Reviewer 1 - Louise Delaigue

The manuscript by Bajon et al. presents a robust assessment of the seasonal to long-term variability of natural and anthropogenic carbon concentrations and transports across the A25-OVIDE section in the subpolar North Atlantic. Combining ship-based observations, ocean reanalyses, neural networks, and a back-calculation approach, the authors build a 30-year monthly time series of [Cnat] and [Cant]. They show that [Cnat] remains stable, while [Cant] increases by over one third, reflecting rising atmospheric CO2 and circulation variability. The study highlights strong seasonal and interannual variability in the mixed layer and upper AMOC, with transport variability mainly driven by volume changes and long-term Cant trends by concentration changes. These findings provide valuable insight into North Atlantic carbon dynamics and offer a strong reference for model evaluation and regional carbon budget studies.

This is an excellent paper: very well written and structured, with a clear scientific contribution and a very complete methodology.

Thank you a lot, Louise, for your constructive comments. All the authors appreciate your comments that have contributed to the overall improvement of the manuscript. We particularly thank you for the quick review.

We answered your specific comments in the following in blue.

Specific comments

L20 surpassing 420 ppm, in 2024, 2025? Depending on the acceptance date of the paper it might be best to be precise

Revised as “420 ppm in 2023”.

L20 I think natural reservoir can lead to confusion, maybe it’s best to say say carbon sink

Changed to ”carbon sink”.

L22 + L24 use the latest GCB citation

Updated to latest GCB citation.

L25 defining DICtotal as Cnat + Cant is a bit reductive - or you need to say Cnat then includes everything else (i.e. preformed, the BCP and the carbonate pump)

Added an extra sentence "C_{nat} includes preformed DIC as well as DIC from the biological and carbonate pumps".

L28 I think it's an oversimplified definition of the ML, especially since this is core to the analysis of the paper. Something like « The oceanic mixed layer (ML) corresponds to the near-surface layer of the ocean where turbulent processes, primarily induced by wind forcing, buoyancy fluxes, and wave breaking, maintain quasi-homogeneous temperature and salinity profiles. It represents the portion of the ocean directly interacting with the atmosphere, where weak vertical gradients may still persist. The depth of the ML is generally defined from a threshold criterion based on potential density or temperature relative to surface values. » gives more ground to set the scene

We appreciate the reviewer's detailed suggestion. The proposed text has been incorporated in the Introduction as provided.

L34 did you define NA in the intro?

Added the acronym definition.

Section 2.2: overall I am missing the reason why you want to use all of these different products in your comparison (I think that would be Table 1?)

We have now clarified this point in Section 2.2 when introducing the ocean products : "Ocean reanalyses are data-driven products with varying levels of complexity, and each individual product has its own strengths and limitations. Using the mean concentration across products helps mitigate product-specific biases and errors, while the spread among them provides an estimate of the associated uncertainty.".

Using the ensemble mean concentration therefore reduces individual product limitations. This is shown in Table 4, which shows a better agreement between the observations and the multi-product mean than between the observations and any single product (sections 2.8.2 and 2.8.3).

L104 which version of GOBAI-O2 did you use? Latest version is 2.3 and I would recommend using 2.1 onwards

We used version 1.1 at the time of our computations (recent versions were not available at that time). This has now been specified in the main text.

Figure 1: excellent figure - one point though, now that I see Cnat in this context I'm not sure nat is the best abbreviation as Cnat usually directly speaks to the BCP contribution rather than « all of the rest of DIC once we removed Cant »

As recommended, we have clarified the definition of C_{nat} in the introduction. It refers to all dissolved inorganic carbon that is not of anthropogenic origin. We think that there is no more ambiguity at this stage.

L115 PyCO2SYS - which version?

We used version 1.8 at the time of our computation, now specified in the main text.

Section 2.4: I am a bit skeptical about the use of multiple NNs here. I understand using ESPER NN T and S for preformed DIC (so only physical transport) but not to derive O2. Also, did you propagate uncertainty from the input data into ESPER and then into CANYON-B-CONTENT? I'm afraid the uncertainties might be very big. For transparency, I'd add some statistics for each step in this section.

Errors are calculated in section 2.8. This is now indicated in the first sentence of section 2.4: "Two different NNs were sequentially applied to ocean reanalysis data to estimate [DIC] and the evaluation of the error is detailed in section 2.8. Each NN has limitations, and the use of two different NNs was intended to build on the strengths of each NN.". We believe that using ESPER to estimate O_2 is a reasonable method. We were able to verify this by comparing the NN DIC and C_{ant} estimates with the A25 bottle measurements. The error in the DIC estimate based on an O_2 value estimated by ESPER is 9.7 μmol/kg, whereas it is 9.5 μmol/kg if we use measured O_2 (see Table S1 and discussion in section 2.8). The error in the C_{ant} estimate based on an O_2 value estimated by ESPER is 5.1 μmol/kg, whereas it is 4.7 μmol/kg if we use measured O_2. The difference is small and, in our opinion, validates the use of ESPER to estimate O_2. The error on the estimation of O_2 by ESPER is equal to 7.8 μmol/kg (Table S1).

We added in 2.8.1: "The use of ESPER to estimate O_2 was validated by comparing NN-derived estimates against A25 bottle measurements. DIC RMSD increases only marginally when using ESPER-estimated rather than measured O_2 (9.7 vs. 9.5 μmol/kg, Table S1), as does C_{ant} RMSD (5.1 vs. 4.7 μmol/kg, Table S1), supporting the suitability of ESPER for O_2 estimation in this framework."

To estimate the error, we used the measurements (concentrations, property transports) from A25 as the ground truth. The error in the NN estimates is therefore estimated relative to this ground truth, rather than by error propagation. We elaborate on this response below, following another one of your comments.

Also, using CANYON-B to retrieve macronutrients from GOBAI O2 is an excellent idea, but I believe the time component is not included in the CANYON-B algorithm. One would assume this is okay since the changes in the input parameters will have time varying changes but maybe having a check on that in the supplementary material would reassure the readers

As you noticed, CANYON-B is not using the time component in its computation for nutrients. Here, we are using ESPER for nutrient predictions. Taking nitrate as an example, we found a relatively low error for the 2002-2018 period: 2.8 μmol/kg (RMSD between nitrate bottle measurements at A25 and estimations by ESPER with bottle T, S, pressure). This is now included in the error section on hydrographic data: "ESPER also retrieves confident nutrient values. For example, nitrate shows an RMSD of 2.8 μmol/kg with the A25 bottle data."

Overall, I think you need a very robust uncertainty propagation section and 2.8 seems a bit weak in that regard (or maybe not detailed enough at this stage)

We thank the reviewer for giving us the opportunity to clarify this point. While we acknowledge that uncertainty propagation is a traditional method for assessing the error on an integral quantity (e.g., average concentration per layer or property transport by uMOC), it requires assumptions regarding the correlation structure between variables that are actually unknown. For this reason, we instead base our uncertainty assessment on direct comparison with observation-based results. For transparency, we have added this clarification in the introduction of Section 2.8:

"Using error propagation to calculate an error on an integral quantity (e.g., average concentration per layer or property transport by uMOC) would require postulating the error correlation between different variables. This correlation is unknown. Our method of direct comparison with observational results avoids this issue, enabling us to compare final integrated values to reference hydrographic data." On a single concentration, our approach gives similar results as error propagation. The latter was assessed using data from 3 Argo-O_2 floats by (Asselot et al., 2024) and a NN-based estimation approach similar to ours. They found errors on DIC and C_{ant} (10.5, 5.9 μmol/kg respectively) similar to ours (9.7, 5.1 μmol/kg respectively).

We added at the end of 2.8.1: "Our concentration errors are consistent with those derived from error propagation by Asselot et al., 2024, using 3 Argo-O2 floats data and a similar NN-based approach. They reported DIC and C_{ant} errors of 10.5 and 5.9 μmol/kg, respectively, compared to 9.7 and 5.1 μmol/kg in the present study using reference hydrographic data."

Section 2.8.1 did you take into account the recent paper by Bushinsky et al., 2025 that assesses float oxygen offsets of approximately -2.7 μmol kg^{-1} at depth lead to an overestimation of surface pCO2 by +3.2 μatm - this would matter in the use go GOBAI O2

As you noted, GOBAI-O_2 uses Argo-O_2 data to train the algorithms and, hence, it might be affected by the induced bias at depth as notified by Bushinsky et al. However, we note that the use of GOBAI-O_2 shows better results than O_2 generated by ESPER. Most importantly, our error estimation method, based on the comparison to the A25 ground truth, includes any bias that may be induced by the use of Argo-O_2.

Bushinsky et al., 2025 is now cited in the main text. Bushinsky, S. M., Nachod, Z., Fassbender, A. J., Tamsitt, V., Takeshita, Y., & Williams, N. (2025). Offset Between Profiling Float and Shipboard Oxygen Observations at Depth Imparts Bias on Float pH and Derived pCO2. *Global Biogeochemical Cycles*, *39*(5), e2024GB008185.https://doi.org/https://doi.org/10.1029/2024GB008185

Specific comments to Results:

The results are well presented and supported by figures. However, there is some redundancy between Sections 3.1 and 3.2 that could be reduced by emphasizing the new insights instead of re-describing patterns already visible in the figures.

We have thoroughly revised Section 3.2 to reformulate or delete redundant material, and have also shortened Section 3.1 accordingly, also in response to RC2's suggestion). For simplicity's sake, surface comparisons for both C_{nat} and C_{ant} have been shortened and moved to the Discussion Section 4.5, and figure to Supplementary. Overall, we think these changes help emphasize the new insights presented in Section 3.2.

Section 3.1.1:The seasonal analysis is convincing, but the interpretation of the [Cnat] seasonal amplitude could be more explicitly linked to mixed-layer depth dynamics (beyond the schematic explanation in Fig. 3b). It might be worth quantifying the relative contributions of MLD deepening vs. biological activity (e.g., from satellite chlorophyll or primary production climatologies).

Thank you, we should have mentioned this. We add a purple dotted line in Fig. 3b to show the difference between the seasonal signal of C_{nat} in the ML and the seasonal signal of C_{nat} due to the deepening of the ML (with an annual C_{nat} value). We attributed this difference to biological activity acting on C_{nat}. We reformulated it in the caption of the figure to be clearer, and we added it in the method section as suggested by RC2. We also added this information in Section 3.1.1 dealing with ML C_{nat}: "To better understand the effect of varying the ML depth, we calculated the effect that the seasonality in ML depth applied to an annual mean [C_{nat}] profile would have (Fig. 3b, see 2.7), the difference between the two being related to the seasonality of the biological activity." and in the following sentence "the remainder in summer being attributed to biological consumption (Fig. 3b)".

We believe that adding information from satellite products that would warrant detailed explanation is out of the scope of this study.

Section 3.1.2: The interannual signal in [Cnat] (notably the 4–6 year periodicity) is intriguing. Could this be related to NAO variability? This connection would be worth testing or at least mentioning.

We found limited correlation with NAO variability (pearson correlation of 0.32 (0.55) between interannual NAO and interannual C_{nat} in uMOC (ML), now mentioned in 3.1.2.

Fig. 5:It might be useful to report confidence intervals on the trends (e.g., shading or ± values in the legend) to facilitate comparison across layers.

The confidence intervals of the linear trends (+/- confidence intervals at 90%) are now reported in the caption of figure 5.

Section 3.2 (Transports):The diapycnal vs. isopycnal decomposition is elegant. Still, the physical meaning of the "Test" estimator (Eq. 4) could be better integrated into the discussion — especially regarding how it relates to AMOC variability.

This estimator was not much discussed (1 sentence in the original manuscript) as it provides redundant information with diapycnal decomposition. We decided to delete it.

Also, the relative contribution of concentration vs. velocity variability to total transport (Fig. 8) could be more clearly quantified (percentages or variances explained).

We added in 3.2.2 the Pearson coefficient for the correlation between uMOC volume transport and natural/anthropogenic carbon transport, showing that changes in transport dominate changes in concentration. “Pearson coefficient correlation of 0.98 (0.99) is found between interannual volume and C_{nat} transport in the uMOC (lMOC), highlighting that changes in volume transport outweigh changes in concentration.” and for C_{ant} transport: “Pearson correlation coefficients of 0.91 and 0.93 were obtained between the two interannual transports for uMOC and lMOC, respectively.”

Uncertainties:The RMSD and propagated uncertainties are well computed, but they are presented late. I would suggest summarizing the main numbers (e.g., 1–2 μmol kg-1 for [Cant]) in the Results rather than only in the Methods, to remind the reader of the confidence level.

We summarize the main numbers for C_{nat} and C_{ant} at the beginning of the Results Section before presenting the results in concentration.

Discussion

The discussion is rich but somewhat descriptive in parts. It could benefit from a sharper focus on mechanisms driving the observed signals (ML deepening, AMOC variability, regional contrasts) and on how this study advances beyond previous works (Zunino et al., 2015; Pérez et al., 2013).

This was added at the end of the 1st discussion (Section 4.1): “This study corroborates previous estimates (Zunino et al., 2015; Pérez et al., 2013), which relied on cruise observations, and provides a more detailed perspective on C_{nat} and C_{ant} concentration and transport variability at seasonal to interannual time scales.”

Section 4.1: The role of the mixed layer in modulating both [Cnat] and [Cant] is clearly established. It would be interesting to discuss whether the modeled MLD variability in the reanalyses (e.g. GLOSEA5 vs. ECCO) could bias the amplitude of the seasonal carbon signal.

We added a discussion of this point in 4.5: “As for the inter-product comparison, GLOSEA5 simulates deeper ML values in winter and shallower ML in summer than ECCO (Fig. S2), producing higher (lower) ML [C_{nat}] values in winter (summer), thereby amplifying the seasonal carbon signal. In contrast, the weaker ECCO ML depth seasonal amplitude dampens the seasonal variability of this property. The opposite behavior is observed for [C_{ant}], owing to its opposed vertical gradient.”

Section 4.2: The link between AMOC strength and Cant transport trends is key. However, the text could more directly address whether the observed Cant increase is primarily due to atmospheric CO2 forcing or to changes in circulation pathways (uMOC thickening/thinning).

We added extra (and more direct) information in 4.3. "This rise is primarily driven by the atmospheric increase in CO_2 and the increase in air-sea CO_2 fluxes in the NA (Gruber et al., 2023), as the constant C_{ant} transport at 26.5°N between 2004 and 2012 would suggest it (Brown et al., 2021)." As well as "[C_{ant}] variability is dominated by long-term changes in concentration, due to atmospheric CO_2 forcing, while [C_{nat}] variability is dominated by seasonal and interannual changes.".

Section 4.3: The authors mention good agreement between reanalyses and observations, but this could be quantified (e.g. comparing GLOSEA5 and ECCO vs. bottle data).

It is quantified in the uMOC (in which all reanalyses are defined) in Tables 4, S3, S5, Section 2.8.2 and 2.8.3. We added the reference to the dedicated section in Section 4.1: "The agreement between reanalysis products and bottle data was evaluated and summarized in Tables 4, S3, S5, and discussed in Sections 2.8.2 and 2.8.3." and at the end of 4.3: "(see Section 2.8.3 and Tables 4,S5 for a comparison of ocean analysis products)".

Reviewer 2

General comments

The study describes a nearly 30-year monthly time series of natural (C_{nat}) and anthropogenic carbon (C_{ant}) concentrations and transports in the subpolar North Atlantic at the A25 section, using a combination of ship-based observations, ocean reanalysis, neural networks, and a back-calculation approach. As pointed out by the authors the study is the first quantitative assessment of C_{ant} seasonality in the SPNA, making it impossible to validate the findings. Nevertheless, the agreement in transports and changes from other studies is impressive and give confidence in the approach. It is a very interesting and mostly well written work, which deserves publication. I have some mostly minor comments that should be dealt with before publication.

One comment is on the Results section. This is fully packed with details and frankly took me some time to get through. This may in part be needed when there is so much information to juggle but this makes it less approachable for the reader. It is difficult to put the finger on specific parts, but I simply ask the authors to go through the Result sections and see if something could be described somewhat more accessible. The Discussion and Conclusions are all excellent.

Thank you a lot for your comments. All the authors agreed that your remarks improved the clarity of the manuscript.

We tried to reformulate whenever possible the Results section to make it more accessible, as also pointed in RC1. We suppressed unnecessary details, in particular in section 3.1 and 3.2.

We answered your specific comments in the following in blue.

Specific comments

L67: This is a minor remark, but it should read “GO-SHIP”. (See also other instances in the manuscript.)

We changed GOSHIP to GO-SHIP in the manuscript.

L109 + 111: Since [C_{ant}] means “concentrations of C_{ant}” I am not sure this should be used here. “The inventory of ...” would be “C_{ant}” and not the concentration of C_{ant}, right? Same for “the influence of ...”. The same may be true for L274.

[C_{ant}] was changed to C_{ant} in L109+111, L274 as well as wherever we reported “fraction of ... “ or “... fraction“.

P5, Fig. 1 caption: The EGC is shown in the figure but not defined in the caption, and WBC is defined in the caption but not shown in the figure.

We changed the figure label to WBC, and added in the caption “ ..(WBC) including the East Greenland Current (EGC) ..” .

Also, a comma after “respectively” after “top panels”. (L4 of the caption)

Comma added after “top panels”.

L260-261: The transport numbers presented, are they shown in a table or figure? I was looking in table 3, but don’t fully recognise the values.

We reported uncertainties on the transport numbers computed from A25 observations and not from reanalysis. This was misleading. This information has been moved to Table 3 caption.

P14, Fig. 2: As far as I can find this figure is first referred to on page 18, and there in the figure caption of Figure 4. Since Figure 3 and 4 are referred to before, you should renumber this figure (Fig. 2 that is) for clarity.

As Fig. 2 is the key figure of the concentration time series (before seasonal and interannual filtering), we added reference to it before Fig. 3 and Fig. 4 for consistency in the manuscript.

L306-07: It is not clear to me how this is calculated (the seasonality by taking an annual value of [C_{nat}]). There is a reference to Figure 3a, and the figure caption say something more (but not enough, to me), but this should also be added in the methods, as I see it. (I may of course have missed this information.)

An explanation was added at the end of Section 2.7: “To better quantify the effects of seasonal changes in ML and uMOC thickness on the seasonality of [C_{nat}]and [C_{ant}], we calculated an idealized seasonal anomaly for ML and uMOC using the annual mean vertical profile of the property ([C_{annual}]), rather than monthly varying concentrations, and the full variability of ML depth and uMOC thickness. This approach isolates the impact on ML concentrations of physically-driven seasonal changes in layer thickness from biologically-driven seasonal variations in concentration. "

L319-321: Since it has been stated that C_{nat} is much larger than C_{ant} the difference in amplitudes is of course expected. The same goes for the opposite max/min of C_{nat} and C_{ant} with deepened ML.

We deleted the two sentences that were providing too much detail.

L403: Be clear in the phrasing; the net transport does not define a layer (as the sentence may be read as).

We delete "other" and give their name.

L411: These two maxima for the seasonal net signal are very small, and in addition there is another small maximum in June, and the value in December is as large as the maxima in March and September.

We delete the monthly description for the seasonal net signal, and simply highlight that it is "very small".

L450-451: The way I read Fig. 8c is that lMOC shows no trend between 1993 and 2008/09, after which it increases. Thus, saying it is an increase over the time series is misleading.

We reformulated: "In contrast, the lMOC C_{ant} southward transport shows no trend until 2008, after which it increases. This results in an overall increase between 1993 and 2021 from -0.19+/-0.03 PgC/yr to -0.32+/-0.04 PgC/yr (0.047 PgC per decade, Fig. 8c)."

L554-555: Maybe more a technical comment but there must be some words missing in this sentence, between North Atlantic and Steinfeldt.

Added parenthesis.

L558: Does this need to be true what you are suggesting? The deeper ML would lead to a minimum in [C_{ant}], and wouldn't this be true even if the [C_{ant}] in the NAC was higher or unchanged? Possible I am misunderstanding something, but please have a look if this can be made clearer.

You're right. As you noted, there might be two mechanisms that explain the reduced C_{ant} increase. We rephrased it in the discussion section and added: "In addition, deeper ML (as in the 2014 decade) would lead to a minimum in [C_{ant}], even if the [C_{ant}] from NAC was higher or unchanged."

L590: The 36°N section is not defined in Figure S9. Yes, it is section A03 (right?), but please make this clear.

We now refer only to A03 for consistency.

L613-14: From Fig. 3b I can't see an order of magnitude difference, but then I read that you refer to the difference between ML and ML annual, but maybe this is not true? Please make this clearer. If you compare the variability between C_{nat} and C_{ant} then yes.

We clarified the text. We were comparing variability between C_{nat} and C_{ant}, and then variability between direct ML concentration and ML concentration for an annual mean profile of property.

L662: It may be suitable to mention the lack of observational evidence of this reduction until today. If there are indeed any documentations of such a change then refer to that.

We added a reference on the observational reduction: "We speculate that the projected reduced AMOC, which may already have begun (e.g. Caesar et al. 2018), will reinforce the [C_{ant}] gradient between the upper and lower limbs of the MOC as the accumulation of [C_{ant}] at depth will be less rapid and C_{ant} will continue to grow.".

Technical comments

Table captions: This is a very minor remark but there is an inconsistency in the placement of the table captions, where the ones in the main paper are below the tables and the ones in the Supplementary are found above. Typically, I think they should be on top, but this may of course differ among journals.

We put all captions on top.

L182: A typo: remove "Irminger" (in the "East Greenland Current".

L182: We removed "Irminger"

L220: “RMSD” is used here for the first time (unless I missed an earlier one) but only defined in the paragraph after.

Added the definition of the acronym at the correct place.

L265: Should this refer to Table 3, and not 4?

We also added the reference to Table 3, as the number is as well on Table 4.

L305: You refer to Fig. 1b, but there is no such figure (only Fig. 1).

We changed the reference to Fig. 1.

L325: Maybe use “hence” instead of “for”?

Changed to “hence”.

P20, Fig. 5: The numbers/details of the figure are difficult to read due to the small size/font. Maybe this is mostly in the printout, but please consider this when revising the manuscript.

We redo the figure with bigger tick labels and, and make it bigger in the manuscript.

L438: Do you mean “magnitude” here (instead of “amplitude”)?

We meant “magnitude”, now changed in the text.

L532: Should it be “average” instead of “averaged”?

It should be “average”, changed in the text.

Supplementary Information:

The table caption information should be made clearer for Tables S2-S5. Now it reads “Table 3 details” (for Table S2) or “Table 4 details” (for Tables S3-S5).

We specify the specific detailed variables in each supplementary Table at the beginning of the caption to better readability. We also added “The spread between ocean reanalysis is illustrated by this Table.” or “The spread between A25 years is illustrated by this Table.” when necessary.

Fig. S3-S4: The figures are very small making it difficult to read numbers. Also, change “Iceland basin” to “Iceland Basin” in the caption.

We increased the size of S3-S5 figures in the Supplementary Information. We also changed “Iceland basin” to “Iceland Basin” in the captions of Fig. S3, S5.

Fig. S9: As mentioned earlier please define that section 36°N refers to section A03.

We only refer to A03 now for consistency.