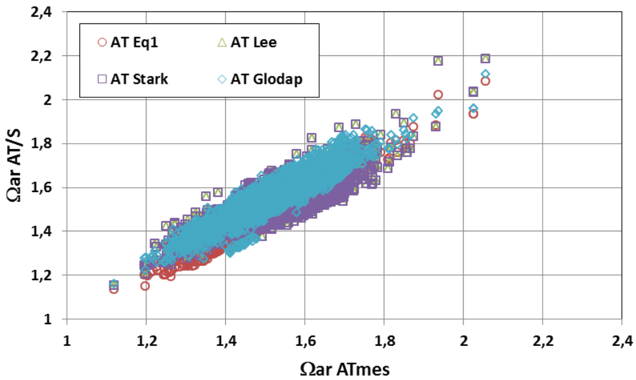

Figure R2: Ωar calculated using four AT/S relationships versus Ωar calculated from measured AT-CT for 1915 data in the region south of 60°S for the period 1994-2018.

line 167: I am sceptical about how representative this neural network might be of the region. I would suggest adding a short paragraph comparing the in situ data from summer 2024-2025 with neural networks from summer 2024-2025, similar to the comparison between lines 211 and 220. Previous cruises could be included in this comparison (those in Table S1). The paragraph between lines 177 – 192 could be modified to be more method-like than result-like. Additionally, the paper lacks a discussion on modes of climate variability in terms of their regional impact on circulation and biological activity. This is important because it can influence CO2 emission and uptake, as well as

long-term trends. Incidentally, sessions “2.2” and “2.4” could be reorganised for easier reading. Example: I think BCG-Argo could have its own session.

AR-23: The reviewer is skeptical about how representative this neural network might be of the region. As indicated above (AR-R02), the FFNN model was validated against several time-series data-sets in the global ocean and coastal zones but not specifically in the Adélie Land region investigated here. The aim of our analysis was not to validate the FFNN model (long-term time-series not available in Adélie Land), but use this model to complement seasons and years not available from in-situ data and explore the annual fluxes (Figure 2 in the submitted manuscript). A detail comparison between FFNN model and data was shown for the period 2000-2006 (Figure S6) to highlight the results when biological activity was pronounced and uncertainty when trying to evaluate the trends in summer. As suggested by the reviewer we have compared the in situ data in 2024-2025 with the FFNN. The results for both measured and calculated values are presented in figure R3.

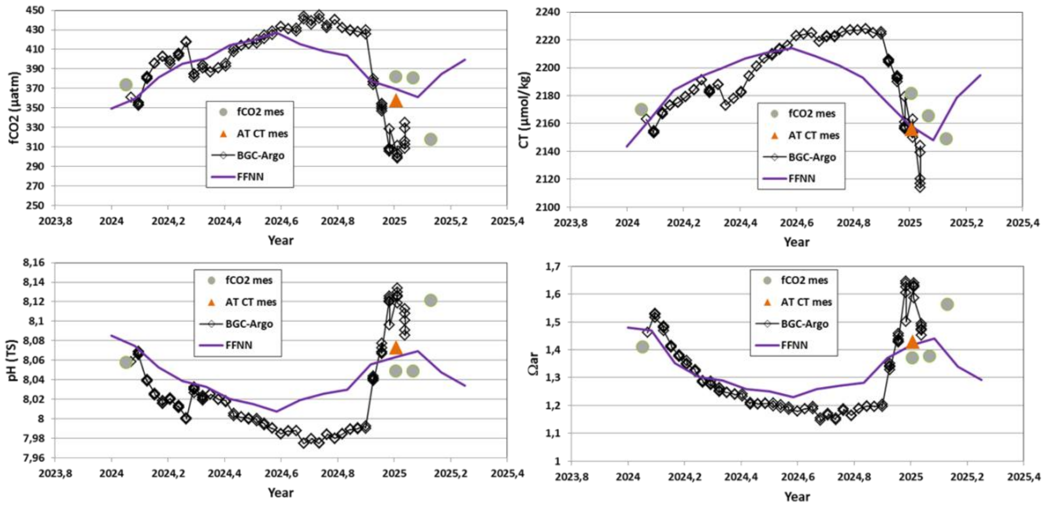

Figure R3: Time-series of (a) sea surface fCO2 (µatm), (b) CT (µmol/g), (c) pH and (d) Ωar observed south of 64°S in 2024-2025 based on measured fCO2 (grey circles) , BGC-Argo float pH data (open diamonds) and measured AT CT (orange triangle). Also shown are the results from the FFNN model (purple line).

Reviewer: “Additionally, the paper lacks a discussion on modes of climate variability in terms of their regional impact on circulation and biological activity.”

AR-24: The impact of climate variability on circulation or biological activity is not discussed in detail because there is no data to fully quantify the processes. We somehow opened new question regarding the inter-annual to decadal variability observed for the trends and air-sea CO2 fluxes. For example, on lines 186-191 we discussed the possible link between the fluxes and SAM index. On lines 292-296 we discussed the observed 2013 anomaly, probably related to change in circulation, here specifically a change in upwelling and the intrusion of deep water in subsurface. This is shortly recalled in the summary, suggesting new specific studies should be conducted to better understand and quantify the variability of the CO2 system in this region, including challenges for ocean biogeochemical modeling.

Correct me if I am wrong. CO2Sys and LIAR were used to estimate AT data, which was then used to estimate pH and omega (lines 149 - 150). If so, how did you propagate the errors of the different estimates into the pH and omega outputs?

AR-25: Recall lines 149-150: “From fCO2 or AT and CT data, we calculated pHT (pH at total scale) and the saturation states of aragonite and calcite (Ωar, Ωca). “ As indicated in the manuscript this depends on the data used. For fCO2 underway measurements we used fCO2 and AT/S relationship to calculate CT, pH and Ω. For discrete AT and CT measurements we used AT and CT to calculate fCO2, pH and Ω. For BGC-Argo we used pH data and AT from LIAR to calculate other properties (fCO2, CT and Ω). Finally, the FFNN model used the same calculation (same constants) to derive CT, pH and Ω from fCO2 and AT (LIAR). As shown in Figure 5 and tables, errors are identified, either linked to the measurements, the calculations or averages in the selected region. We have also taken into account all errors associated to the calculations on line 230-231: “Accounting for AT and CT measurements uncertainty (± 4 µmol.kg-1, Lauvset et al, 2024) the pHT decrease ranges from -0.021 to -0.035 per decade”.

lines 227 – 228: From this line onwards, I have seen the trend estimates as the slope/difference between two points divided by the time interval. Based on the high interannual variability in Antarctica, I am a bit sceptical of this approach. In winter, with only a few in situ measurements, assumptions are required to do so. However, in the summer, several cruises could provide the interannual variability required to determine the long-term trend. Modes of climate variability and signal decomposition could support interannual controls, improving the interpretation and impact of the paper. The same points are valid for lines 242 – 247.

AR-26: Recall lines 227-228: “A pHT decrease of 0.0781 in 28 years corresponds to a trend of -0.028 per decade. “ Recall lines 242-247: ”The average fCO2 and CT data present higher values in recent years (Figure 6). The fCO2 average for January 2025 (mean = 382.3 µatm) was much higher than the first data in January 1984 (fCO2 = 317.5 µatm). The difference of 64.8 µatm would translate into a trend of +1.58 µatm.yr-1 with the atmospheric trend of +1.98 ppm.yr-1 for the same period. In January 2025, the CT measurements (CT normalized at salinity 34, N-CT = 2182 µmol.kg-1) increased by +10 µmol.kg-1 compared to January 1994 (N-CT = 2172 µmol.kg-1) corresponding to a trend of +0.32 µmol.kg-1.yr-1. ” The reviewer is correct, in these sections we translated the trends based on 2 points for winter data and 2 points for summer (imagine one has only the first in 1984 and the last new data in 2025). This was to introduce that (i) for winter the use of FFNN model for all years is indeed relevant and (ii) for summer, given the high variability, the trends are uncertain (Table 1). Consequently, to investigate the changes in the water column (Figure 8) and the future changes (Figure 9, 10) we selected winter data for the reconstruction and projection.

lines 238 – 241: This should be method.

AR-27: Recall lines 238-241: “To evaluate trends we averaged data from each cruise around 140°E and south of 65°S. In this region fCO2 data from SOCAT are available since 1984 and AT and CT data from GLODAP and SNAPO-CO2 since 1994. The FFNN model starts in 1985 and was constrained by satellite Chl-a data since 1998, and by climatological Chl-a before then.“ We prefer to keep this short introduction for this section to recall the periods for different datasets. No change.

lines 249 – 251: data for Chl-a in 2002 support this interpretation (S6), but it is not valid for 2015. Data for Chl-a 2015 in Fig.S7, please reference this.

AR-28: Recall lines 249-251: “Indeed, the time series of fCO2 and CT present large inter-annual variability in summer (Figure 6) which is particularly apparent in 2002 and 2015 and was clearly linked to biological activity (Figure S6).” Thank you, corrected as follows: “Indeed, the time series of fCO2 and CT present large inter-annual variability in summer (Figure 6) which is particularly apparent in 2002 and 2015 and was clearly linked to biological activity (Figure S6 and S7).”

lines 255 – 257 & 331 - 335: How does this trend compare with that in other regions surrounding Antarctica?

AR-29: Recall lines 255-257:”A simple linear projection of the Ωar trend in winter from the FFNN model (-0.0035 ± 0.0001 .yr-1) suggests that a value of 1.2 would be reached in 2032 and saturation (Ωar=1) in 2090.”

Recall lines 331-335:” Based on this estimation, we conclude that the anthropogenic CO2 uptake represents 70% of the CT increase over 1985-2025. This also drives the decrease of pHT and Ωar (Figures 10 c,d). Projecting the trends from the FFNN model and the reconstruction, a value of 1.2 for Ωar would be reached in year 2032 from the FFNN model against 2034 for the estimation based on Eq. 5. If correct, this would lead to “severe shell dissolution” for pteropods (Bednaršek et al, 2019).”

Thank you for the question. It is not easy to compare our results with other regions around Antarctica. Most studies present projections at basin scale (using models), not at local scale. This is why we refer to Jiang et al (2023) on lines 349-350: “This is much sooner than deduced in the

Southern Ocean from the Coupled Model Intercomparison Project Phase 6 (CMIP6) global models (Ωar = 1.04 in 2080, Table S9 in Jiang et al, 2023).

As suggested, we have explored the observations in regions surrounding Antarctica. Here we used the GLODAPv2023 dataset (Lauvset et al, 2023). Figure R4 shows the locations of data with Ωar < 1.3 in surface waters. Only 2 locations experienced Ωar < 1.25 (at 139.85°E/65.53°S in Jan 2008 and at 0°E/68°S in Dec 2014). We suggest add reference to Kerr et al (2018) as follows: "Low Ωar of 1.03 ± 0.10 has been observed in summer 2015 in the Gerlache Strait, Antarctic Peninsula (Kerr et al, 2018)."

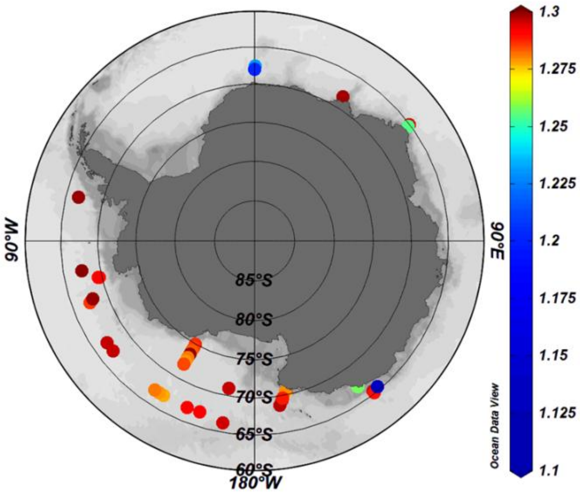

Figure R4: Location and value of Ωar in surface waters where Ωar is < 1.3 (data from GLODAPv2023 dataset (Lauvset et al, 2023). Blue dots for Ωar < 1.25.

lines 277 – 278: Do the mixing and Redfield ratios in this region support the application of this method in the region? What is the error associated with such an estimate?

AR-30: Recall lines 277-278: "To estimate the anthropogenic CO_2 concentrations (Cant) in the water column we used the TrOCA method developed by Touratier et al. (2007)." Yes, the TrOCA method is adapted as long as we select the data below the surface layer as explained in the manuscript. We selected three layers, 100-150 m, 150-200 m and 200-250 m (Figure 9). The first layer (100-150m) is below the maximum of Chl-a (productive layer) identified in this region as presented in Figure S11. For this reply we show examples of Cant profiles estimated in winter 1996 and summer 1995, 2023 (Figure R5). The Cant is homogeneous in the mixed-layer during winter but highly variable in summer. Therefore, to derive the trend of Cant, we selected data below 100m and compare results in three layers (Figure 9 and S12). The error associated to TrOCA of 6.25 µmol/kg was given by Touratier et al (2007). The lowest Cant value evaluated in our region is +15 µmol/kg (Figure 9 and line 282).

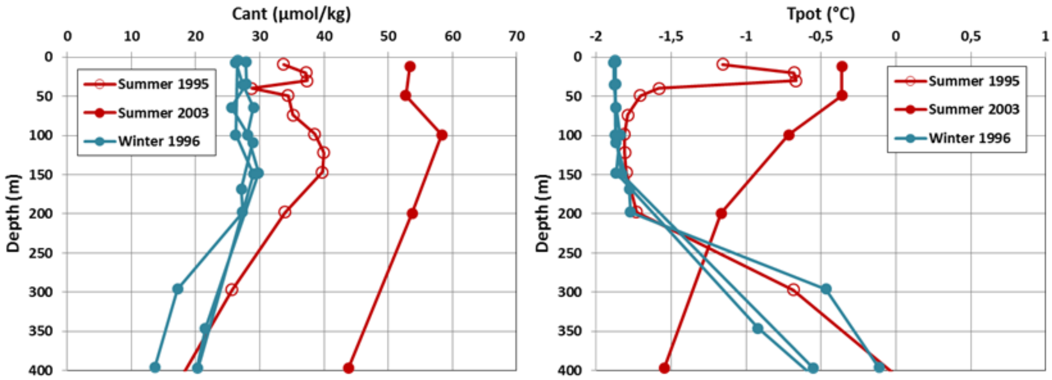

Figure R5: Profiles of Cant and potential temperature around 65°S/140E. In winter the Cant concentration is homogeneous in the mixed-layer whereas in summer Cant is not well estimated mainly due to biological activity.

lines 373 – 374: What is the relative contribution of anthropogenic carbon uptake versus CDW upwelled during winter to the impact on CT in the study area? Other regions (e.g., Monteiro et al 2020) have suggested more intense CDW intrusions in the Northern Antarctic Peninsula. Could this be experienced in Adélie Land? Could the sea ice block sea-atmosphere interactions during winter and freshening (+ biological activity) during summer, spiking carbon uptake? I think this deeper discussion would be essential to improve impact.

AR-31: Recall lines 373-374:" The winter CT increase from 1996 to 2024 translates into a signal of anthropogenic CO2 uptake." This is an important point regarding our knowledge (or better un-knowledge) of the processes that drive the CT and Ωar temporal changes (also commented by reviewer 2). Recall line 331-332:"Based on this estimation, we conclude that the anthropogenic CO2 uptake represents 70% of the CT increase over 1985-2025." The value of 70% was a fixed value from the average of the Cant trends. Taking into account the uncertainties listed in table 2, we prefer to revise as follows: "Based on this estimation, we conclude that the anthropogenic CO2 uptake is responsible for 64% to 84% of the CT increase over 1985-2025."

Unfortunately we have no data to quantify the remnant % and help to interpret the processes. We discussed the intrusion of CDW when explaining the 2003 anomaly (lines 293-296) but for long-term change we have not yet the full story including quantification of CDW intrusion as discussed by Monterio et al (2020) who concluded that in the Gerlache Strait the variability of the CO2 flux in summer 1999-2017 was mainly modulated by phytoplankton activity and likely upwelling processes (but not quantified). Quantifying the variations of UCDW/CDW intrusion in surface layers and how this changed the distribution of CT or pCO2 in surface or subsurface remains complex (Olivier and Haumann, 2025). In our analysis, a possibility is the increase of the mixed-layer depth related to the cooling observed in this region (Figure R5) but how this is translated in CT change is beyond the scope of our analysis.

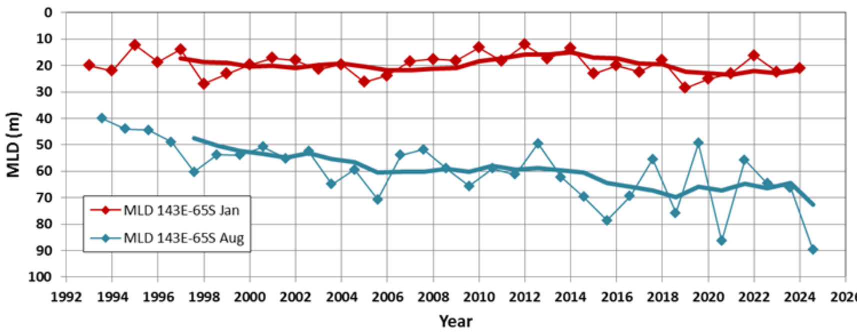

Figure R5: Mixed-layer depth derived from an ocean model (CMEMS GLORYS12V1 product, https://doi.org/10.48670/moi-00021). In summer the MLD is stable whereas in winter the model suggests an increase, on average around 60m in 2004-2014 and 70m in 2015-2024.

AR-32: Concerning the last question (impact of sea-ice), we identified a sudden change of the fluxes after 2016 (Figure 2) that might be related to the decrease of sea-ice observed since 2015. This is indicated in the manuscript (lines 178-179) and we leave the complete CO2 flux/sea-ice story in coastal regions for another analysis, hopefully using all data available around Antarctica to get a global view as was recently explored in the open ocean (Olivier and Haumann, 2025).

Fig 1. Suggest adding isobaths to mark the bathymetry (200 m and 1000 m, for instance). This would improve visualisation.

AR-33: Figures 1a and 1b present all the data available in this region with color code for year and grey scale for bathymetry. We think adding isobaths could be very busy. No change.

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