

Review on 'The evolution of anthropogenic carbon accumulation in the North Atlantic Ocean: the influence of physical and chemical drivers over the last three decades' by Cathy Wimart-Rousseau et al.

This paper describes the temporal increase of anthropogenic carbon (Canth) in the northern part of the Atlantic from 1993 to 2021. Therefore, the North Atlantic is divided into 13 biogeochemical provinces and 7 isopycnal layers. The Canth increase in each of these regions and layers is divided into the contribution from volume change of the layers and Canth concentration increase. The results are then correlated with NAO (North Atlantic Oscillation) and the AMOC at RAPID and OSNAP to identify the physical drivers of the Canth change. I find the topic of the manuscript is of high interest and merits publication in BG after major revisions.

In this response, the “original manuscript” refers to the first submitted manuscript that has been evaluated by the reviewer, and the “revised manuscript” refers to the manuscript that has been modified according to the reviewer’s comments. Comments from the reviewer are pasted below in black font; our point-by-point responses immediately follow in blue font. Blue italic sentences are those that have been modified/added in the revised manuscript. When indicated, line numbers refer to the new version of the manuscript (Pdf version). We have also submitted a document with the “track changes” function activated, which should help the reviewer in identifying the changes.

General comments

Such a detailed analyses of Canth changes and its causes is an ambitious task, maybe at some points too ambitious. This refers to the availability of Canth data as well as to the conclusions drawn in the text. These often sound reasonable, but sometimes they are not supported by the presented data/results, sometimes they even contradict the results from the data analyses. Also, the direct results presented in the figures are sometimes questionable.

We thank reviewer #2 for their helpful and stimulating comments regarding our manuscript, their positive opinion concerning the relevance of this manuscript, and for reviewing it. We most appreciate the relevant comment about the data interpretation as well as the questions about how some conclusions were drawn. In the revised manuscript, we have further detailed our analyses in order to better clarify the raised comments and points, and inherent assumptions. We hope that all comments and suggestions made by Reviewer #2 have been fully addressed.

Specific comments

Section 2, Data and methods

This section does not adequately describe the methods used for the following analyses of Canth inventories.

Following both referees’ comments, the Data and Methods section has been comprehensively modified and developed in the revised manuscript. Please see below our point-by-point modifications.

Subsection 2.1, Carbon data

I. 114/115 ,Canth values from GLODAP’ and I.117/118, ,Canth observations’

The GLODAP data product does not contain C_{Anth} values, and there are also no (direct) observations of C_{Anth} . C_{Anth} is a (theoretical) quantity that might be inferred/calculated from measurements of other quantities. Please always differentiate properly between measured/observed and inferred/calculated values.

We acknowledge the reviewer for raising this important point. Indeed, GLODAP variables (temperature, salinity, DIC, TA, O_2 , and nutrients) were used to derive the C_{Anth} concentrations, as stated in the original manuscript L.100-102 (*"Since C_{Anth} concentrations cannot be measured directly, they are derived from in situ observations using previously applied methodology. Here, C_{Anth} concentrations were estimated with the ϕC_0^T back-calculation method [...]."*). However, we do agree that this clarification about measured and derived data might not have been clear enough in certain sentences, as raised, for instance, in L. 114-115. In the revised manuscript, the distinction between measured and derived values has been made explicit, particularly in L. 114-115, L. 125-130, page 9 - Fig.2's caption, page 22 - Fig.7's caption, page 26/27 - Fig.8's caption, page 32 - Fig.10's caption, and page 36 - Fig.A2's caption.

L.114-116

, C_{Anth} values from GLODAP were annually averaged to reduce seasonal bias, and trends and standard deviations were then calculated per region, density interval and year':

I was really surprised when reading that, as the GLODAP data product does not contain enough carbon (and carbon related data) to do so. When reading further, it became clear mainly from the figures (Fig. 6 and A2), that C_{Anth} data are NOT available for every year/region/layer, but often they are (linearly) interpolated. This has to be clearly stated here in the method section and not be ,hidden' in the legend of a figure.

We agree with the referee that the interpolation procedure was not described in the Data and Methods section of the original manuscript, and we thank the referee for pointing this out. In the revised manuscript, we have added a sentence clarifying that, when C_{Anth} could not be inferred, missing values were estimated from linear regressions fitted to the available data to fill the gaps: *"Moreover, it should be noted that when no GLODAP data were available for a given province-year combination to infer C_{Anth} concentrations, these values were estimated from linear trends fitted to the available time series."* (L. 125-126)

Together with the temporal interpolation, another question arises: Which time series did you use for estimating the correlations and their significance? The time series based on the available C_{Anth} data, or the one with the linearly interpolated C_{Anth} quantities? Using the linear interpolation might alter the correlation and especially the significance, hence I would really recommend to only use the ,real' C_{Anth} values here.

The regression analyses presented in the original manuscript (particularly in Figs 7 & 8) were performed using the "complete" C_{Anth} time series, composed of both annually averaged C_{Anth} values directly derived from GLODAP measurements and gap-filled C_{Anth} values derived from the linear trend fitted to the available observations. We acknowledge that this was not explicitly stated in the original manuscript and that the associated uncertainty was not quantified. However, following the referee's recommendation, Figures 7 and 8 have been redrawn in the revised manuscript. To produce them, the

temporal background trend for each province/density layer combination is still derived from the complete C_{Anth} time series (both directly derived and gap-filled values). We made this choice to be consistent with the trend analysis presented throughout the manuscript (e.g., Fig. 5c) and to ensure that the background trend estimate is not biased by the uneven temporal sampling of GLODAP cruises (using only measured years would give disproportionate weight to periods of dense observational coverage and could produce poorly constrained trends for province/layer combinations with few available cruises). Then, the residuals (anomalies) were computed exclusively from years with direct observational coverage. For years without direct observations, no anomaly is computed, and those years do not contribute to the regression. The overall spatial patterns and regression slopes in Figs. 7 and 8 are unchanged in comparison with the original Figures 7 and 8 (see Figure 1 below), confirming the robustness of the results, though the R^2 values are slightly modified as expected given the reduced number of data points entering the regression. In the revised manuscript, this information has been added: “Moreover, it should be noted that when no GLODAP data were available for a given province–year combination to infer C_{Anth} concentrations, these values were estimated from linear trends fitted to the available time series. To perform the regression analyses (see Sections 3.4.1 & 3.4.2), the C_{Anth} anomaly for each province and density layer combination was defined as the deviation of the annually averaged C_{Anth} value from the long-term linear trend derived from the complete time series (see Section 3.2). Only years for which direct GLODAP observational coverage was available were retained for the NAO and AMOC regression analyses; years inferred from the linear trend were excluded.” (L. 125-130)

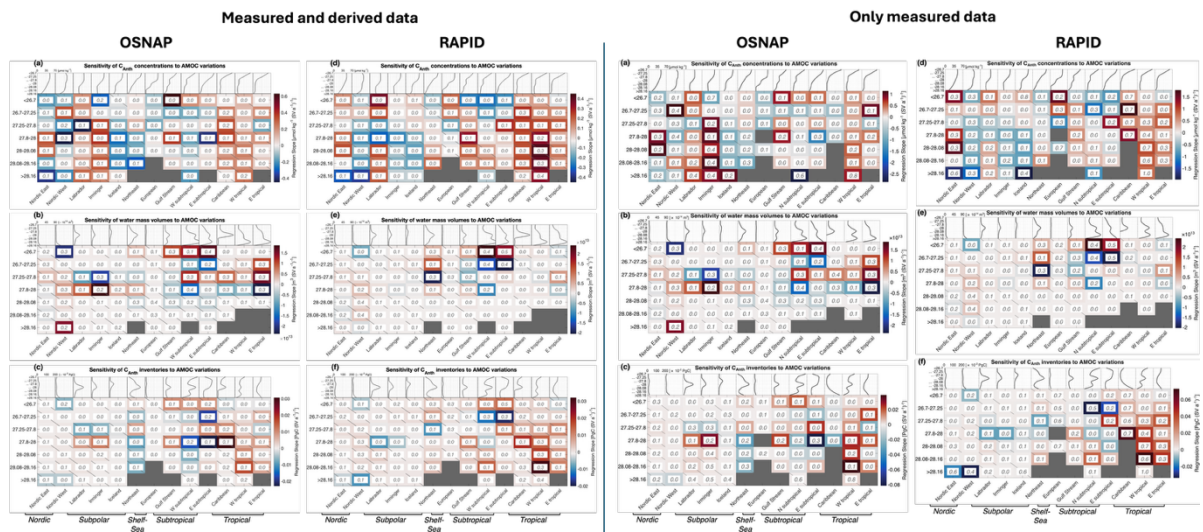


Figure 1 (not fully included in the revised manuscript). Comparison between the original Figure 8 (left panels) and the revised Figure 8 (right panels) displaying the sensitivity of (a & d) C_{Anth} concentration residuals (inferred minus long-term linear trend), (b & e) water mass volume anomalies (relative to 1993 baseline) and (c & f) C_{Anth} inventory residuals (inferred minus long-term linear trend, in PgC) to the Atlantic Meridional Circulation (AMOC) at OSNAP (panels a-c) and RAPID (panels d-f). Heatmaps display the regression slopes derived using the whole reconstructed C_{Anth} dataset (original Figure 8) and the derived-only C_{Anth} values structured dataset (right panels, revised Figure 8). It can be noted that similar patterns can be observed, with the only changes being related to the R^2 values observed.

Another question is, how did you calculate the mean values for one year/region/layer? Did you take the mean of all C_{Anth} values within one region/layer? Or did you grid the C_{Anth} data to infer the regional mean? If you calculate the C_{Anth} inventory just from the total volume and the mean C_{Anth}

value for every region/layer, you introduce an error as C_{Anth} and layer thickness might be correlated. You also introduce an error due to the sparse C_{Anth} data, which even within one region are not located at the same places from year to year (spatial bias).

I am not going to force you to use a specific method for calculating all the layer mean C_{Anth} concentrations and inventories, but you have to (i) clearly state WHAT you have done and (ii) estimate the uncertainties arising from that procedure.

In this study, mean values were derived for each year, region, and province by averaging the data, either calculated from direct GLODAP measurements or derived from the linear trend fitted to the available observations. No data gridding was performed.

About the inventory: We believe that the inventory calculation avoids the potential bias described above by the referee because C_{Anth} concentrations and water mass volumes were derived from independent data sources: C_{Anth} from GLODAP measurements, and water mass volumes from the ARMOR3D gridded product. Indeed, the inventory calculation has been computed as follows: (1) When GLODAP data were available, C_{Anth} values were derived and assigned to one of the seven neutral densities based on the neutral density computed for each individual observation. (2) For each density/layer-region-year combination, a mean C_{Anth} concentration has been computed by including both the calculated values and the “gap-filled” ones. (3) Independently, the ARMOR3D gridded data product was used to derive the volume of each density layer within each province for each year, by integrating the layer thickness (defined by the depth interval bounded by the isopycnal surfaces of the density layer) over the known surface area of the province. (4) The C_{Anth} inventory for each layer-province-year was then obtained as the product of the layer mean C_{Anth} concentration and the corresponding layer volume. Hence, because the mean C_{Anth} concentrations and layer volumes were computed independently for each density interval, any covariance between these variables is explicitly resolved rather than aliased into a bulk mean. In the revised manuscript, it has been clarified: “ *C_{Anth} column inventories for each province and density layer were computed by multiplying the annually averaged C_{Anth} calculated values (in $\mu\text{mol kg}^{-1}$) by the mean seawater density (in kg m^{-3}) and the corresponding water mass volume derived from ARMOR3D (in m^3 , see Section 2.3) for each year, layer and region considered, yielding absolute C_{Anth} inventories expressed in moles.*” (L. 132-135)

About the spatial repartition: We agree that spatial averaging induces a “compromise” between regional representativeness and statistical robustness. The sub-regions used in this study were specifically selected and modified to capture biogeochemically coherent water masses, as evidenced by the tight $C_{\text{Anth}}-\theta$ relationships shown in Figure 2 (in the revised manuscript). Across most sub-regions, C_{Anth} concentration and potential temperature collapse onto a single regression line with high R^2 , indicating that profiles sample coherent water masses with consistent θ - C_{Anth} co-variation rather than a heterogeneous mixture. However, we acknowledge that residual spatial heterogeneity exists and have added some sentences throughout the revised manuscript to highlight that: “*Additionally, it can be noted that in years with sparse sampling concentrated in a sub-region, annual means may not be fully representative of the full province extent, as illustrated by the 2009 anomaly in the Northeast deepest layers where a single cruise sampling the western part of the province drives the high annual mean despite physically consistent hydrographic properties.*” (L. 469-472)

L.126/127

,annually averaged C_{Anth} values deviating ... from the regional-layer-mean pressure’

A Canth value cannot deviate from a pressure value. You probably mean the location (in depth/pressure) of the Canth values.

We thank the referee for spotting this confusion between pressure and C_{Anth} that has been corrected in the revised manuscript: *“Mean pressure was used as an additional criterion: annually averaged C_{Anth} values were flagged and excluded when the mean sampling pressure in that year deviated by more than 2σ from the regional-layer climatological mean pressure. Values were considered outliers if they exceeded either the adjusted standard deviation or the 2σ pressure criterion”* (L. 119-122)

Subsection 2.2, AMOC and NAO time series

Here you say, a 12-month running mean of AMOC strength was used. Later, you mention an ,accumulated' AMOC according to Bryden et al. (2014). Which of the two have you really used? If you used the accumulated AMOC, you should show that time series, maybe in Fig. A1 together with the ,real' AMOC.

We thank the referee for pointing out this ambiguity. The 12-month running mean and accumulated AMOC anomaly are two distinct, but complementary metrics used at different stages of the analysis. To be more precise:

- The 12-month running mean AMOC values were downloaded directly from the MET office webpage (<https://climate.metoffice.cloud/amoc.html#%23datasets>) for both the RAPID and OSNAP arrays. For this study, we preferred to use the 12-month running mean values over the raw transport signal, as this smoothing reduces short-term and seasonal variability (Jackson et al., 2019). These “smoothed” time series are shown in Fig. Supp. A1 (colored lines in each panel).
- Following Bryden et al. (2014), the accumulated AMOC anomalies were derived from these smoothed values by integrating monthly deviations from a 10-year time window moving baseline. This metric hence quantifies the cumulative effect of AMOC deviations over multi-year periods rather than representing instantaneous transport strength. The accumulated AMOC anomalies are the quantity used in the regression analyses of Section 3.4.2. and in Fig.8. Following the referee's comment, the accumulated AMOC anomalies for both OSNAP and RAPID are now shown in Fig. Supp. A1 (dashed black lines, right y-axis of each panel) alongside the 12-month running mean transport values.

In the revised manuscript, the difference has been clarified: *“In this study, rather than raw transport signals, 12-month running means of AMOC strength were used to reduce short-term and seasonal variability (Jackson et al., 2019) and facilitate comparison with model outputs. Following Bryden et al. (2014), these values were then used to derive the accumulated AMOC anomalies by integrating AMOC deviations at both sites from a 10-year moving baseline, providing a physically consistent measure of the cumulative circulation anomaly that integrates persistence and magnitude over multi-year timescales (see Supp. Fig. A1). This avoids spurious correlations with short-term variability and better reflects the long-term redistribution of heat, freshwater, and carbon by the overturning circulation. Hence, the 12-month running mean values characterise the temporal evolution of AMOC transport while the accumulated anomaly captures the integrated, multi-year influence of overturning variability on carbon storage.”* (L. 188-195)

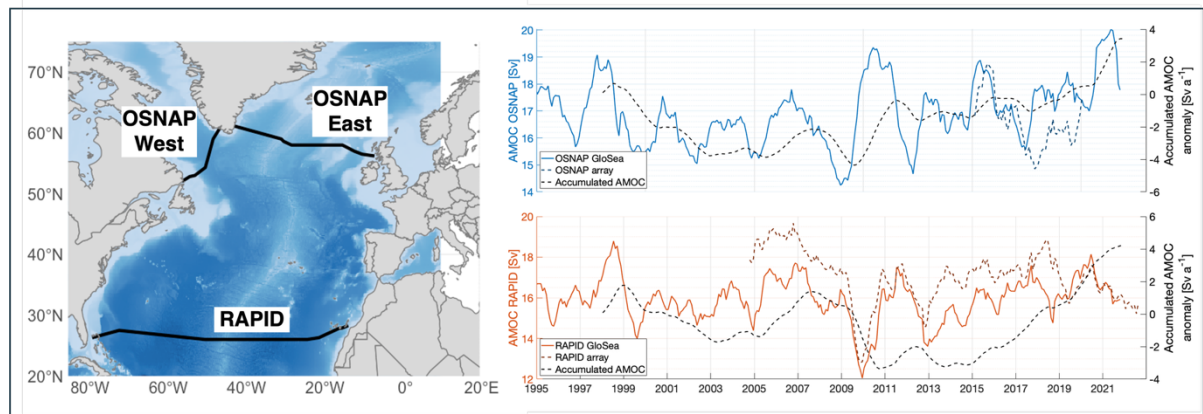


Figure A1 – Supplementary Material (included in the revised manuscript). Time-series of the 12-month running mean AMOC values acquired across the OSNAP array in the subpolar North Atlantic (top panel) and the RAPID array at 26°N (bottom panel). On both plots, two datasets are shown: OSNAP or RAPID array observational data (dashed colored lines) and GloSea5 global ocean reanalysis data (full colored lines). Dashed black lines on both panels represent the accumulated AMOC anomaly time series, derived following Bryden et al. (2014) using a 10-year moving baseline. The inset map shows the locations of the two arrays.

Jackson, L. C., Dubois, C., Forget, G., Haines, K., Harrison, M., Iovino, D., et al. (2019). The mean state and variability of the North Atlantic circulation: A perspective from ocean reanalyses. *Journal of Geophysical Research: Oceans*, 124, 9141–9170. <https://doi.org/10.1029/2019JC015210>

Subsection 2.4, Biogeochemical provinces and density layers

I think the area of the 'Caribbean' province should be revised. According to the map in Fig.1, this province contains a small area north and west of the Antilles Islands. There, the Deep Western Boundary Current (DWBC) is located. This means, for the Caribbean province you combine the old deep water from within the Caribbean (LSW only) with the young deep water from the DWBC, which for me does not make sense. The DWBC east of the Antilles should be accounted to the West Tropical Province. Note that inside the 'proper' Caribbean (south and west of the Antilles), the maximum density is smaller than $\gamma_n=28$. The 'Caribbean' data for the denser layers originate completely from the DWBC outside the 'real' Caribbean. This can also be seen from their σ_{θ} values, which are higher for the densest Caribbean layers than for the densest West tropical layers.

We acknowledge the reviewer's concern and thank their suggestion to modify the Caribbean region. The original Caribbean province boundaries followed Longhurst (2007), ensuring reproducibility and comparability with the existing literature. However, following their comment, the boundaries have been updated, now being more restrictive to the region west of 75°W (see Fig.2 below, not in the revised manuscript). By doing so, the maximum neutral density found is now 27.92 kg m⁻³, in agreement with what has been reported in the literature for this region (as raised by the referee). It can be noted that the data points located east of 75°W are now included in the Western tropical province.

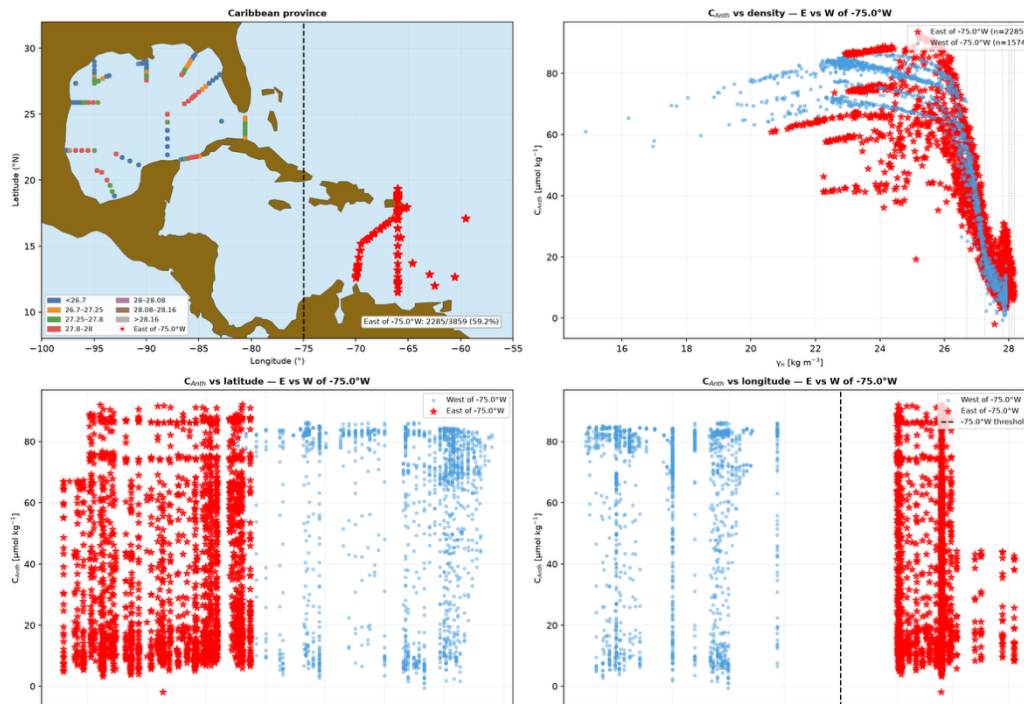


Figure 2 (not included in the revised manuscript). Geographic distribution of the inferred C_{Anth} data assigned to the Caribbean province. (a) Spatial distribution of data points coloured by density interval. Values that are now associated with the Western tropical province are highlighted in red. (b) C_{Anth} ($\mu\text{mol kg}^{-1}$) as a function of neutral density γ_n . (c) C_{Anth} as a function of latitude. (d) C_{Anth} as a function of longitude. In panels (b)-(d), red stars represent the subset east of -65°W .

Longhurst, A. R.: *Ecological geography of the sea*, Academic Press, Amsterdam Boston, 2nd ed edn., ISBN 978-0-12-455521-1, 2007

Density layers (l.169)

You note the density layers, but you do not give any information which water mass is in which density layer (e.g. 27.8-28: LSW, >28.16: AABW). Please do so.

We thank the reviewer for this comment, that was also raised by the other referee, In the revised manuscript, the rationale behind the density layer selection and the water masses has been highlighted: “From the surface to the bottom, the seven selected density intervals tend to represent: the surface waters ($\gamma_n < 26.7 \text{ kg m}^{-3}$), the North Atlantic Central Waters ($26.7 < \gamma_n < 27.25 \text{ kg m}^{-3}$), the Antarctic Intermediate Waters ($27.25 < \gamma_n < 27.8 \text{ kg m}^{-3}$), the Labrador Sea Waters ($27.8 < \gamma_n < 28.0 \text{ kg m}^{-3}$), the upper North Atlantic Deep Waters ($28.0 < \gamma_n < 28.08 \text{ kg m}^{-3}$), the lower North Atlantic Deep Waters component with the Iceland-Scotland Overflow Waters ($28.08 < \gamma_n < 28.16 \text{ kg m}^{-3}$), and the lower North Atlantic Deep Waters component with the Denmark Strait Overflow Waters ($\gamma_n > 28.16 \text{ kg m}^{-3}$).” (L. 214-219).

l.250

,increases this contribution to 35%’

I have no idea what ,contribution’ is meant here. Do you refer to the surface area, or to the surplus in efficiency?

This affirmation was about the surplus in efficiency: “Although the Labrador, Irminger, Iceland, and Northeast provinces cover only 18% of the surface area, they are approximately 25% more efficient at storing C_{Anth} per unit area than the basin mean. Including the W/E subtropical provinces increases this storage efficiency to almost 35% above the basin mean, despite these six provinces together covering only 44% of the total surface area.” (L. 299-302)

I.256/257

,In the Nordic provinces, ... the densest layers contribute more substantially’.

This is not surprising, as they have by far the largest volume (see Fig. 3b), please mention that here.

The referee is correct, and this information has been added in the revised manuscript (L. 308-309).

Fig.5 (l. 264) is mentioned before Fig.4 (l.268), exchange the numbering. l.281 and l.283

In the revised manuscript, figure numbers are similar to the original ones, but the text has been modified to present Fig. 4 first and then Fig. 5 (L. 318-406) .

In l. 281 you mention 3 decadal Canth inventories, which increase roughly by 10% per decade. How can you infer an annual increase rate of 1.72% from that? Or did you apply a linear fit (inventory versus time) in Fig. 4a? Please explain. If you apply a linear fit, include it in Fig. 4a. Later, in l. 285/286, you mention a linear fit. However, the increase rate of 0.39 PgC/yr does not equal 1.72%/a if the total inventory is about 35 PgC.

We apologise for the ambiguity. The annual rate of 1.82% yr^{-1} (previously 1.72% yr^{-1} in the original manuscript) was not derived from the three decadal snapshots but from the full time series: $((40.13 \text{ (PgC in 2021)} - 26.26 \text{ (PgC in 1993)}) / 26.26) \times 100 = 52.82\%$ total change over 29 years (1993-2021), divided by 29 years. This is a simple mean annual percentage change relative to the 1993 baseline, different from the absolute linear trend of 0.42 PgC yr^{-1} (from Fig. 4a; previously 0.39 PgC yr^{-1}), which is a separate metric; the two quantities use different reference bases and are complementary. We have clarified this distinction in the revised text as follows: “We derived full North Atlantic inventory values of 30.5, 34.5, and 38.0 PgC for the years 2000, 2010, and 2020, respectively, which compare well to the independently calculated estimates of Steinfeldt et al. (2024), ranging from 21.9 ± 3.4 PgC to 36.6 ± 5.7 PgC for decadal assessments from 1990 to 2020. We also identify a total NA CAnth inventory increased from 26.3 PgC in 1993 to 40.1 PgC in 2021, representing a cumulative increase of 52.8%, meaning an inventory increase rate of 1.82% a^{-1} over the 30-year time period considered.” (L. 335-339)

We also note that during the revision process, we identified a minor error in the C_{Anth} -filtering step applied to the inventory computation, which slightly affected the three decadal snapshot values reported in the original manuscript. This has been corrected (the updated values are 30.5 PgC in 2000, 34.5 PgC in 2010, and 38.0 PgC in 2020) and the correction has been propagated consistently throughout all figures and analyses. However, the impact on the results and conclusions is negligible.

I.304/304

,Nordic-W shows strong positive trends in multiple density classes (27.25-27.8), consistent with isopycnal deepening and enhanced deep water formation.

The positive trends are correct, but the deep water formed in the Nordic seas is denser than 28.0. Note that the volume of the lighter layers is very small, so trends here do not alter the Canth inventories significantly.

We thank the referee for this important clarification. We agree that deep water formed in the Nordic Seas is denser than $\gamma_n = 28.0 \text{ kg m}^{-3}$, and the water volume trend panel (Fig. 5e in the revised manuscript) indeed shows that the largest positive trend there occurs in the densest layer, consistent with enhanced deep-water formation. We have revised the manuscript to clarify this point, as the original sentence was misleading. We also acknowledge the referee's comment about the negligible contribution of light layers to C_{Anth} inventory trends in this region, which is now explicitly noted in the revised manuscript: *"For instance, Nordic-W shows strong positive trends across multiple density classes. In the intermediate layers ($\gamma_n = 27.25\text{--}28.0 \text{ kg m}^{-3}$), positive trends are consistent with isopycnal deepening while the strongest positive trend occurs in the densest layer ($\gamma_n > 28.16 \text{ kg m}^{-3}$), consistent with enhanced deep-water formation which thickens deep layers while thinning the surface."* (L. 379-382)

I.323-324

,Conversely, the loss or redistribution of water mass with relatively low Canth content induces a positive contribution to the total inventory'.

First, in Fig 5 only the contribution of volume change to the layer-wise Canth inventory is shown, not to the total (top to bottom) inventory.

Second, I think your statement is a misinterpretation. If the Canth inventory trend is negative (as it is for the upper Nordic-W layer), a volume reduction is accounted positive according to your algorithm (i.e. the contribution of the volume reduction strengthens the negative trend), whereas a volume increase opposes the trend and hence is counted negative. I would change that and show a negative volume trend always as a negative contribution to the Canth inventory trend and vice versa, independent from the sign of the Canth inventory trend. The same feature (negative volume trend, negative Canth inventory trend, positive contribution of volume to inventory trend) can be seen e.g. for the $\gamma_n=27.8\text{--}28.0$ layer in the Caribbean.

We thank the reviewer for identifying both issues. First, we confirm that panels (d) and (f) of Fig. 5 in the revised manuscript show the layer-wise contributions, not the total (top-to-bottom) inventory contribution. We have corrected the text and caption accordingly to clarify this point: Fig.5 caption-page 15, *"Panels (d) and (f) of Figure 5 emphasise the relative contributions of the C_{Anth} concentration and volume trends to C_{Anth} area-normalised inventory changes for each density layer."* (L. 386-387).

Second, the reviewer correctly identifies an ambiguity in our decomposition algorithm: dividing by the signed $\Delta\text{Inventory}$ total caused a negative volume contribution to appear as positive when $\Delta\text{Inventory}$ total was also negative. We have revised the algorithm to normalise by absolute ($|\Delta\text{Inventory}|$), thus ensuring that a negative (positive) volume trend is always displayed as a negative (positive) contribution, independent of the sign of the inventory trend. Panels (d) and (f) of Figure 5 (in the revised manuscript) have been regenerated accordingly, and the associated paragraph has been corrected to remove the erroneous physical interpretation.

"In the Nordic-W surface layer, small mean water volumes and high C_{Anth} concentrations (Fig. 3a, b), combined with a negative volume trend and a positive C_{Anth} concentration trend, result in a negative

volume contribution and a positive C_{Anth} contribution to the layer C_{Anth} inventory trend (Fig. 5d, e). Thus, the strong volume decrease (Fig. 5e) more than offsets the positive C_{Anth} concentration trend, resulting in a near-zero net layer inventory trend despite sustained C_{Anth} accumulation. This illustrates that volume changes can dominate over concentration trends in shaping the layer C_{Anth} inventory evolution. As a result, strong concentration increases appear partly muted in the decomposition, and the opposing contributions yield a near-zero net inventory trend.” (L. 394-400)

I.348

Explain here in the main text, what volume anomalies and Canth concentration residuals are, and not in the legend to Fig. 6.

The information has been added in the main text of the revised manuscript as follows: “[...] *water mass volume anomalies (difference from 1993 baseline) and C_{Anth} concentration residuals (deviation from expected linear trend) were computed to disentangle [...]*” (L.420-421)

I.359-360

,The Nordic W region exhibits large swings, broadly following NAO phases.’

This is wrong. Only the increase in the densest layer in 2014 is in line with the NAO. Before, the volume of the densest layer appears almost anticorrelated with the NAO.

We thank the reviewer for this careful observation. Upon re-examination of Fig. 6a, we partially agree that the NAO-volume anomaly relationship in the Nordic West region is not uniformly clear across the full record. The most robust expression of this relationship is visible post-2012, where sustained positive NAO phases are associated with clear increases in the densest layer volume ($\gamma_n > 28.16 \text{ kg m}^{-3}$) and concurrent negative anomalies in the surface layer ($\gamma_n < 26.7 \text{ kg m}^{-3}$), consistent with NAO-driven vertical redistribution: enhanced deep convection during positive NAO phases increases the volume of the densest layer while thinning the surface layer, with the opposite pattern during negative NAO phases. This antiphase signal between the surface and densest layers is also partially visible around 1997-2000, where a negative NAO phase coincides with negative dense-layer volume anomalies. However, we acknowledge that the 2000-2012 period shows a weaker and less systematic relationship, consistent with the reviewer's assessment. We have revised the text to make this mechanism explicit: “*The Nordic West region exhibits large interannual swings, particularly in the surface ($\gamma < 26.7 \text{ kg m}^{-3}$) and densest ($\gamma > 28.16 \text{ kg m}^{-3}$) layers, displaying an antiphase pattern consistent with NAO-driven vertical redistribution. This relationship is most clearly expressed after 2012, where positive NAO phases are associated with enhanced deep convection, increased dense layer volumes and reduced surface layer volumes.*” (L. 431-435)

I.361-362

,In both provinces, Canth residuals oscillate around zero rather than showing steady accumulation.’

That is true, but it is merely a consequence of the definition of the Canth residuals. The linear trend has been subtracted from the residuals, so there CANNOT be a steady accumulation.

The referee is correct that oscillation around zero is a mathematical artefact of the residual definition and carries no information in itself. This part of the sentence has been deleted in the revised manuscript.

I.363

,intermediate density classes (yellow and purple line)‘

What do you mean with ,intermediate? From your 7 density classes, these (3, yellow: subpolar Mode Water and 4, purple: Labrador SeaWater) are somehow in the middle. Regarding the water column, the SPMW is in the upper 1000m, LSW reaches from a few hundred down to ~2000 m. Regarding the classification, however, LSW for me is a deep water mass, as it is part of the lower AMOC limb and also called Upper North Atlantic Deep water (south of the Labrador Sea). I know that some people interpret LSW as a kind of mode water, but I think it has clearly be distinguished from the subpolar Mode water, which is mainly formed in the northeastern Atlantic. Please make clear what you mean.

In this sentence, the term intermediate referred to the seven selected density classes, as these layers correspond to the middle density intervals among the seven classes considered. The word intermediate has been replaced with “mid-range” in the revised manuscript.

I.368

,... in close agreement with Canth concentrations ...‘

I do not see a volume decline in the Labrador Sea of the LSW layer from 2000-2007. I only see a minimum in the Labrador Sea in 2006-2007.

The reviewer is correct that “marked decline during 2000-2007” was an imprecise description and an overstatement. Furthermore, we would like to clarify that the intended message was that this negative volume anomaly is mirrored in the C_{Anth} concentration residuals for the same layer, illustrating the coherence between the two metrics. The revised sentence now reads: “*For example, both Labrador and Irminger basins experienced a decline in the 27.8-28.0 kg m⁻³ layer during 2000-2007, followed by recovery; a signal mirrored in the C_{Anth} concentration residuals for the same layer (Fig. 6b; Fig. Supp. A5).*” (L. 439-441)

As supporting evidence, we have plotted the C_{Anth} concentration time series for both regions (Fig.3 below, not included in the revised manuscript). While the 2000-2007 sub-period does not exhibit a strongly negative linear trend in concentrations, the data points fall below the long-term trend line during this period, indicative of a negative anomaly coherent with the volume signal described in the text.

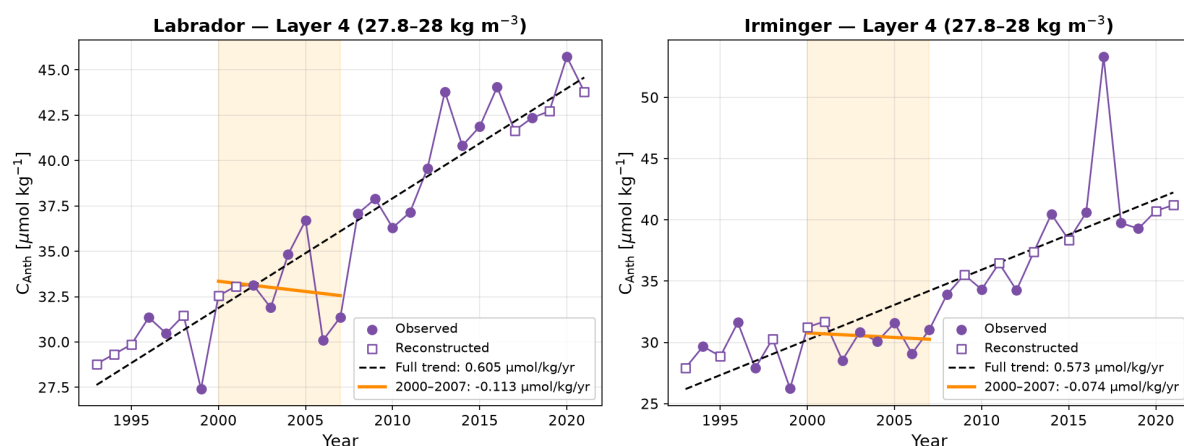


Figure 3 (not included in the revised manuscript). Time series of C_{Anth} concentrations for the Labrador (left panel) and Irminger (right panel) regions and the 27.8-28 kg m^{-3} density layer over the 1993-2021 time period. The orange line shows the linear sub-trend for 2000-2007 while the black dashed line indicates the long-term trend derived using all the C_{Anth} data, both calculated and derived from the linear fit applied to the available data.

L.369

‘... the densest layers show reduced ventilation ...consistent with the weaker Labrador Sea Water formation ...’

Which layers do you have in mind? The LSW layer is the $\gamma_n=27.8-28$ layer, all deeper/denser layers are not (directly) influenced by convection in the Labrador Sea.

This part of the sentence was about the denser 27.8-28.0 kg m^{-3} layer (in comparison with the 27.25-27.8 kg m^{-3} layer above). It has been clarified in the revised manuscript.

L.373

‘... and again from 2013-2017 ... (Yashayaev and Loder, 2016)’

How can a paper from 2016 say something about convection in 2017?!

In fact, the intense convection in the Labrador Sea continued until 2018, not only 2017 (see e.g. Yashayaev, 2024).

We thank the referee for having spotted this error. The reference has been updated in the revised manuscript, as well as the time period.

L.376-377

‘... positive residuals (e.g. early 1990s, 2013, 2017) alternating with negative phases (e.g. 2000, 2005-2008).’ I do not see these features in Fig. 6, and also not in Fig. 10 (where they would be better visible due to the larger figure size).

We thank the reviewer for raising this point. The features described in L.376-377 (now L. 449-450 in the revised manuscript) refer to anomalies relative to the long-term trends that are poorly visible in Fig.6 due to the size of the plot. To respond to the referee, we have plotted the C_{Anth} concentration time series for both regions and the relevant layers considered in this paragraph (i.e., 27.25-27.8 kg m^{-3} & 27.8-28.0 kg m^{-3} ; Fig.4 below, Fig. A4 in the revised manuscript), with long-term trends overlaid. The shaded periods highlight the positive anomaly years in green and red mentioned in the manuscript. Hence, in both basins and layers, the data points fall above the trend line during green periods (positive C_{Anth} anomalies) and below during red periods (negative C_{Anth} anomalies), except for the year 2000, which has been corrected in the revised manuscript (replaced with 1999).

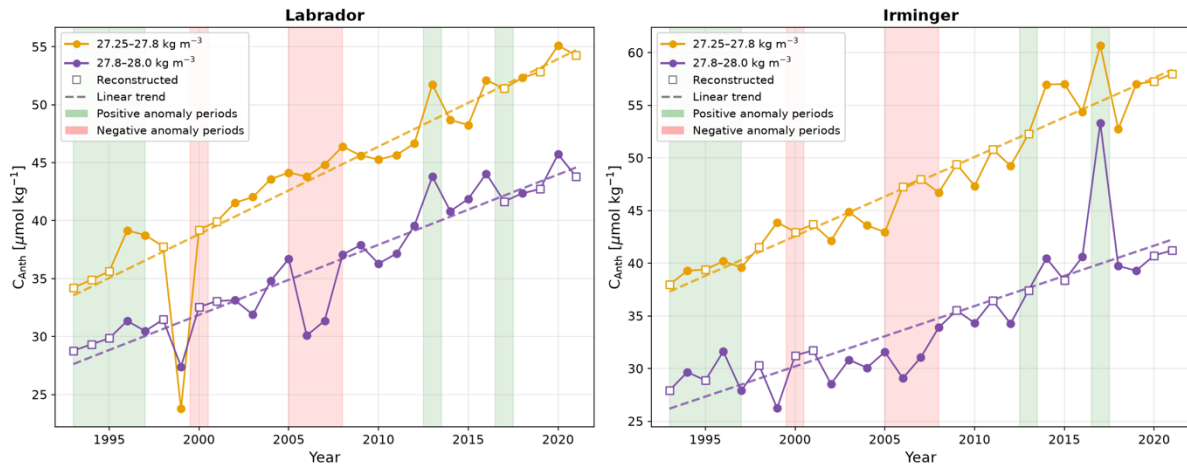


Figure 4 (Fig. A4 in the Supplementary Material in the revised manuscript). Time series of C_{Anth} concentrations for the Labrador (left panel) and Irminger (right panel) regions and the $27.25-27.8 \text{ kg m}^{-3}$ (yellow line) and $27.8-28.0 \text{ kg m}^{-3}$ (purple line) density layers over the 1993–2021 time period. The dashed lines represent the linear fits applied to the data and used to derive the long-term trends. Periods cited as positive phases (i.e., early 1990s, 2013 and 2017) and negative phases (i.e., 2000, 2005–2008) in the original manuscript have been highlighted in green and red, respectively.

I.378-380

,In the Nordic West province, sharp C_{Anth} deep density layer ($>28.16 \text{ kg/m}^3$) shifts in 2015 and 2017 (Fig. Supp. A4) align with slightly earlier volume anomalies in the same density class, a pattern also evident in the late 1990s.'

In the early 1990s, volume AND C_{Anth} anomalies are negative, in 2015 both anomalies have an opposite sign, hence it is not comparable.

We thank the reviewer for this observation and acknowledge that the text was ambiguous and that the comparison between the 2015 event and the late 1990s pattern was not justified. We have removed the reference to the late 1990s in the revised manuscript.

We would also like to highlight an issue identified during the revision of the figures asked by both reviewers, notably in Fig. 6 and Supp. A5. A careful re-examination of the plotting code revealed that an error had been made in the region labels displayed in these figures. We sincerely apologise for this oversight. Importantly, this labelling error does not affect the analyses, results, or text of the manuscript, as the underlying data and computations were correct throughout. The figures have been fully replotted with the corrected labels.

I.398-399

,In contrast, the Northeast region exhibits episodic positive deviations from the expected trend beginning in the late 2000s, including within the deepest density layers.'

I am pretty sure that the high C_{Anth} concentration anomaly in 2009 in the $28.08-28.16$ layer in the Northeast region is not real. The mean C_{Anth} concentration there is 9 μmol/kg (Fig.3), and the anomaly is at least as high, i.e. C_{Anth} would have doubled from one year to the other. This high value is probably either due to a regional bias of the single C_{Anth} points or it is due to a data quality issue.

We thank the reviewer for this careful observation. We examined in detail the individual profiles contributing to the 2009 annual mean in the Northeast 28.08-28.16 kg m⁻³ layer (see Fig.5 below, not in the revised manuscript). The high mean C_{Anth} value (17.2 $\mu\text{mol/kg}$ vs. 8-10 $\mu\text{mol/kg}$ in other years) is indeed driven by a single cruise (cruise 26) concentrated in the western part of the province ($\sim 47^\circ\text{N}$, $33-41^\circ\text{W}$). However, we find no justification for excluding these data: (1) all profiles passed our GLODAP data additional quality control (detailed in the manuscript), (2) the hydrographic properties (salinity $\sim 34.89-34.91$, temperature $\sim 2.2-2.6^\circ\text{C}$) are fully consistent with those of other years in this layer, and (3) the elevated C_{Anth} values in this cruise are coherent across all density layers, suggesting a physically consistent signal rather than a data artefact. In consequence, we prefer retaining these data in the analysis.

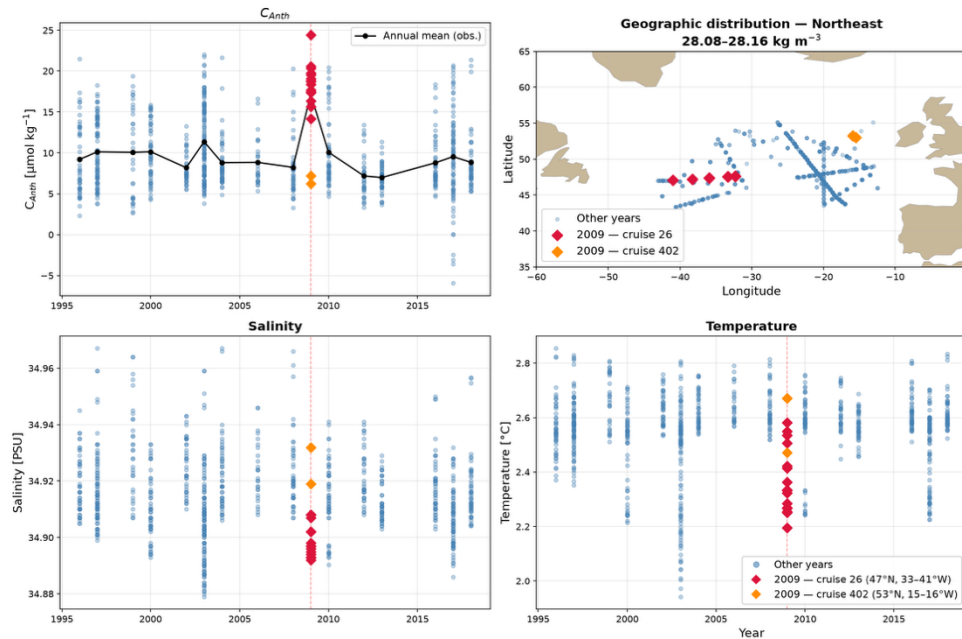


Figure 5 (not included in the revised manuscript). Examination of the C_{Anth} , salinity and temperature data acquired in 2009 in the Northeast region for the density interval 28.08-28.16 kg m⁻³.

However, we acknowledge that this case illustrates well the limitations associated with clustering the data: in years where sampling is spatially concentrated in one part of the region, the annual mean may not be fully representative of the full province and appears as an “outlier”. This echoes the reviewer's broader concern about spatial representativeness raised earlier. We have added a sentence to the manuscript to make this caveat explicit: *“Additionally, it can be noted that in years with sparse sampling concentrated in a sub-region, annual means may not be fully representative of the full province extent, as illustrated by the 2009 anomaly in the Northeast deepest layers where a single cruise sampling the western part of the province drives the high annual mean despite physically consistent hydrographic properties.”* (L. 469-472)

I.424-426

,While the basin-wide inventory increases steadily over the study period ($0.39 \pm 0.02 \text{ PgC/a}$), the magnitude and timing of this increase differs markedly between the five main regions.’ that the magnitude of the C_{Anth} storage varies between the main regions is mainly a consequence of the different volumes of these regions.

The referee is correct in stating that larger regions store more C_{Anth} than smaller regions, as expected and directly related to their size. In the revised manuscript, this paragraph has been simplified and clarified as follows: *“The temporal evolution of the total NA C_{Anth} inventory and the relative contribution of its major regions (Fig. 4) indicates a significant basin-wide inventory increase over the study period ($0.42 \pm 0.02 \text{ PgC a}^{-1}$), with long-term C_{Anth} inventory trends ranging from $0.004 \pm 5.4 \times 10^{-5} \text{ PgC a}^{-1}$ (Shelf Sea) $0.16 \pm 0.01 \text{ PgC a}^{-1}$ (Subtropical regions). As expected from the large differences in water-mass volumes, decomposing the inventory by region shows that subtropical and tropical provinces dominate the total C_{Anth} inventory over this timescale (ca. 40% and 30%, respectively), whereas subpolar and Nordic regions contribute smaller absolute amounts (22% and 4%, respectively). However, when expressed as area-normalised inventories (moles m^{-2}), a strikingly different picture appears: subpolar regions exhibit comparatively large storage per unit area (Fig. 4b), consistent with enhanced uptake and their contribution increases from 22% to ca. 125% when switching from PgC to moles m^{-2} (Fig. 4c), highlighting that volume alone does not capture the efficiency of carbon storage across regions”* (L. 318-326)

I.443-445

,In subtropical regions, inventory anomalies are particularly large (up to $\pm 0.05 \text{ PgC/a}$ in the West subtropical province)‘

That is correct, however, if I see it correctly, according to Supp. Fig. A5, these anomalies are not significant.

We agree that the original sentence requires more nuance. Looking at Supp. Fig. A6, some anomalies in the W Subtropical province are significant in some windows, notably around 2003-2004 and 2010-2011, but not systematically across the full record. The sentence has been revised as follows: *“In subtropical regions, inventory anomalies can be particularly large in magnitude (up to $\pm 0.04 \text{ PgC a}^{-1}$ in the W subtropical province), with episodic windows statistically significant ($p < 0.05$; (Supp. Fig. A6) reflecting the combined effect of concentration and volume changes on carbon storage; a behaviour consistent with the dominant role of volume transport identified by Bajon et al. (2026) along the A25-OVIDE section.”* (L. 497-500)

I.465

,... explains up to 40%...‘

This is true, but only for one region/layer. In most cases, the R^2 value is between 0 and 0.2. Please make that clear.

We agree with the referee that the original phrasing “explains up to 40%” was misleading, as it highlights only the maximum R^2 across all regions and layers without clearly stating that the NAO explains only a small fraction of interannual variance in most cases. In the revised manuscript, we would also like to point out that R^2 values are now higher. Hence, we have modified the sentence to give a more representative picture of the distribution of R^2 values: *“In these areas, while the R^2 values are typically low (0-0.2 across most regions and layers), the regression slopes (sensitivities) indicate that positive NAO phases are mostly associated with enhanced C_{Anth} content and storage. Hence, NAO variability can explain up to 80 % of the interannual variance in specific cases, particularly for the C_{Anth} concentration residuals, though these high values in data-sparse cells should be interpreted with caution given the limited degrees of freedom when only measured years are retained.”* (L. 521-525)

I.477-478

,In addition, the similarity in spatial patterns of inventory anomalies and water mass volume residuals in most subpolar regions (Fig. 7b & c) reflects the tight coupling between carbon concentration anomalies and water mass volumes.'

Note, however, that all correlations between NAO and Canth inventory residual in the subpolar regions are not significant.

We thank the reviewer for this important clarification: the NAO- C_{Anth} inventory residuals as well as NAO-water volume anomalies are indeed largely not statistically significant. Hence, the phrasing "tight coupling" was therefore overstated. In the revised manuscript, this statement has been toned down as follows: *"In addition, the similarity in spatial patterns of inventory anomalies and water mass volume residuals in most subpolar regions (Fig. 7b & c) is consistent with a coupling between carbon concentration anomalies and water mass volumes, although it has to be noted that the inventory-NAO correlations in most regions do not systematically reach statistical significance."* (L. 535-537)

I.485-490

it could also be possible that the waters exported from the subpolar North Atlantic, which are high in Canth during high NAO phases, arrive in the tropics/subtropics several years later, and then they are out of phase with the NAO. At least, the gyre circulation (subtropical gyre?) should only have a small impact on the deep water layers, where the negative inventory-NAO correlations are highest.

We thank the reviewer for these insightful suggestions. We agree on both points raised:

The lagged advection mechanism: we acknowledge that the negative NAO- C_{Anth} correlations in subtropical and tropical regions are more consistently explained by the delayed arrival of C_{Anth} -enriched subpolar waters exported southward via the Deep Western Boundary Current during high-NAO periods, rather than by a direct local NAO response. We have revised the text accordingly.

Secondly, we agree that using gyre circulation as an explanation for the deep density layers is physically inconsistent, as the subtropical gyre is largely confined to surface and intermediate layers and cannot account for negative correlations in the densest classes. We have therefore restricted the gyre circulation argument to surface and intermediate layers only, and attributed the deep-layer signal to the lagged advection mechanism described above.

The manuscript has been revised as follows: *"Negative or near-zero C_{Anth} inventory sensitivities are prevalent in subtropical and tropical regions and within most intermediate and densest layers, perhaps reflective of their distance from active ventilation zones. Similarly, C_{Anth} concentration residuals are mostly negatively correlated with NAO, suggesting that the physical mechanisms driving C_{Anth} concentration variability also drive inventory residuals. In surface and intermediate layers, these negative slopes may reflect NAO-driven shifts in gyre circulation that displace carbon-rich waters away from these regions during positive NAO phases. However, for deeper density layers, a physically consistent explanation could be the lagged advection of C_{Anth} -enriched subpolar waters exported southwards via the Deep Western Boundary Current during high-NAO periods (Smethie and Fine, 2001). These waters arrive in subtropical and tropical regions with a delay of several years, by*

which time the NAO may have shifted phase, hence generating an apparent negative correlation between NAO and C_{Anth} reflecting remote subpolar forcing rather than a direct local response.” (L. 542-552)

I.503-505

,These strong into-the-ocean fluxes imply that mode waters formed predominantly during positive NAO phases initially have low pCO₂ values, introducing a negative relationship between local mode-water C_{Anth} content and the NAO.’

I do agree that the newly formed mode waters are still undersaturated with respect to CO₂. But do they really have lower C_{Anth} concentrations than the old mode waters which they replace? In addition, outcropping of mode waters would hardly reduces their C_{Anth} concentration.

We agree with the referee. We have revised the sentence to reflect the correct mechanisms we wanted to propose. The negative local relationship between mode-water C_{Anth} content and the NAO is better understood in terms of residuals relative to the long-term trend rather than absolute C_{Anth} concentrations. During positive NAO phases, enhanced mode water formation produces a larger water mass volume. Even if C_{Anth} concentrations continue to increase in absolute terms, the concentration increase per unit volume may be slower than the long-term background trend, yielding a negative C_{Anth} concentration residual. Furthermore, since net volume anomalies remain small, inventory residuals are similarly negative, reflecting growth slower than expected rather than an actual decrease. The revised text now reads: “*Consistent with this, Andersson et al. (2013) and Bates (2007) reported large oceanic CO₂ uptake (negative sea-air flux) in newly formed, outcropping subtropical mode waters. Hence, the negative local relationship between mode-water C_{Anth} content and the NAO might likely reflect a dilution effect relative to the long-term trend: during positive NAO phases, enhanced mode water formation increases water mass volumes, such that C_{Anth} concentrations, while continuing to increase in absolute terms, rise more slowly than the long-term trend, producing negative concentration residuals.*” (L. 563-568)

I.510-511

,... in newly formed, outcropping subtropical mode waters. However, it can be noted that subtropical and tropical regions tend to be more influenced by advection and air-seaCO₂ exchanges than water volume variations.’

This is true, but the reason for this is mainly the relatively small change in water mass volumes in the (sub-)tropics. Note that these regions have much larger volumes than the Nordic/Subpolar regions, but the volume anomalies are of the same size ($2 \times 10^{14} \text{ m}^3$ for the most variable layers).

The referee highlighted a valid and important point which was not present in the original manuscript. We have revised the sentence to explicitly state that the weaker influence of volume variations in subtropical and tropical regions stems from their relatively small fractional volume anomalies: “*However, it can be noted that subtropical and tropical regions tend to be more influenced by advection and air-sea CO₂ exchanges than water volume variations, partly because despite their large total volumes, the absolute volume anomalies in these regions are of comparable magnitude to those in the subpolar and Nordic regions, resulting in relatively small fractional volume changes.*” (L. 573-576)

I.519

,accumulated AMOC'

Please explain what is meant by this (see remark above). If I understand correctly, the AMOC anomalies are summed up over time. However, I doubt how reasonable this is. In Bryden et al. (2014) this was done for a few years after the AMOC decline at the RAPID array. But is it really reasonable for a 28 years period? There are a kind of ,saturation' effects, e.g. the volume of LSW in the Labrador Sea cannot increase more and more despite of intense convection/formation and high AMOC, because it already reaches close to the surface after a few years of intense convection. Also the Canth saturation does not steadily increase for a period of years with strong convection (see Fig. 10).

Both referees requested further detail on the use of the accumulated AMOC. In response to their comments and suggestions, we have added an explicit methodological description in Section 2.4 of the revised manuscript: *“In this study, rather than raw transport signals, 12-month running means of AMOC strength were used to reduce short-term and seasonal variability (Jackson et al., 2019) and facilitate comparison with model outputs. Following Bryden et al. (2014), these values were then used to derive the accumulated AMOC anomalies by integrating AMOC deviations at both sites from a 10-year moving baseline, providing a physically consistent measure of the cumulative circulation anomaly that integrates persistence and magnitude over multi-year timescales (see Supp. Fig. A1). This avoids spurious correlations with short-term variability and better reflects the long-term redistribution of heat, freshwater, and carbon by the overturning circulation. Hence, the 12-month running mean values characterise the temporal evolution of AMOC transport while the accumulated anomaly captures the integrated, multi-year influence of overturning variability on carbon storage.”* (L. 188-195)

We believe that this new paragraph addresses the reviewer's concern about applying this over a 28-year period. Indeed, the accumulation was not performed over the full record: anomalies were computed relative to a rolling 10-year baseline (specifically a 5-year reference window followed by a 5-year comparison window), meaning the baseline shifts continuously through time. This prevents unbounded accumulation and the saturation effects the reviewer rightly highlights: if AMOC volumes plateau, the anomaly relative to the rolling baseline approaches zero. This metric thus captures how much AMOC has deviated from its recent past state at any given time, rather than from a fixed origin. We acknowledge that the relationship between accumulated AMOC and C_{Anth} need not be strictly linear over all timescales, and the approach is used here as a diagnostic metric rather than a predictive model. A sentence acknowledging this limitation has been added in the revised manuscript at the end of the added paragraph, as follows: *“However, it should be noted that this accumulated metric is used diagnostically: non-linear effects such as density layer saturation or C_{Anth} equilibration during sustained AMOC anomalies may limit strict linear interpretation over long periods.”* (L. 195-197)

I.524

,... mode-water formation in the subpolar gyre'.

Do you mean Subpolar Mode Water (SPMW) or Labrador Sea Water (LSW) or both? Also note the ,dipole' in the Labrador and Irminger Sea between the SPMW(27.25-27.8, negative correlation with OSNAP and partly with NAO) and LSW (27.8-28, positive correlation with OSNAP and NAO).

As highlighted by the referee, the wording in the original manuscript was unclear, and the term “mode-water” was ambiguous. This has been clarified in the revised manuscript: *“Stronger accumulated*

AMOC phases enhance deep convection in the subpolar gyre (e.g., Lozier et al., 2019; Dima et al., 2022), driving the formation of Labrador Sea Water ($\gamma_n = 27.8\text{-}28 \text{ kg m}^{-3}$) at the expense of the overlying Subpolar Mode Water ($\gamma_n = 27.25\text{-}27.8 \text{ kg m}^{-3}$), producing the observed dipole in C_{Anth} and volumes sensitivities across these two density classes in the Labrador and Irminger basins (Fig.8a & b).” (L. 593-596)

L.530-532

,In several northern provinces, densest layers exhibit positive concentration anomalies but weak or negative inventory anomalies because enhanced overturning shoals or thins these density classes.’ I do not see any negative correlation between the AMOC at OSNAP and the volume of dense northern layers, only the opposite (Fig. 8b).

Upon close examination of the slopes presented in Fig. 8a-c, we agree with the referee that the original statement was too broad. We have carefully re-examined the regression slopes for all six northern provinces and the three densest layers ($28.08 - >28.16 \text{ kg m}^{-3}$), as shown in Table 1 below (not in the revised manuscript). This analysis reveals a more nuanced picture than the original text suggested. The combination of negative volume slopes (shoaling), positive concentration slopes, and weak or negative inventory slopes is most clearly observed in the Irminger Sea across all three dense layers, and partially in Nordic East at $28\text{-}28.08 \text{ kg m}^{-3}$. However, Nordic West shows the opposite volume response (strongly positive, $+1.457 \times 10^{13} \text{ m}^3$ for $>28.16 \text{ kg m}^{-3}$) (which dominates the visual impression of Fig. 8b). Labrador and Iceland show near-zero volume slopes with inconsistent concentration patterns. We therefore agree that the original statement was overstated, and have revised the manuscript to restrict this observation to the provinces where it is supported: *“In the Irminger Sea in particular, the densest layers ($\gamma_n = 28$ to $> 28.16 \text{ kg m}^{-3}$) exhibit positive C_{Anth} concentration anomalies alongside near-zero or weakly negative inventory anomalies under enhanced accumulated AMOC, consistent with shoaling of these density classes (e.g. Mork et al., 2025). This signal is also partly detectable in Nordic East but not a general feature of all northern provinces”* (L. 600-604)

Table 1 (not included in the revised manuscript). Regression slopes between the water mass volume anomalies (relative to 1993 baseline), C_{Anth} concentration residuals (observed minus long-term linear trend), and C_{Anth} inventory residuals (observed minus long-term linear trend) and the Atlantic Meridional Overturning Circulation (AMOC) at OSNAP. Units: volumes $\times 10^{13} \text{ m}^3 (\text{Sv a}^{-1})^{-1}$; concentrations $\mu\text{mol kg}^{-1} (\text{Sv a}^{-1})^{-1}$; inventories PgC $(\text{Sv a}^{-1})^{-1}$. Only the densest layers $\gamma_n = 28$ to $> 28.16 \text{ kg m}^{-3}$ are shown. As in Figs. 7 and 8 in the revised manuscript, slopes were computed with data “exclusively from years with direct observational coverage” (see our response to the referee p.2-3).

Volumes anomalies ONSAP						
	NordicE	NordicW	Labrador	Irminger	Iceland	Northeast
	1.0e+13 *					
28-28.08	-0.0837	0.1302	-0.0006	-0.0163	-0.003	-0.2849
28.08-28.16	-0.1366	0.0771	0.0017	-0.0126	0.0006	-0.0118
>28.16	0.0081	1.4573	-0.001	-0.0099	-0.009	0
Concentrations residuals ONSAP						
	NordicE	NordicW	Labrador	Irminger	Iceland	Northeast
28-28.08	1.0859	-0.3545	0.1883	0.7683	-0.7349	-0.5276
28.08-28.16	-0.4969	0.3906	0.4615	1.0826	-0.8335	-0.9935
>28.16	-0.6948	-0.4644	0.6753	0.8771	1.0023	0
Inventory residuals ONSAP						
	NordicE	NordicW	Labrador	Irminger	Iceland	Northeast
28-28.08	0.0002	0.0008	0.0011	0.0012	-0.0002	-0.0052
28.08-28.16	-0.0005	0.001	0.001	0.0002	0	-0.0072
>28.16	-0.0037	-0.0035	0.0002	0	0 NaN	

I.537-539

,This behaviour reflects classical density-layer heave (e.g. Eden and Willebrand, 2001), whereby accumulated AMOC-driven renewal ventilates intermediate waters but compresses deeper ones...
Again, I cannot find an example for that in Fig. 8b.

The reviewer is correct that, while the volume anomalies should highlight the statement, the compression of the deepest density classes ($>28 \text{ kg m}^{-3}$) is not clearly visible in Fig. 8b (in the manuscript). However, we believe that the density-layer heave signal described in the paragraph is in fact present in the figure, most prominently as the near-perfect volumetric dipole between the intermediate density layers SPMW ($27.25\text{-}27.8 \text{ kg m}^{-3}$) and LSW ($27.8\text{-}28 \text{ kg m}^{-3}$) in the Labrador and Irminger basins. The examination of the regression slopes (Table 2 below, not in the revised manuscript) confirms this:

Table 2 (not included in the revised manuscript). Regression slopes between the water mass volume anomalies (relative to 1993 baseline) and the Atlantic Meridional Circulation (AMOC) at ONSAP. Units: volumes $\times 10^{13} \text{ m}^3 (\text{Sv a}^{-1})^{-1}$.

Volumes anomalies ONSAP						
	NordicE	NordicW	Labrador	Irminger	Iceland	Northeast
	1.0e+13 *					
<26.7	-0.0449	-2.0545	0.0188	0.0132	-0.0371	0.3034
26.7-27.25	0.106	0.0122	0.001	-0.0019	-0.0337	-0.0479
27.25-27.8	0.2829	0.2576	-0.8937	-1.8293	-0.2311	-0.3744
27.8-28	-0.1055	0.4794	0.8894	1.8643	0.3659	0.4157
28-28.08	-0.0837	0.1302	-0.0006	-0.0163	-0.003	-0.2849
28.08-28.16	-0.1366	0.0771	0.0017	-0.0126	0.0006	-0.0118
>28.16	0.0081	1.4573	-0.001	-0.0099	-0.009	0

Hence, we think that the near-mirror symmetry of these slopes is a classic signature of isopycnal heave, as described in the original manuscript: under enhanced accumulated AMOC, convection converts lighter SPMW into denser LSW, expanding the LSW layer at the expense of the overlying SPMW (as described in Eden and Willebrand, 2001). However, we agree that the compression of the overflow-

dense layers ($>28 \text{ kg m}^{-3}$) is less clearly evident in our results. The revised manuscript has been modified to moderate this points as follows: “*This behaviour is consistent with classical density-layer heave (e.g., Eden and Willebrand, 2001), whereby accumulated AMOC-driven renewal ventilates intermediate waters - most clearly manifested as the near-mirror volumetric dipole between SPMW and LSW in the Labrador and Irminger basins (Fig.8b) - consistent with previous evidence that overturning variability governs the sequestration of anthropogenic carbon in intermediate densities (Pérez et al., 2013; Fröb et al., 2016).*” (L. 609-613)

I.544-546

,This reflects its downstream position within the overflow-fed circulation of the subpolar North Atlantic: during strong accumulated AMOC phases, the basin is flushed by recently ventilated overflow waters low in Canth. Thus, overturning enhances renewal and slightly expands volumes, but reduces local Canth concentrations and inventories.’

Why should recently ventilated overflow waters be low in Canth?! LSW is higher in Canth, indeed, but that belongs to another density layer. In addition, it is new to me that the Iceland basin is ,flushed’ by overflow waters during strong AMOC phases. So far as I know, the overflow transports are fairly constant in time. Please give a citation here that proves strong overflows in concert with strong AMOC at OSNAP or remove this statement.

The reviewer raises two valid and important points, and we thank them for the precision. (1) We agree that characterising overflow transports as variable with AMOC is not well supported. Overflow transports through the Iceland-Scotland Ridge are indeed relatively stable in time (Østerhus et al., 2019), and we have removed this statement in the revised manuscript. (2) We also acknowledge that the question of why recently ventilated overflow waters would carry low C_{Anth} was not adequately supported. The revised text removes the mechanistic interpretation and instead presents the Iceland Basin negative AMOC- C_{Anth} relationship as an empirical contrast with neighbouring convective provinces, noting that its position downstream of the Nordic Seas overflow pathway likely contributes to its distinct behaviour, but without speculating a specific mechanism. The revised text reads: “*A notable exception in the subpolar gyre is the Iceland Basin, where both C_{Anth} concentration and inventory anomalies (Fig. 8a & c) show strong negative or null correlations with accumulated AMOC (i.e., C_{Anth} increases more slowly than the long-term trend during strong AMOC phases), despite weakly positive volume anomalies (Fig. 8b). This contrasts markedly with the neighbouring subpolar provinces (i.e., Labrador and Irminger basins) and likely reflects the Iceland Basin’s distinct position at the interface between the Nordic overflow pathway and the subpolar convective regime, though the precise mechanism driving its negative AMOC- C_{Anth} relationship remains difficult to isolate with our data.*” (L. 613-618)

Østerhus, S., Woodgate, R., Valdimarsson, H., Turrell, B., de Steur, L., Quadfasel, D., Olsen, S. M., Moritz, M., Lee, C. M., Larsen, K. M. H., Jónsson, S., Johnson, C., Jochumsen, K., Hansen, B., Curry, B., Cunningham, S., and Berx, B.: Arctic Mediterranean exchanges: a consistent volume budget and trends in transports from two decades of observations, *Ocean Sci.*, 15, 379–399, <https://doi.org/10.5194/os-15-379-2019>, 2019.

I.550-554

Indeed, waters formed in the Nordic Seas and subpolar gyre experience short surface exposure times and thus do not fully equilibrate with rising atmospheric CO₂ (Olsen et al., 2010). During strong accumulated AMOC phases, a greater fraction of these young and recently ventilated waters

which can exhibit low apparent C_{Anth} concentrations (i.e., because they were not fully equilibrated with atmospheric CO_2 when ventilated; Matsumoto and Gruber, 2005) is flushed downstream, reducing local concentrations even as ventilation increases.’

At least for LSW this is wrong, as the C_{Anth} concentration residuals of the LSW layer in the Labrador and Irminger Sea are positively correlated with the AMOC.

The reviewer is correct. The original text incorrectly extended the carbon disequilibrium argument to waters from the “subpolar gyre” broadly, which implicitly includes LSW. Our regression results confirm that C_{Anth} concentration residuals in the LSW density class ($27.8\text{--}28.0 \text{ kg m}^{-3}$) are positively correlated with accumulated AMOC in both the Labrador ($+0.318 \text{ } \mu\text{mol kg}^{-1} \text{ Sv a}^{-1}$)⁻¹ and Irminger ($+1.022 \text{ } \mu\text{mol kg}^{-1} \text{ Sv a}^{-1}$)⁻¹ basins. In these provinces, enhanced overturning intensifies deep convection and subducts C_{Anth} -enriched surface waters into the LSW density class, producing a positive (not negative) AMOC- C_{Anth} signal. The revised text now explicitly restricts this mechanism to the Nordic provinces and excludes LSW. Given also the reviewer's preceding comment (about 1.544-546 in the original manuscript) regarding the stability of overflow transports, we have been careful not to link this disequilibrium signal mechanistically to AMOC variability in the Iceland Basin, which is treated separately and more cautiously in the revised text (see our response to the comment above). *“Additionally, carbon disequilibrium of newly ventilated waters may further contribute to the negative accumulated AMOC- C_{Anth} correlations observed in the Nordic provinces specifically. Indeed, waters formed in the Nordic Seas experience short surface exposure times before subducting, and thus do not fully equilibrate with rising atmospheric CO_2 (Matsumoto and Gruber, 2005; Olsen et al., 2010), resulting in low apparent C_{Anth} . This mechanism does not apply to convectively formed intermediate waters such as LSW in the Labrador or Irminger basins, where enhanced overturning subducts C_{Anth} -enriched surface waters downward, producing a positive AMOC- C_{Anth} relationship (Fig. 8a-c).”* (L. 618-624)

I.557-558

,These correlations likely reflect changes in net meridional export associated with high-latitude overturning variability, rather than direct advective connectivity.’

Net meridional export provides an advective connectivity, I do not see the difference.

The referee is correct, and the last part of the sentence has been deleted in the revised manuscript.

I.561-563

,In contrast, tropical provinces show mostly positive correlations with OSNAP and RAPID, consistent with downstream dilution of signals through mixing and recirculation.’

If the signals are diluted, the correlation should decrease/vanish. In addition, the recirculation would disturb a clear temporal correlation between C_{Anth} and AMOC.

We agree with the referee that the words “dilution” and “recirculation” were not adequate in the original sentence as they imply signal loss rather than signal transport. The intention was to express the downstream transport of C_{Anth} -rich waters from the subpolar region, which are transported via the DWBC. Indeed, we believe that the positive (sub)tropical correlations, while weak and/or non-significant, are consistent with attenuation over distance, and are physically coherent with the

southward export of C_{Anth} -enriched waters via the deep return limb of overturning circulation. In the revised manuscript, the phrasing has been corrected as follows: “*In contrast, tropical provinces show mostly positive correlations with accumulated AMOC at both OSNAP and RAPID, consistent with the southward advection of C_{Anth} -enriched deep waters via the return limb of the overturning circulation.*” (L. 630-632)

L.571-573

,In the subpolar provinces, surface layers generally show positive slopes: when accumulated AMOC is stronger, enhanced overturning injects Canth -rich surface waters into the upper subpolar gyre, increasing carbon concentrations.’

I do not see a large difference between the Canth-AMOC correlation between RAPID and OSNAP for the upper subpolar layers. Only in the Labrador Sea, this correlation is positive for the upper layers, but for both AMOC estimates, at OSNAP and RAPID.

Upon close examination of the slopes presented in Fig. 8a-c, we agree with the referee that the original statement was incomplete: slopes are similar at both sites. We have carefully re-examined the regression slopes for all six northern provinces and the two lightest layers (< 26.7 to 27.25 kg m^{-3}), as shown in Table 3 below (not in the revised manuscript), and concluded that the positive signal is present for both RAPID and OSNAP. We have restricted the statement accordingly in the revised manuscript: “*Positive surface-layer slopes are observed primarily in the Labrador Sea for both RAPID and OSNAP, indicating that when accumulated AMOC is stronger, enhanced overturning injects C_{Anth} -rich surface waters into the upper subpolar gyre, increasing carbon concentrations. A weaker positive signal is also detected in the Iceland basin for RAPID only, representing one of the genuine differences in spatial structure between the two AMOC estimates. However, as shown in Fig. 3b, these lightest density layers ($26.7 < \gamma_n < 27.25 \text{ kg m}^{-3}$) occupy very small volumes relative to intermediate water masses, thus contributing only marginally to the overall regional C_{Anth} inventory.*” (L. 640-646)

Table 3 (not included in the revised manuscript). Regression slopes between the water mass volume anomalies (relative to 1993 baseline), C_{Anth} concentration residuals (observed minus long-term linear trend), and C_{Anth} inventory residuals (observed minus long-term linear trend) and the Atlantic Meridional Overturning Circulation (AMOC) at OSNAP (left panel) and RAPID (right panel). Units: volumes $\times 10^{13} \text{ m}^3$ (Sv a^{-1}) $^{-1}$; concentrations $\mu\text{mol kg}^{-1}$ (Sv a^{-1}) $^{-1}$; inventories PgC (Sv a^{-1}) $^{-1}$. Only the lightest layers $26.7 < \gamma_n < 27.25 \text{ kg m}^{-3}$ are shown. As in Figs. 7 and 8 in the revised manuscript, slopes were computed with data “exclusively from years with direct observational coverage” (see our response to the referee p.2-3).

Volumes anomalies OSNAP						
	NordicE	NordicW	Labrador	Irminger	Iceland	Northeast
	1.0e+13 *					
<26.7	-0.0449	-2.0545	0.0188	0.0132	-0.0371	0.3034
26.7-27.25	0.106	0.0122	0.001	-0.0019	-0.0337	-0.0479
Concentrations residuals OSNAP						
	NordicE	NordicW	Labrador	Irminger	Iceland	Northeast
<26.7	-0.7055	-0.2605	0.5462	-0.9963	-0.0625	0.0914
26.7-27.25	-0.4061	1.1349	0.4325	-0.06	-0.1362	0.1076
Inventory residuals OSNAP						
	NordicE	NordicW	Labrador	Irminger	Iceland	Northeast
<26.7	-0.0005	-0.0014	-0.0001	-0.0004	-0.0006	0.0035
26.7-27.25	0.0003	0.0003	0	-0.0012	-0.0011	-0.0005

Volumes anomalies RAPID						
	NordicE	NordicW	Labrador	Irminger	Iceland	Northeast
	1.0e+13 *					
<26.7	-0.0227	-0.6913	-0.0499	0.0294	0.0232	0.4552
26.7-27.25	0.0627	0.0125	-0.0458	0.104	0.1437	1.1775
Concentrations residuals RAPID						
	NordicE	NordicW	Labrador	Irminger	Iceland	Northeast
<26.7	1.3886	-0.6225	0.7123	0.1936	0.3448	-0.4595
26.7-27.25	0.2727	0.1509	0.4023	-0.0554	0.0997	-0.2145
Inventory residuals RAPID						
	NordicE	NordicW	Labrador	Irminger	Iceland	Northeast
<26.7	-0.0001	-0.0086	-0.0004	-0.0001	0.0004	0.0024
26.7-27.25	0.0001	0.0001	-0.0005	0.0008	-0.0006	0.0072

I.573-575

,In contrast, intermediate and deep density layers exhibit negative slopes, as strong accumulated AMOC intensifies subpolar convection and exports recently ventilated, relatively low-Canth waters southward.'

The correlation between the AMOC at RAPID and Canth of the deep subpolar layers is indeed negative. However, for the ventilation variability in the subpolar North Atlantic, the NAO and the AMOC at OSNAP are better proxies than the AMOC at RAPID, and for them, the correlation is positive. I also do not understand why recently ventilated waters should be low in Canth. The Canth residuals of the LSW and most other layers in the subpolar North Atlantic are positively correlated with NAO (Fig. 7a), which influences/strengthens convection.

As correctly raised by the referee, the information about "which AMOC measurement site" was producing the negative slopes was missing in the original manuscript. Indeed, these negative relationships are only observed at RAPID and not at OSNAP. This has been clarified in the revised manuscript. Secondly, we note that the negative RAPID-subpolar concentration correlation occurs without corresponding volume changes, which rules out simple export as the mechanism. We have revised the text to acknowledge that this signal likely reflects a modification in water mass composition or a lagged relationship between subtropical AMOC transport and subpolar carbon content, rather than a directly interpretable ventilation signal, and that RAPID is not an appropriate proxy for subpolar ventilation variability as relevantly suggested by the referee. The revised sentence reads as follows: "*In contrast, intermediate and deep density layers in the subpolar region exhibit negative concentration slopes with accumulated RAPID AMOC, without corresponding volume changes. This suggests a modification in water mass composition rather than export. However, given that RAPID is not a direct proxy for subpolar ventilation, a robust mechanistic interpretation of this signal is not straightforward. We note that the same layers show positive correlations with accumulated OSNAP AMOC and NAO (Fig.8a & Fig.7a), confirming that local ventilation enriches rather than depletes subpolar C_{Anth} , though recently ventilated waters may show concentrations lower than the long-term trend, even if C_{Anth} remains increasing in absolute terms. The RAPID signal likely reflects a distinct, possibly lagged relationship between subtropical overturning transport and subpolar carbon content.*" (L. 646-653)

I.575

,...Canth at RAPID...'

What is this?! It sounds like the local Canth concentration at the location of the RAPID array, but this not shown at all.

This sentence was referring to the C_{Anth} concentration residuals compared against the AMOC measured at RAPID. In the revised manuscript, this sentence has been deleted as the above line now clearly states that the paragraph and comparison are about RAPID.

I.578-579

,Here, stronger accumulated AMOC enhances southward return flow and retention within recirculating pathways'.

First, the AMOC is identical with the 'southward return flow' (upper and lower limb of the AMOC have the same strength). Second, is the recirculation really increasing? A recirculation does not increase the net southward flow. And why should a recirculation increase Canth? Recirculation makes waters older, and older waters normally have lower Canth concentrations.

The three points raised by the referee have been implemented in the revised manuscript. Thus, we have removed the claim that stronger AMOC “enhances southward return flow”, and the recirculation narrative (which does not increase net southward transport and would age waters rather than enrich them). The revised text proposes instead that positive C_{Anth} concentration slopes in subtropical intermediate and deep layers reflect the arrival of recently ventilated, C_{Anth} -enriched waters via the deep return limb of the overturning, progressively replacing older subtropical intermediate waters at constant volume. The manuscript has been modified as follows: *“In the subtropical region, the vertical pattern is reversed: intermediate and deeper density layers show positive slopes, likely reflecting the arrival of C_{Anth} -enriched waters via the southward return limb of the overturning at constant volume. Upper subtropical layers show weaker or mixed slopes, reflecting competing influences between northward export of surface waters via the upper AMOC limb and equatorward export of subtropical mode waters via the tropical-subtropical cell.”* (L. 653-657)

I.580-581

,Upper subtropical layers may show weaker or mixed slopes due to competing influences of northward export and local recirculation.’

Note that subtropical mode water is also exported southward (equatorward) in the tropical-subtropical cell.

We thank the reviewer for this addition, which has been incorporated in the revised manuscript, replacing the “local recirculation” term with the more physically appropriate (tropical-subtropical cell) export pathways. The revised sentence is: *“Upper subtropical layers show weaker or mixed slopes, reflecting competing influences between northward export of surface waters via the upper AMOC limb and equatorward export of subtropical mode waters via the tropical-subtropical cell.”* (L. 655-657)

I.582-584

,Stronger accumulated AMOC here corresponds to greater accumulation of carbon-rich water along advective pathways, producing consistent positive anomalies.’ I do not see why this effect should be most pronounced in the tropics.

The referee is correct: there is no physical justification for the tropical signal being more pronounced than the subtropical one, and the phrasing overstated the consistency of the tropical response. The sentence has been removed in the revised manuscript.

I.584

,heatmap’

Please refer to figure number here.

,blue-red pattern’

Please rephrase (correlation-anti-correlation pattern), the color scale is arbitrary.

As suggested by the referee, the word “heatmap” has been replaced with “Figure 8” in the revised manuscript. Additionally, the blue-red wording has been replaced with “anti-correlation to correlation pattern”

I.586 ,subpolar intermediate waters‘

Do you refer to LSW? That is a deep water mass (see remark above).

,incoming northern-sourced waters ...‘

What do you mean with ,northern‘? The Nordic Seas or the subpolar region?

Following all the modifications done in the revised manuscript, the whole sentence has been rewritten as follows: *“Overall, Figure 8 exhibits the two-limb structure of the AMOC: the northward upper-limb export depletes surface subtropical C_{Anth} , while the southward deep return enriches intermediate and deep subtropical and tropical layers, producing the observed distinct anti-correlation to correlation pattern separation at RAPID toward the south.”* (L. 658-661)

I.590-593 ,This dual modulation of tracer content and layer geometry produces the strongest multi-decadal accumulated AMOC sensitivity in intermediate density classes, where both mechanisms act together. ,

Again, LSW, where the signal is strongest, is a deep water mass.

,It also clarifies why the intermediate density layer highlighted by Müller et al. (2023) exhibits a particularly pronounced accumulated AMOC imprint: it lies at the transition between subpolar ventilation and subtropical redistribution.‘

I cannot see this ,particularly pronounced accumulated AMOC imprint‘. Please refer to a figure.

The paragraph has been corrected as suggested: the “deep layers” words have been added, a reference to Fig. 8c & f has been added, and the statement toned down: *“This dual modulation of tracer content and layer geometry produces the strongest multi-decadal accumulated AMOC sensitivity in intermediate and deep density classes, where both mechanisms act together. It also clarifies why the density layer at the transition between subpolar ventilation and subtropical redistribution, highlighted by Müller et al. (2023), exhibits a detectable accumulated AMOC imprint: both subpolar ventilation signals and subtropical redistribution signals converge at this density horizon (Fig.8c & f).”* (L. 664-669).

I.597 ,... enhanced subpolar storage of anthropogenic carbon and volume increases in intermediate layers‘ Again, note the SPMW/LSW dipole in the Labrador and Irminger Sea

The reviewer is correct: the original manuscript oversimplified the volume response in the Labrador and Irminger Seas by referring generically to “volume increases in the intermediate layers”. The text has been modified in the revised manuscript: *“A feature of the analysis is the contrast between the relationship with the accumulated AMOC at OSNAP and RAPID (Fig. 8a-c & d-f). At OSNAP, we see the upstream imprint of overturning variability, where stronger accumulated AMOC translates into enhanced subpolar C_{Anth} storage driven by LSW expansion in the Labrador and Irminger Seas, while the overlying SPMW layer simultaneously thins, hence reflecting the heave dipole (Fig. 8b). Concurrently, subtropical reservoirs at equivalent LSW density ($\gamma_n = 27.8-28 \text{ kg m}^{-3}$) show negative inventory slopes (Fig. 8c), consistent with subpolar deep convection drawing on subtropical water mass reservoirs at equivalent density.”* (L. 683-688)

I.597-598

,compensated by negative Canth concentrations residuals in subtropical reservoirs’

Which layers do you have in mind? I still do not see a physical mechanism for this anticorrelation, which is also restricted to a few layers.

As shown in Table 4 below (not in the revised manuscript), the inventory slopes at LSW density (27.8-28 kg m⁻³) in the W and E subtropical regions are -0.0221 and -0.0816 PgC (Sv a⁻¹)⁻¹, respectively. However, we acknowledge that the precise mechanisms driving the negative subtropical inventory residual slopes at LSW density remain uncertain, and we have revised the manuscript accordingly to display it as an observation rather than committing to a specific dynamical interpretation. It has been revised (see the modified paragraph in the former comment’s response) to refer to inventory anomalies at LSW density specifically, with reference to Fig. 8c.

Table 4 (not included in the revised manuscript). Regression slopes between the C_{Anth} inventory residuals (observed minus long-term linear trend) and the Atlantic Meridional Overturning Circulation (AMOC) at OSNAP. Units: inventories PgC (Sv a⁻¹)⁻¹. The SPMW ($\gamma_n = 27.25\text{-}27.8$ kg m⁻³) and LSW ($\gamma_n = 27.8\text{-}28$ kg m⁻³) layers have been highlighted in blue in the subpolar and subtropical provinces cited in the response of the referee. As in Figs. 7 and 8 in the revised manuscript, slopes were computed with data “exclusively from years with direct observational coverage” (see our response to the referee p.2-3).

Inventory residuals OSNAP													
	NordicE	NordicW	Labrador	Irminger	Iceland	Northeast	European	Gulf Stream	W subtropic	E subtropical	Caribbean	W tropical	E tropical
<26.7	-0.0002	-0.0063	0.0001	0.0001	0.0002	0.0023	0.0002	0.0098	0.0085	0.0092	-0.0025	-0.0007	-0.0009
26.7-27.25	0.0003	0.0001	0.0002	0.0001	-0.0006	-0.0052	0	-0.0016	-0.0035	-0.0178	0.0023	0.002	0.006
27.25-27.8	0.0001	0.0006	-0.0076	-0.0061	-0.0013	0.0074	-0.0002	-0.0004	0.0031	0.0104	0.0096	0.0034	-0.0036
27.8-28	0.0006	0.0007	0.0081	0.0116	-0.0009	-0.0089	0	0.0108	-0.0163	-0.0229	0.0307	0.0074	0.0079
28-28.08	0.0002	0.0008	0.0011	0.0012	-0.0002	-0.0052	0	-0.0007	-0.0067	0.0009	0.0003	0.0103	0.0016
28.08-28.16	-0.0005	0.001	0.001	0.0002	0	-0.0072	NaN	0.0012	-0.0036	0.0027	0.0007	0.0165	0.0072
>28.16	-0.0037	-0.0035	0.0002	0	0	NaN	NaN	NaN	-0.0001	NaN	0	0.0009	NaN

I.601-602

,by the time waters reach 26°N, the imprint of northern uptake has been redistributed and partly compensated across basins, so that local Canth anomalies are weaker or even reversed.’

I do not see a reversal of the Canth anomalies. On the contrary, in the regions close to the RAPID array (subtropics & tropics), the correlation of the Canth residuals is mainly positive.

The reviewer is correct that the word “reversed” in the original manuscript was misleading. At RAPID, subtropical and tropical inventory and concentration correlations are predominantly positive, as visible in Fig. 8f. The original phrasing attempted to contrast the difference between the OSNAP and RAPID signals but did not achieve this. The text has been revised to show that the negative deep-density subtropical inventory slopes observed at OSNAP are not apparent at RAPID, where positive associations instead dominate in subtropical and tropical regions; this is consistent with southward C_{Anth} transport via the deep return limb rather than the upstream ventilation signal captured at OSNAP. Additionally, as stated in the manuscript L. 630-632, the OSNAP and RAPID time-series display distinct temporal patterns and phase offsets. This inherent lack of (direct) connectivity between the two arrays, in association with different water mass propagation timescales, may also contribute to the observed differences, as suggested by the referee. The manuscript has been modified as follows: “*This difference may largely arise from time-lagged propagation of accumulated AMOC anomalies: by the time the overturning signal is measured at 26°N, it reflects the integrated downstream redistribution rather than the upstream formation*

signal: the negative deep-density subtropical inventory slopes seen at OSNAP are no longer apparent at RAPID, where positive associations dominate in subtropical and tropical regions (Fig. 8f), consistent with southward C_{Anth} transport via the deep return limb.” (L. 690-694)

I.610-611

,OSNAP, located in the region of deep-water formation, therefore shows opposite signs for NAO- and accumulated AMOC-driven anomalies’

This sounds as if the anomalies were located at the OSNAP array. The OSNAP transport might ‘show a sign for NAO driven anomalies’, but that is not investigated here. Make clear what you mean. The largest difference between the NAO and OSNAP correlations are in the tropical provinces, which are not directly effected by NAO.

We thank the referee for this good point. Here, “OSNAP” refers to the accumulated AMOC values derived from OSNAP measurements, not to anomalies located at the array itself. We also acknowledge the referee’s point about the largest contrast between NAO and accumulated overturning in tropical provinces, which respond to accumulated overturning but are not directly forced by the NAO. Both points have been addressed and clarified in the revised manuscript as follows: “*When compared against the NAO (Fig. 7), the heatmaps display largely spatially (colored) inverted patterns (Fig. 8a-f), with the largest contrast in subtropical and tropical provinces, which respond to the accumulated overturning but are not directly affected by the NAO. This difference arises because the NAO is an atmospheric forcing index, reflecting variability in winds, heat fluxes, and surface buoyancy, whereas the accumulated AMOC represents the time-lagged, integrated oceanic response to high-latitude forcing (Eden and Willebrand, 2001; Delworth and Zeng, 2016). Particularly, the C_{Anth} anomalies correlated with the accumulated AMOC index at OSNAP, capturing the upstream subpolar formation signal, show a similar pattern to the NAO in subpolar regions but diverge in tropical provinces. The RAPID-based index (Fig. 8d-f), sampling a downstream, more advectively integrated regime, shows a more distinct contrast with the NAO across all latitudes: subpolar signals are weakened or reversed as the upstream ventilation signature has been redistributed by the time it reaches 26°N, while subtropical and tropical provinces show predominantly positive correlations, reflecting signals that arrive years later and integrate contributions from upstream convection and large-scale meridional redistribution. The negative subtropical NAO- C_{Anth} correlations likely reflect a delayed, out-of-phase oceanic response rather than immediate atmospheric forcing.*” (L. 670-681)

I.615

...denser, lagged accumulated AMOC response’ What is a denser AMOC response?

“Denser” was inadvertently retained from an earlier draft where it referred to density layers, not to the AMOC response itself. The word has been removed in the revised manuscript (see our above response with the fully revised paragraph)

I.616

,... which smear out the immediate NAO signal’

I do not see why deep waters in the (sub-)tropical region should be immediately impacted by the NAO. The fact that the Canth residuals there are mainly negatively correlated with NAO might indicate a delayed response, which is out of phase with the NAO.

The referee is correct: the negative subtropical Canth-NAO correlations are more likely to reflect a delayed, out-of-phase response rather than an immediate atmospheric forcing. The sentence has been removed in the revised paragraph, which now frames the NAO-AMOC contrast as a temporal distinction between atmospheric forcing and lagged integrated oceanic responses.

L.619

,... use of accumulated AMOC anomalies' See remark to L.519.

This comment is addressed in our response to L.519's comment. The methodological rationale behind the use of accumulated AMOC anomalies, including the rolling 10-year baseline and its limitations, is also now included in the revised M&M Section 2.4. A cross-reference to this section has been added at L.619 of the revised manuscript.

L.640

,In the late 1990s and mid-2010s, periods of persistent positive NAO ...'

The mentioned time frame for the positive NAO is wrong. The NAO was positive in the early/mid 1990s and the mid/late 2010s.

We refer the reviewer to the time series of NAO presented in Figure 10, which is derived from the Hurrell station-based annual index (Lisbon-Reykjavik; available at https://climatedataguide.ucar.edu/sites/default/files/2023-07/nao_station_annual.txt). This shows positive values in the late 1990s: 1998 (+1.03), 1999 (+2.25), and 2000 (+1.18). While the period 1995-1997 was negative, the late 1990s, particularly 1998-2000, constitute a distinct positive phase in this index, with 1999 reaching values comparable to those of the early 1990s peak (i.e., 1989 (+2.38), 1990 (+3.88), 1991 (+1.14), 1992 (+2.54), 1993 (+1.71) and 1994 (+2.86)). This is consistent with what is shown by the grey-shaded NAO in Fig. 10. Therefore, we believe that the characterisation of "late 1990s and mid-2010s" as periods of persistent positive NAO accurately reflects the index used in this study.

However, we agree with the referee that the paragraph was too restrictive and that measurements from the early 1990s were not cited, while being in agreement with the purpose of this discussion section. In the revised manuscript, this time window has been included in addition to the late 1990s and mid-2010s.

L.640-641

,... water mass volumes in the densest layers ($\rho_n = 27.8-28 \text{ kg/m}^3$)'

This is only one layer and not the densest one! (it is the densest layer that is influenced by deep convection in the subpolar North Atlantic)

We agree on both remarks. In the revised manuscript, "layers" has been corrected to "layer" (singular), and "densest" is qualified to reflect that $27.8-28 \text{ kg m}^{-3}$ is the densest layer reached by deep convection (and selected in this Section), not the overall densest layer in our classification.

L.646

,... in both basins, saturation decreases during periods of sustained positive NAO'

I do not see that from Fig. 10. On the contrary, during the long positive NAO period in the 2010s,

Canth saturation is relatively high in the Labrador Sea (Fig. 10b) and increasing in the Irminger Sea (Fig. 10e),

We thank the reviewer for this careful reading and their question about this statement. Upon re-examination, we acknowledge that the direction of the relationship was incorrectly stated in the original manuscript. The figures Fig. 10b & e in fact show that saturation increases during positive NAO phases and decreases during negative NAO phases. This is most clearly visible during the 1999-2001 negative NAO period, where both basins show a pronounced saturation minimum. This is physically consistent: under strong positive NAO, intensified deep convection ventilates interior waters and brings them closer to equilibrium with the rising atmospheric CO₂, thereby increasing saturation. Conversely, during negative NAO phases, reduced convection allows a growing lag to develop between interior C_{Anth} and atmospheric forcing. To further support this, we derived the regressions between the annual NAO index and annual mean C_{Anth} saturation for each basin and density layer (Figure 6 below, not in the revised manuscript). Three of the four basin/layer combinations show a positive NAO-saturation relationship, with statistical significance in the Irminger SPMW layer ($R^2 = 0.22$, $p = 0.043$). The Labrador SPMW relationship is negative but non-significant ($R^2 = 0.02$, $p = 0.53$), likely reflecting the influence of an isolated event in 2010. The manuscript has been corrected accordingly.

“The C_{Anth} saturation ratios (Fig. 10b & e), following the formulation of Fröb et al. (2016), further illustrate these dynamics: in both basins, saturation declines markedly during negative NAO phases, most clearly around 1999-2001, reflecting reduced convection and growing lag between interior C_{Anth} and rising atmospheric CO₂. Conversely, sustained positive NAO periods support higher C_{Anth} saturation by renewing deep water masses and driving interior storage progressively closer to the anthropogenic signal.” (L. 740-743)

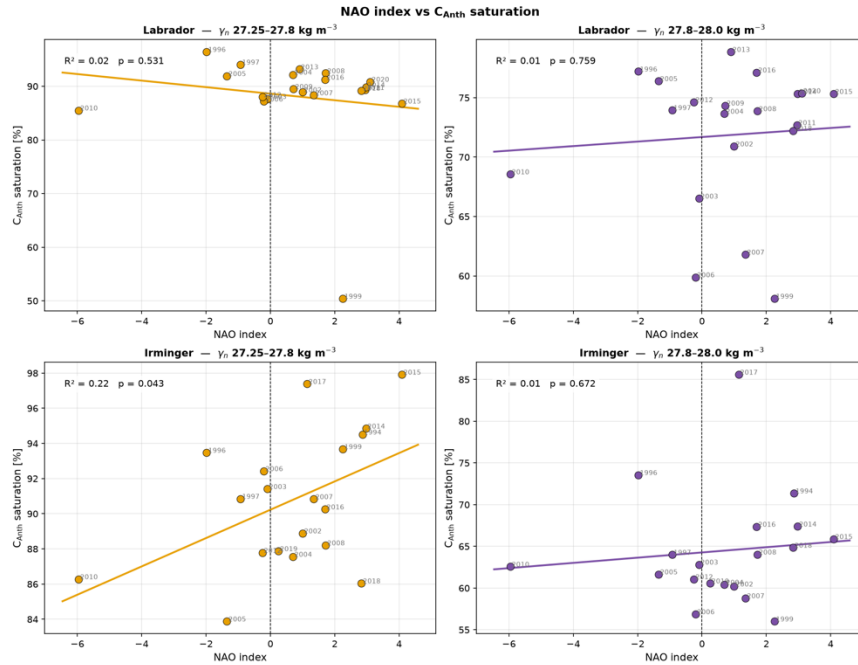


Figure 6 (not included in the revised manuscript). Annual mean C_{Anth} saturation (%) as a function of the annual NAO index for the Labrador (upper panels) and Irminger (lower panels) basins and for the 27.25-27.8 kg m⁻³ (yellow line) and 27.8-28 kg m⁻³ (purple line) density layers over 1993-2021.

I.649

,... and deeper, less frequent convection compared to the Labrador Sea‘

The convection in the Irminger Sea is not ,deeper‘ than in the Labrador Sea. You probably mean ,less frequent deep convection.

We thank the referee for highlighting this and have corrected the manuscript accordingly. Indeed, the intention was to express the “less frequent deep convection” in the Irminger basin in comparison with the Labrador province.

I.666

,...particularly in the densest layers‘

Again, you probably mean the LSW layer, which is not the densest one.

Indeed, this statement was intended to refer to the densest layer of the two considered layers here, i.e. $\gamma_n = 27.8\text{--}28 \text{ kg m}^{-3}$. This has been clarified in the revised manuscript.

I.682-683

,The mirror-image patterns between NAO at accumulated AMOC heatmaps OSNAP...‘ This sentence does not make any sense. I would also not use the word ,heatmaps‘ here, but correlation (maps) or something similar, which is more meaningful.

We agree that the “mirror-image” was imprecise and that the sentence was unclear. The overall color patterns across the 3 regression figures do show meaningful contrasts, but these are better described as contrasting spatial signatures rather than a strict mirror image. The sentence has been revised in the revised manuscript as follows: “*The contrasting spatial signatures across the NAO and accumulated AMOC correlation maps further underscore the importance of distinguishing between atmospheric forcing and oceanic adjustment, which operate on different timescales.*” (L. 785-787)

Figures

In all figures, the font size of the axis‘ labels needs to be larger (except Fig. A3).

All plots have been redrawn, and font sizes increased.

Fig. 4: The left y-axis in Fig. 4b shows wrong numbers. The Canth column inventory in the Atlantic does not exceed 200 moles/m², some of the values in Fig. 4b are twice as high.

We apologise for this big mistake, which has been corrected in the revised manuscript, and the caption has been expanded.

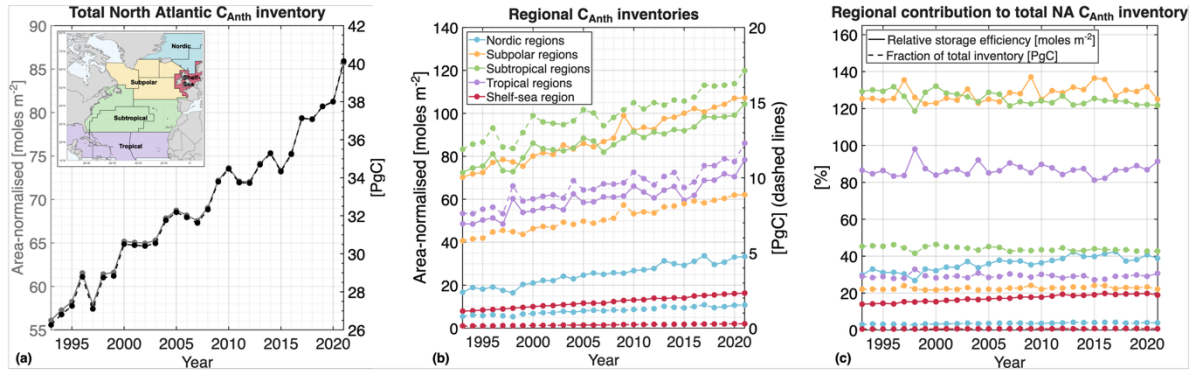


Figure 4 (in the revised manuscript). Time series of (a & b) C_{Anth} inventories in the global North Atlantic Ocean (a) and the main regions (b) in moles m^{-2} and PgC (dashed lines, right y-axis) between 1993 and 2021. Each data point is color-coded based on its corresponding region. Panel (c) indicates the contribution (in %) of each main region to the total C_{Anth} inventories. Solid lines show the area-normalised inventory (in moles m^{-2}) of each region relative to the basin mean (values $> 100\%$ indicate above-average storage efficiency per unit area), while dashed lines show each region's contribution to the total NA C_{Anth} inventory in PgC.

Fig. 6, A2 and A4: Please increase the height of the subfigures, there is some space left on the pages.

All the plots have been redrawn in the revised manuscript, and the height of the subfigures has been increased.

Fig. 6, 7, 8 and 9: The regression coefficients should have units.

Units have been added to the other Figures, 7, 8, and 9. Fig. 6 already has units (m^3 and $\mu mol\ kg^{-1}$)

Fig. 7 and 8, legend: Why do you use a p-value of 0.5 here?

We thank the reviewer for raising this important methodological point. The choice of $p = 0.5$ as the threshold for the grey-line indicator in Figs. 7 and 8 was motivated by the very low statistical results associated with this analysis. However, we acknowledge that this choice is non-standard and deserves explicit justification. Indeed, the key difference between Figure 5, where the “classic” p-value 0.05 threshold has been used to test linear trends against time, and Figures 7 and 8 is the predictor variable. The accumulated AMOC anomaly index is constructed as a running integral from a 10-year time-moving baseline, which by construction introduces very high temporal persistence with consecutive yearly values that are not independent observations. Similarly, the annual NAO index and the C_{Anth} anomaly time series both exhibit significant year-to-year autocorrelation. Hence, applying the “usual” threshold $p=0.05$ to Figs. 7 and 8 would result in almost the entire figure being hatched as non-significant, inducing no useful spatial information even for relationships that are physically interpretable and spatially coherent. In consequence, we chose to use $p=0.5$ not as a formal claim of statistical significance, but as a visual threshold indicating cells where the predictor accounts for a certain fraction of variance. Moreover, our interpretations of Figs. 7 and 8 rely primarily on the spatial coherence of regression patterns across regions and density layers, not on individual cells passing a significance test. However, to avoid any misinterpretation, we have added the following sentence to the Figures 7 and 8 captions (pages 22 and 26/27): “The $p = 0.5$ threshold shown here is used as a visual guide to indicate cells with non-trivial explanatory power.” For information, cells indicated by a grey diagonal line ($p \leq 0.5$) in Fig. 8 explain between 4% and 77% of the variance in C_{Anth} anomalies

at OSNAP ($R^2 = 0.04-0.77$), and between 4% and 99% at RAPID ($R^2 = 0.04-0.99$). In Fig. 7, cells indicated by a grey diagonal line ($p \leq 0.5$) explain between 2% and 45% of the variance in C_{Anth} anomalies ($R^2 = 0.02-0.45$).

Fig. 8: Panels d-f are erroneously labeled a-c. As panels d-f are on a new page, I would suggest to give them a new figure number (9).

We thank the referee for identifying this labelling error. Panels d-f correspond to the AMOC-RAPID heatmaps, which were incorrectly labelled as panels a-c in the original manuscript. To avoid confusion with the AMOC-OSNAP heatmaps, the panel labels have been clarified in the revised manuscript: panels a-c now refer to the AMOC-OSNAP heatmaps, and panels d-f refer to the AMOC-RAPID heatmaps. However, as both panels, a-c and d-f are related to accumulated AMOC sensitivities, we prefer keeping the same figure number while differentiating the two arrays with the numbering.

Fig.9: The colorbar inside the panels is not readable at all (far too small).

The colorbars have been enlarged to improve readability, and, for greater clarity, the figure has been reformatted in landscape orientation. Units have been added too.

Legend:

You say that the correlation for the four main regions is shown. In fact, you show the correlation for every single province.

The reference to “four panels” was indeed an error and has been corrected to “six panels.” We also agree that the schematic displays more than four sub-regions. However, the caption refers to the four major regions defined in the manuscript (Section 2.4), namely the Nordic, Subpolar, Subtropical, and Tropical regions, each of which comprises several sub-regions. We have revised the caption to make this distinction clearer and avoid any ambiguity.

,Dashed-dotted light-pink solid arrows represent the upper AMOC limb (NAC).‘

I do not see them. Instead, I see grey dotted lines/arrows, probably meant to represent the upper circulation including the subtropical gyre. There is also a light pink large arrow in the upper panels, which I cannot assign to anything.

We thank the reviewer for raising this missing explanation. Upon re-examination of the figure, we agree that the arrows were intended to be light pink but, as raised by the referee, appeared rather grey in the Figure and hence did not fully match the description in the caption. Specifically, the arrows representing the upper AMOC limb (North Atlantic Current, NAC) appear white to light grey in the figure rather than light pink as stated in the caption. By increasing the size of the arrows in the revised manuscript, we believe that the color is now more clearly visible. Furthermore, the large 3D arrows present at the top of each panel, which the reviewer could not assign to anything, represent the dominant forcing signal (NAO index or accumulated AMOC anomaly) driving each scenario (positive/negative NAO phase, stronger/weaker AMOC) and were not described in the original caption. The caption has been revised to clearly describe all arrow types: “*Large 3D arrows indicate the dominant forcing signal (positive/negative NAO or stronger/weaker AMOC) in each panel. Small 3D block arrows in the*

Labrador and Irminger regions indicate deep convection events (strong in positive-NAO and stronger-AMOC panels; weak/absent otherwise)."

,...the apparent negative C_{Anth} correlation in subpolar regions under stronger accumulated AMOC (top-middle) reflects the downstream RAPID signature of enhanced export of recently ventilated, relatively low- C_{Anth} deep waters via the DWBC.'

The correlation with the RAPID array is shown in the top right, not middle.

The caption has been corrected accordingly.

Again, I do not see why the exported recently ventilated deep water should be low in C_{Anth} . If that would be true, the correlation of the C_{Anth} residuals should be negative along the export pathways, which they are not, they are positive there. The (small) negative correlation is probably caused by the time delay between C_{Anth} anomalies in the subpolar region and the AMOC at RAPID.

We thank the reviewer for this important correction. We agree that the "recently ventilated, relatively low- C_{Anth} deep waters" wording used in the caption was incorrect, and we acknowledge the logical inconsistency the reviewer identifies: if DWBC-exported waters were anomalously low in C_{Anth} , correlations along the export pathways would be negative... yet they are positive. This concern was already raised for the manuscript body text at L. 573-575 (lines in the original manuscript), and in our response to that comment, we already corrected the text to state that the negative RAPID-subpolar signal *"likely reflects a distinct, possibly lagged relationship between subtropical overturning transport and subpolar carbon content"* (L.652-653 in the revised manuscript). The Fig. 9 (page 30) caption has been revised too to align with the revised manuscript text: the phrase has been replaced by *"the negative subpolar signal in the accumulated AMOC-RAPID panel likely reflects a temporal phasing effect, as the downstream RAPID index integrates the overturning signal with a lag relative to subpolar C_{Anth} accumulation, rather than a physically distinct export of low- C_{Anth} waters."* However, we note that the same layers show positive correlations with accumulated AMOC at OSNAP and NAO, confirming that local ventilation enriches rather than depletes subpolar C_{Anth} .

,not a true reduction (which would be positive as seen at OSNAP, top-right)' OSNAP is shown in the middle.

The caption has been corrected accordingly.

I do not understand how a ,true reduction' can be ,positive'

We acknowledge that this parenthetical was grammatically ambiguous. The intended meaning was: "the negative signal in the subpolar region under stronger accumulated AMOC-RAPID does not reflect a true reduction in subpolar C_{Anth} storage, and we know this because the OSNAP panel (top-middle) shows a positive (red) subpolar signal, confirming that C_{Anth} storage in the subpolar gyre is in fact increasing under stronger overturning conditions." In other words, "positive" referred here to the actual (OSNAP-based) subpolar C_{Anth} storage, not to the reduction itself. To avoid any ambiguity, the caption has been rewritten (page 30) as: *"not a true reduction in subpolar C_{Anth} storage, as confirmed by the*

positive subpolar signal in the OSNAP panel (top-right), which more directly captures convection-driven C_{Anth} uptake in the subpolar gyre.”

Fig.10

The x-labels in panels (c) and (f) differ from the other panels. Please unify.

Panels (a) and (d) do not show exactly the same time period as the other ones. Please unify.

Legend: ,... the maximum (purple) and average (pink) mixed layer depth’ Please add that the pink line is dotted.

In the revised manuscript, Figure 10 has been redrawn with the following modifications: (1) same x-label sizes for all panels and (2) a unique time period from 1993 to 2021 for all panels. The legend has been modified too and now includes the fact that the pink line is dotted.

Minor comments

You often quote density values without $\gamma_n =$ in front. Please add.

The entire manuscript has been verified and γ_n added when missing.

Examples for questionable/wrong results:

Fig. 6b and Fig A2

Both figures show Canth values. In 6b, I assume, only the years with Canth data are shown. In Fig. A2 these are marked with circle dots according to the text in the legend. However, the data based Canth points in both figures do not coincide in time. How can that be?!

In Fig. 6b, the deeper layers for the W. Subtropical region are missing completely.

Fig. A2

The years with with Canth data sometimes stick out, especially for the LSW layer in the Caribbean.

This might be due to spatial aliasing (data from inside the ,real‘ Caribbean and from the DWBC are mixed). This can probable be reduced by taking the DWBC east and north of the Caribbean region to the W tropical region.

In general, the deviations of the years with Canth data from the longterm trend show the uncertainty and should be discussed.

We acknowledge the referee for spotting the mismatch between Fig. 6b and Fig. A2. After a careful check of the code, we found a mistake in the region labelling for Fig. 6b and would like to apologise for that. We reviewed the entire code and checked for any similar issues throughout both the code and the manuscript, but, thankfully, Fig. 6b was the only one with the wrong labels. This issue explains why the deeper layer for the W Subtropical region was missing. In the revised manuscript, Fig. 6b and A2 display similar inferred C_{Anth} points.

Regarding the occasional spikes in the derived C_{Anth} values for the Caribbean LSW layer: as discussed in our response to the comment about Subsection 2.4 (see our response above, pages 6-7 of this document), we have redefined the Caribbean province boundaries in the revised manuscript, excluding data points located east of -75°. By doing so, several values were removed from the Caribbean region, now being included in the Western tropical region. As a consequence, data sparsity (in both temporal and spatial coverage) has increased, and noticeable year-to-year variability may contribute to the (last) observed C_{Anth} inventory spike displayed in the revised Fig. A2 in 2010. Because of missing data in the

Caribbean region, strong conclusions about the rationale behind the observed spike cannot be drawn here.

Finally, in the revised manuscript, a sentence has been added to acknowledge that residual spatial heterogeneity exists and has an impact on the derived annual C_{Anth} mean: “*Additionally, it can be noted that in years with sparse sampling concentrated in a sub-region, annual means may not be fully representative of the full province extent, as illustrated by the 2009 anomaly in the Northeast deepest layers where a single cruise sampling the western part of the province drives the high annual mean despite physically consistent hydrographic properties.*” (L. 469-472)

Fig.9

I am concerned about the spatial pattern of the regression coefficients. The NAO has its largest impact on C_{Anth} in the subtropical and tropical regions, although one would expect the largest impact in the subpolar regions. The AMOC has its largest impact on C_{Anth} in the eastern tropical Atlantic, a region which is not in the main AMOC pathway and hence should be effected less. In the light of this, how reliable are your findings? At least you need to discuss the unexpected pattern of Fig.9.

We thank the reviewer for this important observation. We acknowledge that the spatial patterns in Fig. 9 might appear counterintuitive at first glance, and we agree that this deserves explicit discussion.

The final schematic Figure 9 was designed as a budget-oriented synthesis figure: by displaying regression slopes against C_{Anth} inventory residuals (in PgC) rather than concentration residuals ($\mu\text{mol kg}^{-1}$), it quantifies how much total carbon storage in each province co-varies with NAO or AMOC forcing budget. However, this choice has an important interpretive consequence: subtropical and tropical provinces are substantially larger in spatial extent and ocean volume than the subpolar provinces, and consequently even a modest NAO- or AMOC-driven change in C_{Anth} concentration translates into a larger absolute inventory anomaly in PgC. The apparent dominance of subtropical and tropical regression coefficients in Fig. 9 therefore partly reflects province size and volume rather than a stronger physical sensitivity of these regions to NAO or AMOC forcing.

This is corroborated by the concentration-based regressions (Figs. 7 & 8), where subpolar regions (particularly the Labrador and Irminger Seas) show among the strongest responses per unit volume, consistent with the direct influence of NAO-driven deep convection on C_{Anth} uptake and AMOC-driven ventilation.

However, beyond the volume effect, we believe that the spatial patterns in Fig. 9 are physically interpretable. For the NAO and accumulated AMOC-OSNAP panels, the link is more or less direct: positive NAO (upper left panel) drives enhanced deep convection in the subpolar gyre, increasing local C_{Anth} storage (positive subpolar signal), which is then exported southward via the DWBC under a stronger AMOC. Crucially, stronger AMOC at OSNAP also implies enhanced throughflow between the subtropical and subpolar gyres, reducing subtropical recirculation, hence explaining the predominantly negative (blue) subtropical signal under stronger AMOC-OSNAP, and its reversal under weaker AMOC when subtropical waters accumulate with reduced northward export.

The accumulated AMOC-RAPID column reflects a different and complementary signal: at 26°N, a stronger AMOC pumps more C_{Anth} -rich waters northward from lower latitudes, producing a broader and more spatially diffuse positive signal across subtropical and tropical provinces that diminishes when the AMOC weakens.

Regarding the AMOC signal in the eastern tropical Atlantic specifically: we agree that this province is not on the direct DWBC pathway. Its positive correlation with accumulated AMOC likely reflects indirect interior spreading of NADW-derived C_{Anth} -enriched waters over multi-year timescales, further amplified into large PgC anomalies by the province's large volume. We acknowledge that this signal should be interpreted with caution, as it may also incorporate shared long-term trends rather than a purely mechanistic AMOC- C_{Anth} connection; a limitation consistent with the modest R^2 values reported across tropical provinces. Finally, we would also like to point out that a mistake was made in the original manuscript for the tropical regions, which had been inverted (their volume values) in Figure 9: the eastern Tropical volume had been assigned to the western region and vice versa. In the revised Figure 9, the western region now shows the strongest relationships, consistent with its position regarding the AMOC pathway.

Finally, we would also like to point out that the contrasting relationship between temperature and anthropogenic carbon is also reflected in this schematic figure: more C_{Anth} dissolves in warmer waters with a lower buffering capacity. In the revised manuscript, the following sentence has been added to the manuscript to clarify the role of province volume in the interpretation of Fig. 9: *“Additionally, it should be noted that Figure 9 displays regression slopes against C_{Anth} inventory residuals (PgC), which integrate concentration changes over the full province volume. The larger apparent sensitivity of subtropical and tropical provinces therefore partly reflects their greater ocean volume rather than a stronger physical response to NAO or AMOC forcing, as well as lower buffering capacity of warmer subtropical and tropical waters, which enhances anthropogenic carbon uptake. Indeed, concentration-based regressions (Figs. 7 & 8) confirm that the strongest per-unit-volume signals remain in the subpolar provinces.”* (706-711)

Fig. 10

Canth

The low values for the Labrador Sea in 1999, 2006 and 2007 might be artefacts (spatially biased or data quality issues). Especially the low upper layer values in 1999 are not realistic. Such drastic changes are also not present in the time series of oxygen and Canth for the central Labrador Sea in Rhein et al.(2017). Also the high lower layer values in the Irminger Sea in 2017 appear too high (although I would expect a maximum).

MLD

The maximum mixed layer depths are too high for my knowledge. I do not believe the values close to 3000 m in some years. In most of the years, the maximum MLD is close to 2000 m both in the Irminger and Labrador Sea. This would indicate very deep reaching convection every year. In addition, the values for the Irminger and Labrador Sea are comparable. In the text you have written that convection in the Irminger Sea would be less frequent, which is a contradiction to Fig. 10. I also wonder how the average MLD can be larger than the maximum MLD, which is the case for the first year in the Irminger Sea.

We thank the reviewer for this careful examination of the C_{Anth} time series presented in Fig.10. We address each concern in turn.

About the C_{Anth} values:

Labrador Sea - 1999: We acknowledge that, in 1999, the two considered density layers contain just 2 profiles in the 27.25-27.8 kg m^{-3} layer (yellow line, SPMW) and 8 profiles in the 27.8-28 kg m^{-3} layer

(purple line, LSW), all from a single station at 51°N, 43°W, well outside the main deep convection zone (~55-60°N; Figure 7 below). We agree that this limited spatial coverage introduces genuine sampling uncertainty for that year. In the revised manuscript, Figure 10's caption has been amended to highlight this data sparsity in 1999 specifically: *“Note: the apparent inversion between density layers in the Labrador Sea around 1999 is based on two observations from a single station at the southern basin margin (51°N, 43°W); adjacent years are reconstructed (no direct observations) and should be interpreted with caution.”* (Page 32)

That said, deriving lower C_{Anth} values could be physically consistent with the well-documented transition from the intense positive NAO phase with deep convection episodes in the early 1990s to a weakened convective regime after mid-1990 (e.g., Lazier et al., 2002), which is directly associated with reduced C_{Anth} uptake in Labrador Sea Water. Indeed, Rhein et al. (2017) stated that “the years 1996-1999 represent the period after the cessation of convection intense and deep enough to form and renew deep LSW, while the lighter upper LSW was still produced.”, Hence, Rhein et al. (2017) as well as Pérez et al. (2010) and Steinfeldt et al. (2009) reported a reduction in the uptake of C_{Anth} by LSW between the NAO high period related with intense convection in the 1990s and the period of weaker convection after 1997. In agreement with this observation, Steinfeldt et al. (2009) observed that, “over the whole water column, the C_{Anth} inventory in the western subpolar North Atlantic increased only by 2% between 1997 and 2003 (or decreased by 9% if the 1997 data are time-corrected), much less than the 11% that would be expected from the rising atmospheric CO_2 levels.” Finally, it can be noted that Raimondi et al. (2021), already cited in our manuscript, reported an average C_{Anth} concentration of ca. $45 \mu\text{mol kg}^{-1}$ for the LSW layer in 1999 (see Fig. 9a in Raimondi et al., 2021).

Labrador Sea - 2006–2007: We do not consider these years to be artefacts. Both years are sampled by 14 and 17 observations from multiple stations located in the interior of the Labrador basin at 59-60°N (Figure 7, not in the revised manuscript), within the deep convection zone. In addition, the mean C_{Anth} values in the SPMW (43.8 ± 4.4 and $44.8 \pm 3.7 \mu\text{mol kg}^{-1}$, respectively) and LSW (30.1 ± 5.3 and $31.4 \pm 5.1 \mu\text{mol kg}^{-1}$, respectively) layers fall within the interannual range of this study and within the GLODAPv2 climatology C_{Anth} range (Figure 8, not in the revised manuscript).

Irminger Sea - 2017: As raised by the reviewer themselves, an elevated C_{Anth} concentration maximum would be expected at this time in the denser layer of the Irminger Sea. Frøb et al. (2016) documented that the deep convection events in the Irminger Sea during winter 2014–2015 nearly tripled the local C_{Anth} storage rate compared to previous surveys. Zunino et al. (2020) further document four consecutive winters of sustained deep convection (2015-2018) south-east of Cape Farewell, with maximum mixed layer depths exceeding 1300 m. The cumulative effect of these successive convective events naturally might have produced elevated C_{Anth} concentrations in the LSW density layer by 2017 (Figure 8 below), consistent with the elevated C_{Anth} saturation visible in Fig. 10e. We therefore consider this feature to be a plausible signal. Moreover, according to the position of the data sampling, the C_{Anth} values derived in this study are in agreement with the C_{Anth} data calculated with the TTD method and normalised to the year 2018 presented in Figure 2 b & d in Asselot et al. (2024).

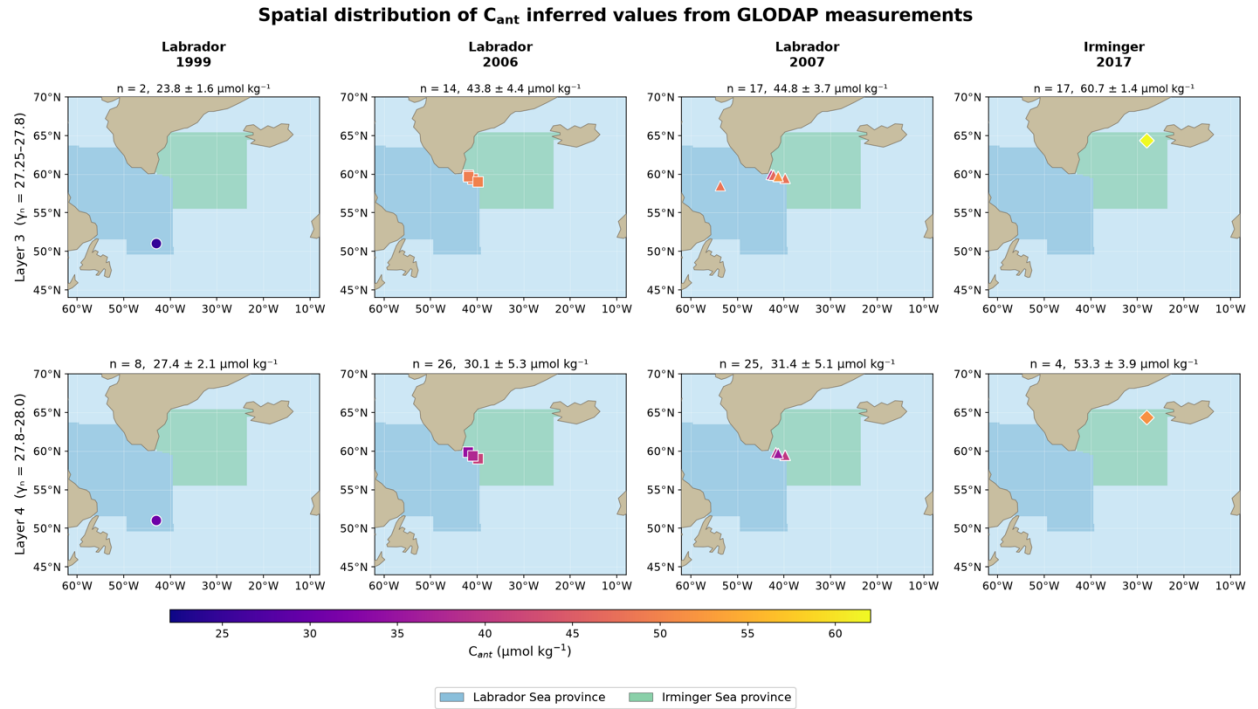


Figure 7 (not included in the revised manuscript). Geographical position of the GLODAP data used to derive the C_{Anth} concentration values in the Labrador and Irminger provinces, for the years 1999, 2006, 2007 and 2017 and the layers 27.25-27.8 kg m^{-3} (upper panel) and 27.8-28 kg m^{-3} (lower panel) density layers. Datapoints have been coloured according to their mean C_{Anth} concentration value (in $\mu\text{mol kg}^{-1}$). The number of measurements available in each layer as well as the mean C_{Anth} concentration value have been added.

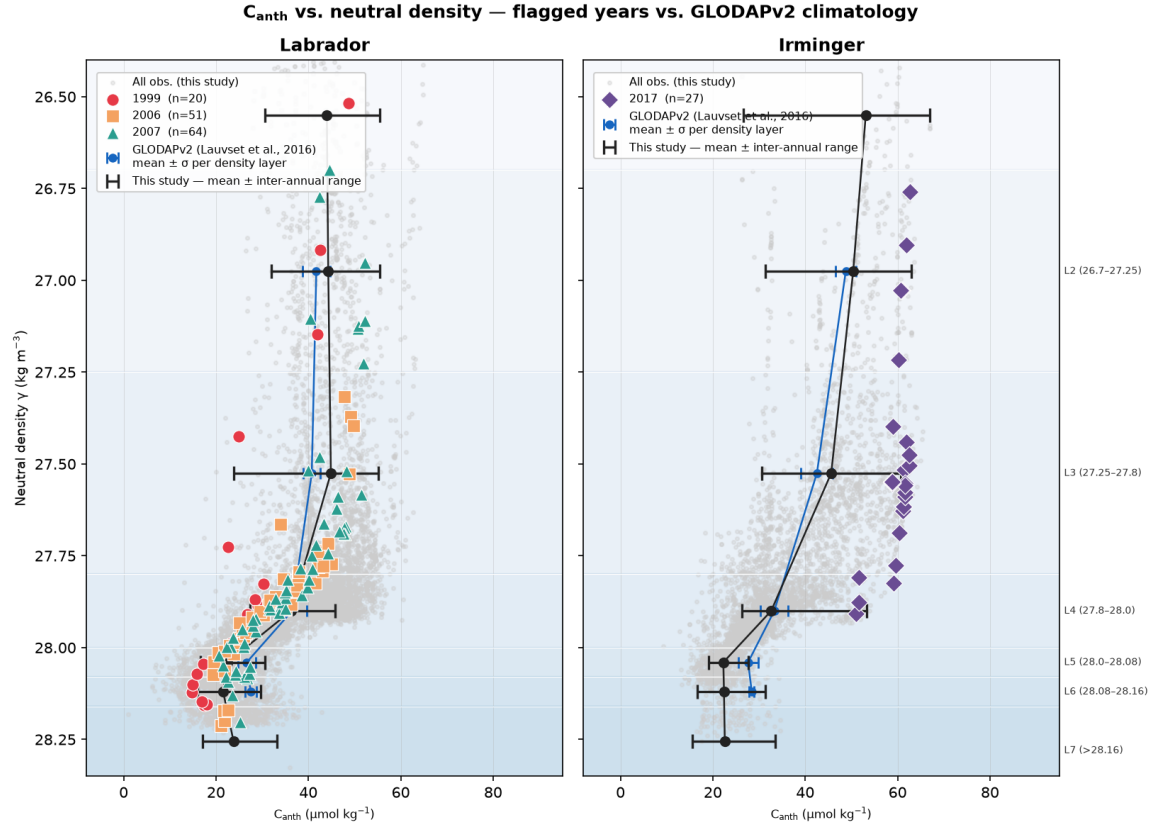


Figure 8 (not included in the revised manuscript). C_{Anth} concentration ($\mu\text{mol kg}^{-1}$) as a function of neutral density (γ , kg m^{-3}) for the Labrador Sea (left panel) and Irminger Sea (right panel). Grey dots: all GLODAP observations used in this study. Coloured symbols indicate the flagged years: 1999 (red circles), 2006 (orange squares), and 2007 (teal triangles) for the Labrador Sea; 2017 (purple diamonds) for the Irminger Sea. The blue line with error bars shows the GLODAPv2 climatology (Lauvset et al., 2016) mean $\pm 1\sigma$ per density layer. The black line with error bars shows the mean $\pm 1\sigma$ across all individual observations of this study per density layer. Horizontal grey bands delineate the density layer boundaries.

Moreover, we would like to point out that all data used in the study passed the second quality control procedure (described in Section 2.1 of the manuscript) implemented in this study, in addition to the Glodap flagging procedure. Furthermore, following Reviewer #2's suggestion, a global comparison with the GLODAPv2 C_{Anth} database has been performed too during the review process (see our response to Referee #2 for more details). Overall, our estimates show a strong correlation with GLODAPv2 across all 13 provinces ($r^2 = 0.65\text{-}0.90$ in most regions), indicating overall consistency between the two back-calculation methods and the resulting C_{Anth} estimates. Additionally, a modest positive offset in our estimates relative to GLODAPv2 (typically lower than $5 \mu\text{mol kg}^{-1}$) was expected and is observed. This shift can be explained by the different temporal coverage of the two datasets: our dataset, which extends to 2021, naturally captures higher C_{Anth} concentrations than the GLODAPv2 climatology, based on measurements collected from 1972-2013 and centered in the year 2002.

Taken together, we believe that these comparisons provide strong evidence that our C_{Anth} estimates are consistent with the GLODAPv2 TTD climatology and that the two methods agree to within the expected temporal drift of the oceanic C_{Anth} signal.

Asselot, R., Thierry, V., Carracedo, L.I., Mercier, H., Velo, A., Bajon, R. & Pérez, F.F. (2024). Temporal evolution of anthropogenic carbon in the subpolar North Atlantic gyre between 2011 - 2021. ESS Open Archive. 15 October 2024. <https://doi.org/10.22541/essoar:172901267.76656819/v1>

Courtois, P., Hu, X., Pennelly, C., Spence, P., & Myers, P. G. (2017). Mixed layer depth calculation in deep convection regions in ocean numerical models. *Ocean Modelling*, 120, 60–78. <https://doi.org/10.1016/j.ocemod.2017.10.007>

Fröb, F., Olsen, A., Våge, K., Moore, G. W. K., Yashayaev, I., Jeansson, E., & Rajasakaren, B. (2016). Irminger Sea deep convection injects oxygen and anthropogenic carbon to the ocean interior. *Nature Communications*, 7, Article 13244. <https://doi.org/10.1038/ncomms13244>

Pérez, F.F., Vázquez-Rodríguez, M., Mercier, H., Velo, A., Lherminier, P. & Ríos, A.F. (2010). Trends of anthropogenic CO₂ storage in North Atlantic water masses. *Biogeosciences*, 7, 1789–1807. (doi:10.5194/bg-7-1789-2010)

Steinfeldt, R., Rhein, M., Bullister, J.L. & Tanhua, T. (2009). Inventory changes in anthropogenic carbon from 1997–2003 in the Atlantic Ocean between 20°S and 65°N. *Glob. Biogeochem. Cycles*, 23, GB3010. (doi:10.1029/2008GB003311)

Raimondi, L., Tanhua, T., Azetsu-Scott, K., Yashayaev, I., & Wallace, D. W. R. (2021). A 30-year time series of transient tracer-based estimates of anthropogenic carbon in the Central Labrador Sea. *Journal of Geophysical Research: Oceans*, 126, e2020JC017092. <https://doi.org/10.1029/2020JC017092>

Lazier, J., Hendry, R., Clarke, A., Yashayaev, I., & Rhines, P. (2002). Convection and restratification in the Labrador Sea, 1990–2000. *Deep-Sea Research Part I: Oceanographic Research Papers*, 49(10), 1819–1835. [https://doi.org/10.1016/S0967-0637\(02\)00064-X](https://doi.org/10.1016/S0967-0637(02)00064-X)

Yashayaev, I. (2007). Hydrographic changes in the Labrador Sea, 1960–2005. *Progress in Oceanography*, 73(3–4), 242–276. <https://doi.org/10.1016/j.pocean.2007.04.001>

Zunino, P., Mercier, H., and Thierry, V.: Why did deep convection persist over four consecutive winters (2015–2018) southeast of Cape Farewell?, *Ocean Sci.*, 16, 99–113, <https://doi.org/10.5194/os-16-99-2020>, 2020.

About the MLD:

We thank the reviewer for flagging the maximum MLD values higher than 3000m. Investigation of the ARMOR3D density profiles revealed that the >3000 m maximum MLD in the Labrador Sea in 1996 is an artefact confined to two grid points (57.4°N, 43.6°W) where ARMOR3D assigns an anomalously high near-surface salinity (~35.2 at 10 m, versus ~34.0–34.5 typical for this region in November). This inflated the reference density used by the threshold algorithm (de Boyer Montégut et al., 2004), causing it to search to ~3000 m depth before exceeding the criterion — corresponding to North Atlantic Deep Water rather than a true mixed layer. We have applied a QC filter removing MLD values at grid points where surface salinity exceeds 35.0 north of 45°N, reducing the Labrador Sea maximum for this month from 3250 m to 1400 m. The revised Fig. 10c no longer shows this spurious maximum. This correction does not affect any scientific conclusions.

Outside this corrected artefact, the remaining maximum MLD values (up to 2000–2300 m) are consistent with the literature: Lazier et al. (2002) document convection reaching 2300 m in the Labrador Sea during winters 1993–1994, and Yashayaev (2007) identifies this as the deepest convection

in the modern observational record. Comparison of our MLD findings against the literature revealed that our derived values are in agreement with Courtois et al. (2017, Figure 7 in their paper) for the period 2002-2011 (mean MLD values from 1000 m up to 2000 m in 2008) and Asselot et al. (2024) for the period 2011-2022 (maximum MLD values ranging from 1000 to 2000). For the Irminger Sea, the MLD maximum during winter 2014-2015 is consistent with Frøb et al. (2016, their Figure 2). We would also like to state that, following Reviewer #2's suggestion, we have updated the density threshold criterion, which brings our MLD values into better agreement with these references.

The similarity in MLD magnitude between the two basins does not contradict our statement that convection in the Irminger Sea is less frequent. Indeed, Figure 10c & f show that deep convective events in the Irminger Sea occur in fewer winters than in the Labrador Sea over the 1993–2022 period. Hence, the MLD signal in the Irminger reaches comparable depths when convection occurs, but events are more episodic. We have clarified this distinction in the revised manuscript as follows: *“Interestingly, while the Labrador Sea shows stronger interannual co-variability between NAO and C_{Anth} (Fig. 10a), the Irminger Sea exhibits a more gradual and smoothed response (Fig. 10d), in line with its weaker stratification and less frequent deep convection compared to the Labrador Sea, although when they occur, they can reach comparable depths (e.g., Våge et al., 2009; Våge et al., 2011; de Jong and de Steur, 2016, and Fig. 10c & f).”* (L. 743-747)

Finally, average MLD and derived MLD values have two separate axes. The right y-axis refers to the mean MLD values while the left one is dedicated to the maximum MLD value. Hence, unless we misunderstood the referee's comment, average MLD is not larger than the maximum values.

Literature:

Rhein, M., Steinfeldt, R., Kieke, D., Stendardo, I. & Yashayaev, I. Ventilation variability of Labrador Sea Water and its impact on oxygen and anthropogenic carbon, *Phil. Trans. R. Soc. A*, 375(2102) (2017), doi:10.1098/rsta.2016.0321

Yashayaev, I. Intensification and shutdown of deep convection in the Labrador Sea were caused by changes in atmospheric and freshwater dynamics. *Commun Earth Environ* 5, 156 (2024). <https://doi.org/10.1038/s43247-024-01296-9>