Reply for the manuscript: egusphere-2026-1664
Title: Acidification in coastal waters of Adélie Land, Antarctica (1985–2025)

In black, the comments listed on-line from the associate editor and from reviewer 2; reviewer 1 is satisfied.
In blue our responses

;;;;;;;; comment from editor
Public justification (visible to the public if the article is accepted and published):
The same two reviewers as before have assessed your manuscript. While reviewer 1 is now satisfied, reviewer 2 indicates that not all reviewer comments have been addressed to the same extent. I agree with the second reviewer in that certain comments were not sufficiently addressed. Specifically, the level of detail of the FFNN method which is still deemed insufficient by reviewer 2; the examples where points were addressed in the response to reviewers but not in the altered manuscript; and some concerns related to uncertainty and error propagation. I should add that I do think that the information previously included in section 2.4 is now properly included in section 3.1. I invite the authors to address the above points as well as the other comments of reviewer 2, after which I will take a decision on the manuscript.

ARR-00: Thank you for your comments and suggestions. We have replied to the comments recalled by reviewer and revised the manuscript accordingly. Concerning the detail of the FFNN method, we are not sure what should add as a dedicated section was included in the revision. We have revised this section, changed the title and add new references.

For clarity, in our present response and for some new comments we recalled the original comment from the reviewers as well as our first replies

;;;;;;;; comment from reviews

This is a rereview of the revised manuscript by Metzl et al. The manuscript was found to be acceptable by both referees, however, some modifications were found to be necessary. In some cases, the authors did follow the referees, but in other cases they did not. In several cases did the authors explain their view with an answer to the referees, but not with text pieces for the manuscript. This is exactly why the referees mention those issues, so a modification of the text would be beneficial.

ARR-01: Thank you, we have replied to each comment below and hope the revised the manuscript now follows what was asked by the reviewer.

For clarity, in our present response and for some new comments we recalled the original comment from the reviewers (1 and 2) as well as our first replies

At some places still Shadwick spelled wrong

ARR-02: Thank you. The name was checked in the revision and corrected but not all. We checked again and now all corrected.

Table 2: psu is not a unit. Salinity is dimensionless, so no unit at all should be used. In the methods section it should be mentioned that salinity is given on the practical salinity scale.

ARR-03: Thank you. Corrected as well as in the methods section with reference to UNESCO (1981) Reference added: UNESCO, Tenth report of the Joint Panel on Oceanographic Tables and Standards. UNESCO Technical Papers in Marine Science. 1981.

Answer to referee #1: "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)." I am not satisfied with this answer. This is exactly what was asked from the authors, namely to explain a little bit more about this. Even if the model has been used in more cases, it is still difficult to find out what has been done if one is not familiar with the model. In a paper everything should be comprehensible.

ARR-04: Although the method has been described in previous studies, we accepted the first recommendation from the reviewer and added information to describe the neural network model in a dedicated section 2.4 in the Manuscript:

"2.4. CMEMS-LSCE-FFNN model

We used the results from an ensemble of feed-forward neural networks (CMEMS-LSCE-FFNN or FFNN for simplicity, Chau et al, 2024) to estimate fCO2, AT and CT for seasons when direct observations were not available. The FFNN model composes monthly surface ocean global fields of surface ocean fCO2 at 0.25°x0.25° resolution based on the SOCAT gridded data. Sea-surface fCO2 values were regressed against a set of predictors with non-linear functions (feed-forward neural network models). The predictors include sea-surface height, sea surface temperature, sea surface salinity, mixed-layer depth, chlorophyll-a, atmospheric CO2 and geographical coordinates (longitudes and latitudes). The reconstructed fCO2 field for 1985-2025 was used to derive air-sea CO2 fluxes, as well as together with AT obtained by a multi-linear regression approach, monthly surface CT, pHT and aragonite and calcite saturation states. To estimate surface ocean CT and pH fields from reconstructed fCO2, AT was 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. When comparing the CT or pH trends from the model based on AT derived from LIAR or from Eq. 1 we obtained the same results (Supplementary Material Table S3). The multi-year reconstruction and the near-real time prediction, including uncertainty estimates, are available under the DOI https://doi.org/10.48670/moi-00047 (last access, 9 August 2026)."

To complete this section, we have revised as follows specifying the full predictors used and NRT model with new references (Takahashi et al., 2009; Garcia et al, 2019; Chau et al, 2022, 2024b).We also changed the title of this section.

2.4. Observation-based reconstructions of surface ocean carbonate system variables

Output from CMEMS-LSCE-FFNN, an ensemble-based feed-forward neural network (CMEMS-LSCE-FFNN or FFNN for simplicity, Chau et al, 2024a) at global scale and 0.25°x0.25° resolution provided fCO_2 for seasons when direct observations were not available. Precisely, an ensemble of 100 models was trained on randomly subsampled SOCAT and predictor data with a ratio of 2:1 for training and validation. The approach uses a 3-month gliding window and the month of reconstruction is excluded from the training data (p-leave out). The means (μ) and standard deviations (σ) are computed from the 100-member ensembles and represent the model best estimate and associated uncertainty. The predictors include sea-surface height, sea surface temperature, sea surface salinity, mixed-layer depth, chlorophyll-a, atmospheric CO_2 and geographical coordinates (longitudes and latitudes) as well as pCO_2 from the climatology (Takahashi et al, 2009). The model uses fCO_2 data from SOCATv2025 (Bakker et al, 2025) that covers January 1985 to December 2024, while 2025 is produced in near-real-time (NRT) prediction mode (Chau et al, 2024b). The reconstructed fCO_2 field

for 1985-2025 was used to derive air-sea CO_2 fluxes. Time and space varying surface total alkalinity fields, A_T, were derived from the multivariate linear regression model LIAR (Carter et al., 2016; 2017) based on sea surface temperature, salinity, and nutrients concentration (nitrate, silicate, from WOA18, Garcia et al, 2019), i.e. the same as for BGC-Argo floats. Surface ocean pH and C_T were obtained from reconstructed fCO_2 and A_T, temperature, salinity, and nutrients through the speciation software CO2sys. Uncertainty of pH, A_T and C_T is derived from LIAR and CO2sys error propagation (Orr et al. 2018) at each grid cell and for each time step. For the in-depth evaluation of fCO_2, pH, A_T and C_T please refer to Chau et al. (2022, 2024a). Note the no specific tuning was employed for the region under study. When comparing the C_T or pH trends from the model based on A_T derived from LIAR or from Eq. 1 we obtained the same results (Supplementary Material Table S3). The multi-year reconstruction and the near-real time prediction, including uncertainty estimates, are available under the DOI https://doi.org/10.48670/moi-00047 (last access, 9 August 2026).

Reference added:

Chau, T. T. T., Gehlen, M., and Chevallier, F.: A seamless ensemble-based reconstruction of surface ocean pCO2 and air–sea CO2 fluxes over the global coastal and open oceans, Biogeosciences, 19, 1087–1109, https://doi.org/10.5194/bg-19-1087-2022, 2022.

Chau, T.-T.-T., Chevallier, F., and Gehlen, M.. Global analysis of surface ocean CO2 fugacity and air-sea fluxes with low latency. Geophysical Research Letters, 51, e2023GL106670. https://doi.org/10.1029/2023GL106670, 2024b.

Garcia, H. E., Weathers, K. W., Paver, C. R., Smolyar, I., Boyer, T. P., Locarnini, R. A., Zweng, M. M., Mishonov, A. V., Baranova, O. K., Seidov, D., and Reagan, J. R.: World Ocean Atlas 2018. Vol. 4: Dissolved Inorganic Nutrients (phosphate, nitrate and nitrate+nitrite, silicate). A. Mishonov Technical Editor, NOAA Atlas NESDIS 84, 35 pp., https://archimer.ifremer.fr/doc/00651/76336/ (last access: 30 July 2022), 2019.

Takahashi, T., Sutherland, S. C., Wanninkhof, R., Sweeney, C., Feely, R. A., Chipman, D. W., Hales, B., Friederich, G., Chavez, F., Sabine, C., Watson, A., Bakker, D. C., Schuster, U., Metzl, N., Yoshikawa-Inoue, H., Ishii, M., Midorikawa, T., Nojiri, Y., Körtzinger, A., Steinhoff, T., Hoppema, M., Olafsson, J., Arnarson, T. S., Tilbrook, B., Johannessen, T., Olsen, A., Bellerby, R., Wong, C., Delille, B., Bates, N., and de Baar, H. J.: Climatological mean and decadal change in surface ocean pCO2, and net sea–air CO2 flux over the global oceans, Deep-Sea Res. Pt. II, 56, 554–577, https://doi.org/10.1016/j.dsr2.2008.12.009, 2009.

From referee #1
Reviewer: “Most of the trends observed in the paper are posed on two points (e.g. year 1 and year2),”
“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).”
This makes the winter trends particularly uncertain. There should be more attention to this in the manuscript.

ARR-05: As in our first reply, we agree with the reviewer that in winter only 2 points are available. This was stated in the manuscript and the results listed for the two periods, 1996 and 2024, indicating significant changes over 28 years (fCO2, CT and pH, and War).

We indicated (lines 340-346 in the revision) that the trend of pH (taking into account the errors) ranges from -0.021 to -0.035 per decade. This is compared with previous work (Pardo et al) and we noticed that the “rapid pHT decrease requires confirmation with longer time series”.

To extend our analysis and project the future changes (section 3.5) we cannot use the trend from observations and we used the results in winter from the FFNN model to compare with the projection (for two emission scenarios).

AR-23 by the authors: It is quite useful that the comparison between the model and the locally measured data was made. However, it does not become clear what the authors did with this new knowledge. This should somehow find its way into the manuscript.

ARR-06: We thank the reviewer for the comment. As noticed in our first reply we have added a new figure in the Supplementary Material with the FFNN results included for comparison (Figure S6 in the revision). Line 327-328: "The FFNN model also produced a maximum of fCO2 in winter (fCO2 = 427 µatm in August 2024, Figure S6)". Figure S6 shows that the FFNN model reproduces quite well the Ωar seasonality observed from the BGC-Argo in 2024-2025 (high Ωar in summer, low Ωar in winter) but we did not add this information in the main text as we focused on the winter season to reproduced recent and future changes (sections 3.5); this is why the winter values from the FFNN model are presented in Figure 9, 10 and 12.

AR-25 by the authors: I think this answer is not satisfying. Something about errors is written in the manuscript but the question by referee #1 was not answered, i.e., how was the error propagation done?

ARR-07: Recall comment from reviewer: "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?"

We are not sure how to reply satisfactory. The reviewer indicated that "CO2Sys and LIAR were used to estimate AT data". As CO2sys is NOT used to calculate AT, to clarify the reviewer's comment, in our first reply we recalled all pH and omega calculations using either underway fCO2 or discrete AT and CT measurements. The errors associated to these calculations are taken into account in CO2sys_v2.5 program (Orr et al, 2018)

AR-27 by the authors: I can imagine that some text explaining the evaluation of trends is mentioned here. However, it would be good to have a general text in the Methods section about this evaluation.

ARR-08:
Recall reviewer comment: "# lines 238 – 241: This should be method."
Recall our response: "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."

These lines introduced the section "3.3 The period 1985-2025: high variability in summer, trends in winter" recalling in few words the data described in the Method section. We think it is not appropriate to separate in the Method section and prefer to keep as it is.

AR-30 by the authors: The error given by Touratier et al may be and probably will be different from that by the authors in this particular case. The comment by the referee was not answered.

ARR-09:

Recall reviewer comment: "# 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?"
We tried to answer the comment as follows:
"AR-30: Recall lines 277-278: "To estimate the anthropogenic CO2 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)."

We should have indicated that the same TrOCA method was used in previous work in the southern ocean as indicated line 528-531 (in revised MS): "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)."

From referee #2
AR-29 by the authors: The authors confirm the different regions, draw AT-S plots, but then do nothing with that ("no change"). Because use of different regions is not clear to the reviewer/ reader, the reviewer would like to see some explanation in the manuscript.

ARR-10:
Recall reviewer comment: "L161 This relationship was determined with data south of 60°S (as seen in Fig. S4). Why was this region different from the region which was taken for other analyses (south of 63°S)?"
We tried to answer the comment as follows:
AR-29: Thank you for this comment. This is correct, we used all measured AT data south of 60°S to derive the AT/SSS relationship (from the SNAPO-CO2 and GLODAP datasets). This includes all seasons. For the trend analysis we selected the region south of 63°S, i.e. south of the nutrients gradient. For this reply we tested the relationship using AT data south of 63°S and found the same result as presented in figure S4 (Figure R1). No change.

As suggested, we have revised this section as follows in the manuscript:
"Here we used the AT/Salinity relationship derived from AT data south of 60°S (Supplementary Material Figure S4)."

And
"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 (Supplementary Material Figure S4). This relation has been checked for the region south of 63°S, i.e. south of the nutrients gradient (Figure S3) and will be used for the trend analysis discussed in section 3.3.

AR-30 by the authors: It may be true that the FFNN model has been described and published. However, I am sure that not many readers will consult the other publications. The model as such will be similar for all different applications, however, the details for each region will certainly be varying. I agree that not the complete detailed description of the FFNN model should be shown here, but some

more details and explanations would be desirable. This is actually also what the editor asked for. I think the authors have not modified the text sufficiently after the first round of reviews.

ARR-11:
Recall reviewer comment: “L169 and further: The info as to the FFNN, how it works and which parameters, variables and data are used is minimal. For the reader to comprehend and reconstruct the data produced this is not sufficient; the results presented are as if coming from a black box.”
We tried to answer the comment as follows:
AR-30: The FFNN model has been described in detail in Chau et al (2024), including statistical comparison with in-situ data from various oceanic regions. 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).

As indicated in our new response (ARR-04), we think the revised dedicated section 2.4 now present all details of the method.

AR-31 by the authors: Again I would expect some explanation in the text.

ARR-12:
Recall comment from reviewer: “L176 Why were only results used for this small region? This region is again different from the two other regions mentioned before.”
And our first response: AR-31: “Recall line 176: “We used the results in the region 65-66.5°S/139-141°E.” “Thank you. We specified the region 65-66.5°S/139-141°E because for the trend analysis we averaged the monthly 0.25x0.25 grid cells in this box.”

Thank you, as suggested we have added in the text:
“To investigate the temporal changes of the air-sea CO2 fluxes we used the model results in the region 65-66.5°S/139-141°E after averaging the monthly 0.25x0.25 grid cells in this box.”

AR-32 by the authors: Results are presented here, so logically this should be brought in the Results section. Also the editor mentions that this should move to the Results section.

ARR-13:
Recall comment from reviewer: « L177-192 This paragraph does not belong to the methods section, but rather to the results section. »
This concerns the original section “2.4. CMEMS-LSCE-FFNN model and air-sea CO2 fluxes“, separated in section 2.4 (method) and 3.1 (results) in the revision.
As noted by the editor: « I should add that I do think that the information previously included in section 2.4 is now properly included in section 3.1.”

;;;;;;;;;;;;;;;;;;; end reply ;;;;;;;;;;;;;;;;;;;;;;;;