Reply to review 2

This is an interesting study on ocean acidification of a region of the ocean that is important but undersampled. It shows the level of OA and extrapolates it to the future, where important results emerge. Not surprisingly, biological activity plays a major role in the observed changes/variability. The combination of different sources of data makes the study more robust. The contents of the manuscript is fine, but I think it needs more explanation on the methods used and how the results were obtained.
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).

It would have been helpful to the reviewer to have the figures embedded in the text. I think the journal also prescribes this format.
AR-02: Good suggestion. The figures will be included in the text in the revision (and for new submissions).

The numbering of the sub-sections of the results is erroneous. I think it should be 3.1, 3.2. 3.3 and 3.4.
AR-03: Thank you. Sub-section number revised as suggested.

At several place in the text: xCO_2 not XCO_2.
AR-04: Thank you, corrected.

Below a list with mostly minor comments, but also a few major ones:

L24 … a permanent CO_2 sink WHICH was most pronounced …
AR-05: Thank you, corrected.

L25 … to AN increase (delete positive)
AR-06: Thank you, corrected.

L26 delete progressive
AR-07: Thank you, corrected.

L28 pteropods
AR-08: Thank you, corrected.

L36 delete enhanced
AR-09: Thank you, corrected.

Line 59 Shadwick
AR-10: Thank you, corrected.

L39-40 … multiple effects SUCH AS anthropogenic CO_2 …
AR-11: Thank you, corrected.

L44-46 “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.” I suggest to refrain from the negative position (no long-term time series) and write this in a positive sense: There are two long-term time series …
AR-12: Thank you, good suggestion. This has been revised as suggested.

L50 per decade (no exponents use with words, only with symbols) (also at other places in the text)
AR-13: We used exponent $decade^{-1}$ in several papers, journals (including in Biogeosciences, e.g. Metzl et al, 2025). No change.

L52 delete comma
AR-14: Thank you, corrected.

L53 TO the deep ocean, implying …
AR-15: Thank you, corrected.

L61-64 This sentence is grammatically incorrect. Please reformulate
AR-16: Recall L61-64: "As such, ALBW links the ocean CO2 uptake in coastal waters, which have been shown to be associated with significant Cant concentrations of up to 44 µmol.kg-1 in the shelf waters (Shadwick et al, 2014), linking the ocean CO2 uptake in coastal waters and OA at depth (Zhang et al, 2023)."

Suggested revision:
"Significant Cant concentrations of up to 44 µmol.kg-1 have been observed in the shelf waters (Shadwick et al, 2014), linking the ocean CO2 uptake in coastal waters and OA at depth (Zhang et al, 2023)."

L68 Please define C_T
AR-17: C_T was defined in the abstract. As suggested, it is recalled in line 68 (total CO2 concentrations)

L70 What kind of CO_2 is meant here? Dissolved CO_2 or DIC? Please be more specific
AR-18: Recall L70: "This region has large inter-annual variability in CO2,"
Here we mean both pCO2 or DIC. Suggested revision: "This region has large inter-annual variability in pCO2 and CT,"

L80 I think you mean: in the region off Adélie Land
AR-19: Thank you, corrected

L81 again which CO_2?
AR-20: Thank you, corrected here: C_T

L85-88 Please reformulate this sentence
AR-21: Recall L85-88: "Based on subsurface anthropogenic CO2 concentrations estimates for 1995 to 2025 we investigate future changes in Ωar, that are predicted to reach undersaturation by the mid-century (Nissen et al, 2024), with potential impact for marine species such as pteropods (e.g. Bednaršek et al, 2023)." Not sure why we should reformulate. No change.

L115 "metadata of SOCAT" needs a reference
AR-22: Thank you. Added as follows: available in the SOCAT data-sets (www.socat.info)

L118 cruises
AR-23: Corrected

L123 is, not are
AR-24: Corrected

L130 Prof. not Pr.

AR-25: Corrected

L131 Which one is the R2 cruise? Please explain. It was not mentioned before
AR-26: Thank you. R2 cruise correspond to the tracks Hobart-DDU-Hobart conducted in late December 2024 and January 2025. This now also specified in Table S1 of the Sup.Mat.

L135-136 and earlier: The authors used CRMs and write: "These data were used for the validation of calculated carbonate system parameters using fCO_2 data with an A_T/salinity relationship" How were the CRMs used? Were the measured data corrected with a factor that was determined from the CRM value and the measured value? Please provide details in the text.
AR-27: On Lines 135-136 we specified that in addition to the AT-CT data used for the trend analysis, the AT data were also used to estimate the AT/SSS relationship that served to calculate pH with the pairs fCO2/AT. This is independent of the use of the CRMs. No change.
In the method section (Lines 117 we refer to Metzl et al, 2024, 2025) for the AT-CT analyses from discrete samples or surface underway. No change.

L139 nitrate (no capital)
AR-28: Corrected

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)?
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.

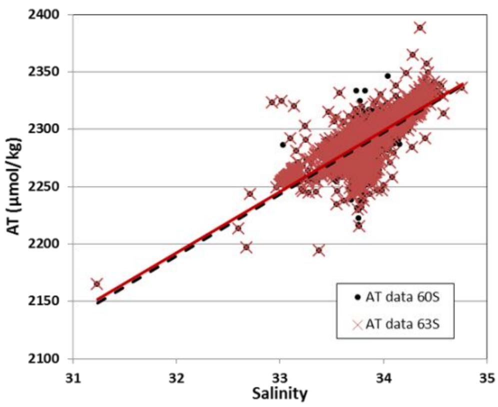

Figure R1: AT/Salinity relationships using data south of 60° (black symbols as in the original paper) or south of 63°S (red crosses) with their respective regressions (black and red lines).

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.
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).

L176 Why were only results used for this small region? This region is again different from the two other regions mentioned before.
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.

L177-192 This paragraph does not belong to the methods section, but rather to the results section.
AR-32: As in section “2.3 Carbonate system calculation and AT/Salinity relationship” where we described calculation and present results of the AT/S relationships, the section “2.4. CMEMS-LSCE-FFNN model and air-sea CO2 fluxes“ introduced the FFNN model and presents results of the fluxes to introduce the link between fluxes and results on OA. We prefer to keep the section 2.4 as presented in the submitted manuscript.

L185 Shadwick
AR-33: Thank you, corrected.

L187, 189 What is the origin of the SAM indices? Please provide reference
AR-34: Thank you. The SAM index was obtained from: https://legacy.bas.ac.uk/met/gjma/sam.html, based on Marshall (2003). This is revised in the manuscript and the reference added.

Figure 2: The orange square is said to be for year 2010, but it covers at least 3-4 years around 2010. This is confusing. Please choose another way of showing the 2010 value.
AR-35: The reviewer is correct, the orange square for 2010 cover few years. Figure revised as suggested below.

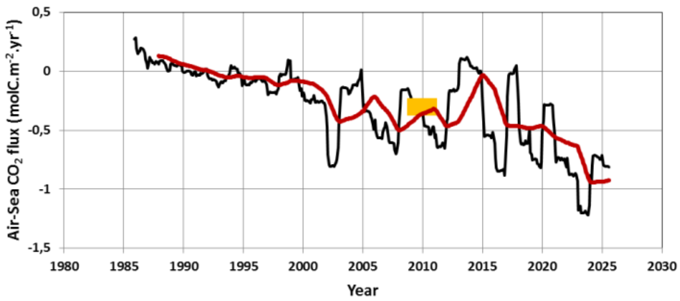

Figure R2: Figure 2 revised for the climatology value in 2010.

Figure 3: The unit of fCO_2 is uatm, but the unit of the atmospheric CO_2 concentration is ppm. This is not correct in the figure. Why don’t they have the same unit? How can the units be reconciled? Same for Fig. S1 and other
AR-36: Thank you. The atmospheric concentration of 422 ppm was converted in µatm (419.5 µatm) at SST=0°C SSS=33. The legend has been revised as follows:
“The red dashed line is the mean atmospheric fCO2 in January 2025 at Cap Grim or at South Pole (419.5 µatm).”

Figure 4: How was normalization of fCO_2 and C_T done? This could be added to the Methods section
AR-37: Thank you. We added this information in the method section and reference as follows:
"To take into account the warming/cooling or the freshening, the fCO2 and CT data were also normalized. For temperature fCO_2 normalization (fCO_2^n) at SST fixed (SST_{fix}) we used the relation described by Takahashi et al (1993):

$fCO_2^n = fCO_2\ EXP(0.0423*(SST_{fix}-SST))$

The salinity CT normalization was calculated at salinity 34 (N-CT = CT x 34/SSS)."

Figure 6: In panel (b), which shows CT, fCO_2 is shown in the inset, but there is no scale. I presume that this is not fCO_2 but something else, possibly C_T calculated from fCO_2? Please correct
AR-38: Thank you. In all panels in Figure 6 or Figure 7 we specified the origin of the data. For example, in panel 6b, "fCO2 Jan-Feb" means that grey circles correspond to CT calculated with fCO2 data. We think this is clearly indicated in the legends. No change

L204 The ship's name is not mentioned in the Methods section. There, only L'Astrolabe is mentioned as ship for the three cruises.
AR-39: Recall line 204: "For example, along the ~140°E track visited in January 2024 (RV/Investigator) and in February 2025 (L'Astrolabe),…" Ship's names are indicated in Table S1. No change.

L242-247 Did you only calculate the trend using the first and last values? Why not using all data in the time period. This would increase the reliability of the trend calculation.
AR-40: Recall line 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."
Yes, in this section, to introduce the range of the temporal differences we started comparing the last (January 2025) new data with the first data (January 1984), i.e. over 40 years. This is presented for measured fCO2 and CT to give a taste of the trend based on only two points (imagine one has only these two data points). This introduced shortly the measured data in 2025, i.e. fCO2 and CT are higher than any previous observation.

L280 "Here we selected the data below 100m depth for 1994-2018" Why again a different time period? The argument should be given in the text. And why 100 m? At least a reference should be given with respect to the depth of the mixed layer in this region.
AR-41: Thank you. The time period for Cant estimates depends on the available data (Table S2).

AR-42; "And why 100 m?" The data-based methods are not able to estimate Cant in surface layer. As indicated 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 review we show example of Cant profiles estimated in winter 1996 and summer 1995, 2023 (Figure R3). 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).

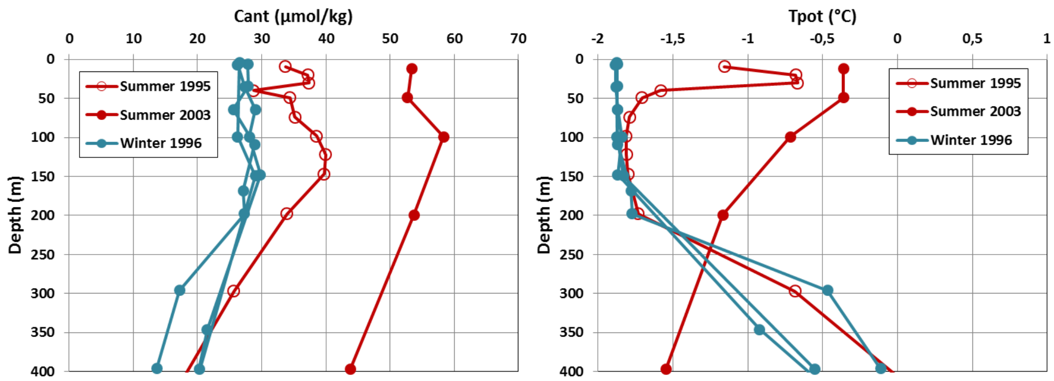

Figure R3: Profiles of Cant and potential temperature around 65°S/140E. In winter the Cant concentration is homogeneous in the mixed-layer whereas in summer Cant is not well estimated mainly due to biological activity.

L285-287 “that d13C observations in January 2025 were lower than in January 2003 suggesting that surface waters captured the anthropogenic Suess effect signal (Figure S13) as previously observed in this region (McNeil et al, 2001)” This asks for a better and more extensive explanation in the text.
AR-43: Thank you. Here we recalled information that anthropogenic signal can be also identified from C13 data as was discussed by McNeil et al (2001) in this region. We think useful to show that the new 2025 C13 data indicate a decrease compared to 2003, but as the seasonality and inter-annual variability could be large, a dedicated analysis based on C13 data in this region deserve for another paper not only off the Adélie Land as shown here, but over the full tracks (Hobart-DDU).

L323 delete version
AR-44: Thank you. Corrected

L324-325 “The estimated C_T using the C_{ant} trend (Eq. 5) is close to the FFNN model and observations (Figure 10a)” This is not completely surprising, as the results were based on the same data.
AR-45: The results presented in Figure 10 are independent. The FFNN model is, in part, constrained by fCO2 data when the estimated CT is based on the Cant trend deduced from stations data.

L325 “The mean difference of C_T” Which difference is meant here? Difference with what?
AR-46: Thank you. The differences were presented in Figure S12. As suggested we revised Line 325 as follows: “The mean difference of CT between the FFNN model and the CT estimated for all years (1985-2025) is -1.72 ±1.53 µmol.kg-1.”

L326-327 “but other process could explain the full trend in surface C_T.” I think you mean: … but additional processes need to be included to explain the full trend in surface C_T.
AR-47: Thank you. Revised as suggested:

L328, 392 C_T-rich, not rich-C_T
AR-48: Thank you. Corrected.

L331-332 I suggest: … that the anthropogenic CO_2 uptake is responsible for 70% of the CT increase over 1985-202. But what about the remnant 30%; what factors are responsible for that? Please explain in the text.
AR-49: Thank you. This is an important point regarding our knowledge (or better un-knowledge) of the processes that drive the CT and Ωar temporal changes. 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 a revision 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. A possibility is the increase of the mixed-layer depth related to the cooling observed in this region (Figure R4) but how this is translated in CT change is beyond the scope of our analysis.

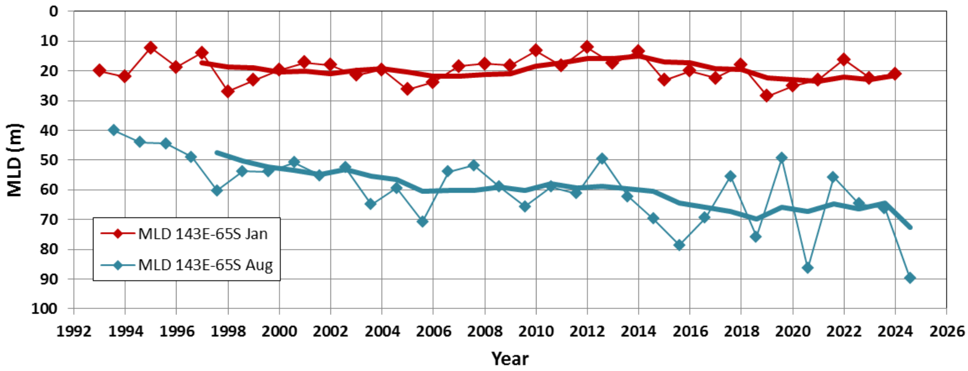

Figure R4: Monthly mixed-layer depth (MLD) derived from an ocean model around 143°E/65°S (CMEMS GLORYS12V1 product, https://doi.org/10.48670/moi-00021). In summer the MLD is stable whereas in winter the model suggests an increase, on average around 60m in 2004-2014 and 70m in 2015-2024.

L340 ... remnant winter WATER layer ...
AR-50: Thank you, corrected.

L342 Suggestion: An important question is when the aragonite undersaturation ...
AR-51: Recall line 342: "We are now interested in exploring when the aragonite undersaturation would be reached in surface waters (Figure 12). " No change, we prefer to keep as written.

L344 ... show SIMILAR results ...
AR-52: Thank you, corrected.

L344 Suggestion: Differences between the two future scenarios ...
AR-53: Thank you, corrected.

L353 will reach, not reached
AR-54: Thank you, corrected.

L362 Place shipboard before fCO_2 and BGC-Argo before pH_T
AR-55: Thank you, corrected.

L365 measurements
AR-56: Thank you, corrected.

L394 surface is mentioned twice; one should be deleted
AR-57: Thank you, corrected.

L404 Australian
AR-58: Thank you, corrected.

L405, 422 Earlier the ship was called L'Astrolabe

AR-59: Thank you, corrected.

;;;;;;;; Reference in this reply, not listed in the Manuscript

Abe, Y., Ito, T., Timmerman, A. H. V., Reinhard, C. T., and Montoya, J. P.: Understanding the resilient carbon cycle response to the 2014–2015 Blob event in the Gulf of Alaska using a regional ocean biogeochemical model, Biogeosciences, 23, 3871–3885, https://doi.org/10.5194/bg-23-3871-2026, 2026.

Da, F., Stock, C. A., Dunne, J. P., Liu, X., Luo, J. Y., Lee, M., & Shevliakova, E. (2025). A global perspective on river alkalinity: Drivers and implications for coastal ocean carbonate chemistry. Global Biogeochemical Cycles, 39, e2025GB008528. https://doi.org/10.1029/2025GB008528

Findlay, H.S., Feely, R.A., Jiang, L.-Q., Pelletier, G. and Bednaršek, N. (2025), Ocean Acidification: Another Planetary Boundary Crossed. Glob Change Biol, 31: e70238. https://doi-org.insu.bib.cnrs.fr/10.1111/gcb.70238

Findlay, Helen S. , Lily Anna Stokes, Carol Turley, 8 - Ocean acidification, Editor(s): Trevor M. Letcher, Indicators of Climate Change, Elsevier, 2026, Pages 163-197, https://doi.org/10.1016/B978-0-443-32838-1.00004-7.

Hsiao, Shih-Chun , Han-Lun Wu, Huei-Shuin Fu, Wei-Bo Chen, Accelerated Ocean acidification (1985–2022) threatens tropical coral reefs and highlights biogeochemical refugia for marine conservation, Journal of Sea Research, Volume 207, 2025, 102612, https://doi.org/10.1016/j.seares.2025.102612.

Hunter, A., Thorpe, S.E., McCarthy, A.H. et al. Microplastic hotspots mapped across the Southern Ocean reveal areas of potential ecological impact. Sci Rep 14, 31599 (2024). https://doi-org.insu.bib.cnrs.fr/10.1038/s41598-024-79816-y

Jiang, L.-Q., et al.,: Synthesis of data products for ocean carbonate chemistry, Earth Syst. Sci. Data, 18, 1405–1462, https://doi.org/10.5194/essd-18-1405-2026, 2026.

Kang, Z.Y., Cheah, W., Ishizaka, J. et al. Asymmetric carbon response to the 2019 extreme positive Indian Ocean Dipole. npj Clim Atmos Sci 9, 126 (2026). https://doi.org/10.1038/s41612-026-01402-y

Marshall, G. J.: Trends in the Southern Annular Mode from observations and reanalyses. J. Clim., 16, 4134-4143, doi:10.1175/1520-0442%282003%29016<4134%3ATITSAM>2.0.CO%3B2, 2003.

Metzl, N., Lo Monaco, C., Tribollet, A., Ternon, J.-F., Chevallier, F., and Gehlen, M.: New observations confirm the progressive acidification in the Mozambique Channel, Biogeosciences, 22, 7187–7204, https://doi.org/10.5194/bg-22-7187-2025, 2025.

Savelli, R., Carroll, D., Menemenlis, D., Lauderdale, J. M., Bertin, C., Dutkiewicz, S., Manizza, M., Bloom, A. A., Castro-Morales, K., Miller, C. E., Simard, M., Bowman, K. W., and Zhang, H.: Implementing riverine biogeochemical inputs in ECCO-Darwin: a sensitivity analysis of terrestrial fluxes in a data-assimilative global ocean biogeochemistry model, Geosci. Model Dev., 19, 867–885, https://doi.org/10.5194/gmd-19-867-2026, 2026.

Takahashi, T., Olafsson, J., Goddard, J. G., Chipman, D. W., and Sutherland, S. C.: Seasonal variation of CO2 and nutrients in the high-latitude surface oceans: A comparative study, Global Biogeochem. Cycles, 7, 843–878, https://doi.org/10.1029/93GB02263, 1993.

Tan et al, 2025. Observed large-scale and deep-reaching compound ocean state changes over the past 60 years, Nat Clim Change.

Wu, Z., Wang, H., Liao, E., Hu, C., Edwing, K., Yan, X-H., Cai, W-J., Air-sea CO2 flux in the Gulf of Mexico from observations and multiple machine-learning data products, Progress in Oceanography (2024), doi: https://doi.org/10.1016/j.pocean.2024.103244

Wu, Q., Hu, K., Ruan, Z., Li, Z., & Wang, X. (2026a). Physical mechanisms governing the response of oceanic pCO2 in the Northern South China Sea to tropical cyclones. Geophysical Research Letters, 53, e2025GL118879. https://doi.org/10.1029/2025GL118879

Wu, H., Ji, Y., Wang, L., Liu, X., Zhou, W., Cui, L., Wang, Y., Liu, M., and Li, Z.: Reconstruction and spatiotemporal analysis of global surface ocean pCO2 considering sea area characteristics, Biogeosciences, 23, 967–994, https://doi.org/10.5194/bg-23-967-2026, 2026b

;;;;;;;;; end reply