We would like to thank the reviewer for the time spending in reading the manuscript and for the valuable comments. Our replies are marked in blue.

Review on ”Decadal changes of anthropogenic carbon in the Atlantic 1990-2010”
by Steinfeldt et al., 2023

A Summary of key results

Steinfeldt et al., 2023 use a modified TTD method based on tracer data such as CFCs and SF6 to quantify the anthropogenic carbon inventory in the Atlantic over the last decades between 1990 to 2010. Compared to previous TTD approaches, mixing of deep water masses with no signature of CFCs is accounted for in the inventory estimates. As a results, the authors find an increase in carbon inventory over time in response to increasing atmospheric CO2 concentrations. The results are extensively discussed in reference to other techniques estimatinganthropogenic carbon.

B Originality and significance

The manuscript is a valuable contribution to the increasing body of literature that aims to quantify the inventory of anthropogenic carbon based on different approaches. Novel is the adapted TTD method that allows an analysis of deep water mixing with no signature of CFCs, i.e., water masses that have not seen the anthropogenic perturbation, however, the details of that approach need clarification. The strength of the paper could have been increased, if the authors had decided to include the most recent reinvigoration of the North Atlantic carbon sink since 2014.

We will calculate the C_{ant} inventory for the period 2014-2020 with the recent version of GLODAPv2.2023.

C Data and methodology: validity of approach, quality of data, quality of presentation

All studies using set time periods to describe decadal changes or shifts in the inventory of anthropogenic carbon are bound by the availability of data, i.e., the calculated storage rates are strongly dependent on the choice of start and end date. Here, the length of the 3 time periods are different (Line 73). This seems arbitrary, why did you choose these time periods, and how dependent are your calculated inventory changes based on the chosen time periods?

The length of the 3 time periods is not exactly equal. We have chosen the time intervals to get a good coverage ot the tlantic for 3 time periods. E.g., excluding the years 1995 and 2005 from the second period would lead to large data gaps in the South Atlantic. We added:‘The periods are chosen to allow for a good data coverage of the Atlantic.‘

How do you account for bias due to different data density in different time periods?

This is hard to estimate. However, for the revision we will use data from 2006-2013 instead of 2006-2014 in the South Atlantic. Also, the total number of cruises, which are used for the climatology, has increased. The changes towards the old results will allow a kind of estimation.

To better ensure reproducibility of the results, I would appreciate a clearer and more precise presentation of a step-by-step post-processing of the cruise data (or even the code shared or gridded data products made available) as this is quite extensive for this study. From times, more precise information is found in the appendix, while sometimes results are presented in the method section already. I suggest to revise the method section accordingly.

We will move some text from the appendix to the main part. We will also publish the gridded fields on which the results are based.
1
The cruise data in GLODAPv2 is extensively bias corrected through the crossoveranalysis between cruises. How does adding other cruises without that bias correction affect the results of anthropogenic carbon inventory estimates? This is partly described in the appendix, but it should be moved to the main section to avoid confusion.
We will change the separation between main paper and appendix accordingly.
I really appreciate the extensive discussion of the impact on anthropogenic carbon estimates by the shape of the TTD through different ratios between mean age and the width of the TTD. As a reader, it could be great to understand better the implications of these shaper parameters, i.e., a stronger discussion on why in specific regions or water masses or depth layers the relative importance of mixing over advection changes.
The question, ‚why' the Delta/Gamma ratio changes in specific regions, is hard to answer. In l. 219-224 we have discussed some principle features.
The overall presentation of the results is clear and the style is concise. I recommend some overall editing of the language as there are some sentences and sub-clauses that seem to belong together.
We will do an overall editing after the main revision has benn finished.
D Appropriate use of statistics and treatment of uncertainties
The TTD methods relies on a number of assumptions as well, which should be discussed in more detail in the manuscript, especially when referring to shortcomings of the back-calculation methods:
How does the choice of degree of saturation affect your results and can you quantify the uncertainty that stems from assuming the same prescribed age tracer saturation history for both CFC-12 and SF6 (and CFC-11); also a constant de-gree of saturation over time? The choice of saturation of CFCs and SF6 in different density layers clearly affects the water mass age estimates and finally anthropogenic carbon estimates (see e.g. He et al., 2018, that shows that saturation history is a large source of uncertainty). Further, Tanhua et al., 2008, find different anthropogenic carbon concentrations estimated with different time-dependent saturation values for CFC-12 and SF6.
The C_{ant} error from a range of (realistic) tracer saturatons has been dealt with in Steinfeldt et al. (2009), the result has been quoted here. We will repeat some of the information from Steinfeldt and al. (2009) and will also quote the results from Tanhua et al. (2008) and He et al. (2018).
There is also an indication thatthe saturation degree for CFC-12 and SF6 differs during water mass formation, shown in Fr¨ob et al., 2016, who actually find excess SF6 during active convection in the Irminger Sea, i.e, supersaturation up to 115% at the base of the mixed layer, which is not observed for CFC-12.
We think the findings from Fröb et al. A bit questionable, as the SF6 oversaturation was only found at the base of the mixed layer, but not throughout the whole mixed layer.
You assume a steady state ocean and a constant degree of mixing, which in regions of infrequent deep water formation clearly is not the case. How do the qunatified anthropgenic carbon concentrations change if TTDs are either calculated over different time periods compared to the entire period?
We are not sure what is meant by ‚TTDs calculated over different time periods'. A TTD always covers the complete age range from zero to infinity. Of course the TTDs calculated from data for different years differ, otherwise the C_{ant}

anomalies would be zero.
Further, parameterized preformed alkalinity in the φCT method accounts for temporal and spatial changes in the ocean air-sea CO2 disequilibrium over time; a benefit of this approach over the TTD method. This should be accounted for when comparing the results by e.g. Perez et al., 2010 (ca. Line 430).
We added to the text:'One of these is that the φCT method takes into account changes in the ocean air-sea CO2 disequilibrium over time.
One remark to the air-sea disequilibrium: The rising atmospheric CO2 concentration is not the only reason for changes in this diswquilibrium. Also natural variability of the water properties (temperature, salinity, alkalinity) can influence the air-sea disequilibrium, without havinfg an effect on $C_{ant.}$
Overall, the estimated uncertainty of the anthropogenic carbon inventory estimates and storage rates should be added throughout the manuscript.
We will mention the errors given in the tables also in the text.
E Conclusions: robustness, validity, reliability
Looking at 2 decades of data does not allow to state that "only a reduction of ventilation over several decades would severely change this relationship" (Line 11). Further, you do not take the period after 2014 into account that clearly shows an increase in deep water convection and subsequent increase in anthropogenic carbon storage rates, i.e, there could be evidence in data for the impact of deep convection on Atlantic carbon storage in relation to patterns of atmospheric variability and circulation changes. I find the final statement in line 615 and following therefore rather weak and unsupported as there is no or not yet a permanent decrease in ventilation rates.
We agree with the reviewer that the reinvocation of deep convection in the North Atlantic might lead to enhanced carbon storage. Including the time period 2014-2020 in our analyses will provide information on that topic. We have not claimed that there has been a ‚reduction of ventilation rates over several decades'. Our results show that the variability in the North Atlantic and elsewhere between 1990 and 2010 has NOT significantly changed the oceanic storage of C_{ant}. Hence, our conclusion is that longer periods (‚several decades') of weak convection would be necessary to show a basin wide effect. However, we will skip the ‚speculative' statements in l.11 and 615.

F Suggested improvements: experiments, data for possible revision, minor comments

Line 18: Reference? Is this the total or natural variability? We deleted the sentence about the +- 5% variability.

Line 61: TTD has also a steady state assumption
The TTD itself does not have a steady state assumtion. A TTD always exists, even in the case of temporal variability. The Inverse Gaussian function used to parameterize the TTD has a ‚steady state' assumption in that way that this function is a solution to the one-dimensional advection-diffusion equation with constant velocity and constant diffusion. None of these ‚assumptions' is fulfilled in the real ocean: The real ocean is 3-dimensional, and velocity and diffusivity are neither constant in space nor in time. This implies that the Inverse Gaussina function can only be an approximation to the ‚real' TTD. In line 187 we now make cleat that the Inversa Gaussian function is an approximation tothe ‚real' TTD.

Line 90: Do you correct for atmospheric CO2 concentraion increase when calculating a climatology for Cant based on data between 1982-2014?
Yes, C_{ant} has been calculated from data between 1982-2014 for a common reference year. This is describes in l.228-230.

Line 91: In which region or which depth layers does the sparsity of data lead to gaps in the gridded data product, i.e., are there some regions more affected thanothers? We will show the regions with data gaps for the different decadal periods in a modified version of Fig. 1.

Line 93: Why can you fill the gaps in the decadal fields with data from the climatology, given that there are changes in decadal storage rates of anthropogenic carbon? Of course the climatology does not contain the temporal variability and changes in C_{ant} storage. We have now added to the text: This might lead to an underestimation of the decadal variability of the C_{ant} storage. On the oher hand, in the regions with high temporal variability, especially the North Atlantic, the data coverage is sufficient to reproduce the temporal changes.

Line 103: Over the period of 2 decades, the signature of the water masses considered here also changes, i.e., due to warming/cooling and or salinification/freshening the density structure of these water masses changes, e.g., ISOW has become warmer and saltier. How can you account for the different contributions of water masses? In the section here, we are mainly interested in the mean state. We discuss the impact on changes in water mass formation/ventilation in section 3.3.2. In principal, we assume a constant density range for the water masses. The change of the core density, e.g. for LSW, is clearly seen in the C_{ant} anomalies discussed in section 3.3.2.

Line 179: please rephrase statement, unclear
To make the statement clearer, we added: , i.~e. a saturation difference of 20% leads to a Cant difference of about 10 %.

Line 193: cite e.g. Smith et al., 2011 Done.

Line 205: Is it possible to use a ratio of CFC-11/CFC-12 or CFC12-SF6, thereby constructing a different atmospheric history, a consequently different tracer source function at surface and TTD - could that prolong the potential use of CFCs be-yond their peak concentration in the atmosphere?
Using the CFC-12/SF6 ratio does not give more information than using CFC-12 and SF6 together to determine the TTD parameters Delta and Gamma (if the observed CFC-12 and SF6 values are reproduced by the TTD, then also the ratio is correct). It is thus true that CFC-12 is still useful. The same holds for CFC-11. Recently, in young waters the CFC-11/CFC-12 ratio has decreased significantly, makes the CFC-11/CFC-12 ratio (or combined CFC-11 and CFC-12 concentrations) more useful than in earlier times, where the ratio was close to 0.5, and variations were in the range of the measurement precision. Unfortunately, CFC-11 is not measured as frequent as CFC-12 anymore. We added in the text:
‚If CFC-12 is measured simultaneously, it is used for the calculation of the Delta/Gamma ratio.‘

Line 210: What are the temporal, spatial and depth boundaries to exclude SF6 data due to the tracer release experiment? It is for the whole period (SF_6 measurements are only available from 2003 onwards) and for the whole deep and bottom water range. To make that clear, we now write ‚Note that SF_6 is not used at all in the deep and bottom waters.

Line 213: reference? Not sure for which statement a reference is required.

Line 236-255: Better start new section. This also mixes results and method. We start a section ‚C_{ant} increase for the standard TTD method' here.

Line 268: The threshold of 100 years seems like an arbitrary choice. It is in a way arbitrary. On the other hand, ages younger than 100 yr only occur in and close to water mass formation regions, where a dilution with waters free of anthropogeic tracers is unlikely. We added this explantion to the text.

Line 281-298: again, are these not results? Yes, but they are strongly related to the TTD method with dilution. We would prefer to keep this paragraph within the ‚Method' section.

Line 303: How do you validate the dilution factor?
Not sure what is meant by that. The results shown in Fig. 5 are theoretical and not ‚validated'.

Line 320: Why √4 ? I am not sure I fully understand this paragraph, can you please revise/rephrase? There are 4 water masses with independent errors of the TTD parameters, that is why we divide the error by sqrt(4).

Line 398: section 3.2. We will change the section numbering.

Line 434: Please specify, as Perez et al., 2010, analyse different smaller regions (Irminger and Iceland Basin), and different time periods. Further, their definition of water masses at depth may reduce comparability.
We have already mentioned the differenet time periods in Perez et al., 2010, in l. 427. The regions in Perez et al are indeed snaller as the boxes in our Fig.8, but also the section plots in Fig. 9 do not show such a large difference in C_{ant} storage between 1990-2000 compared with 2010-2020 (l.431-433). The definition of water masses does not have an impact on changes in the total column inventory, which we do compare here.

Line 590: This is unprecise as overflow waters are found also above 3000m, while Gruber et al., 2019, correct only for anthropogenic storage below that depth level.
That is correct, we mow write :Below a depth of 3000 m, Grubr et al. (2019) could not find a significant increase in C_{ant} and thus added an estimated C_{ant} storage of 1 Pg C for that depth range.

Line 637: Denmark Strait
corrected

Line 690: Missing description for figure in appendix D.
We add a short desription of figure D1 here.

G Figures
Figure 1: I find it hard to see data density based on these maps. I assume background color shows the bathymetry of the basin. Could it be an option to show contours of data density instead to illustrate the gaps that need to be filled?
We could show the areas with data gaps for each time period, but these areas are also dependent on depth/density layer. This makes it difficult to show them in one horizontal map, but one could think of show one map with the areas her ethere are data gaps in all density layers.

Is it possible to highlight where CFC-11, CFC-12, CFC-113, Tritium, and SF6 data are available?
CFC-11 and CFC-12 are both available in most cases, so the maps for CFC-11 and CFC-12 would look very similar. We will add one figure where we show all tritium/CFC-113/SF6 data that have been used to infer the Delta/Gamma ratio.

Figure 2: Light and dark grey lines are not distinguishable. Avoid rainbow color scale (applies to all figures).
We enlarge the contrast between the light and dark grey lines and choose another color map.

Figure 4a, b: Can the fraction be between 0 – 0.25?
No, as 0.25 is the minimum possible value or the fraction of young water (see l. 273).

Figure 8: How exactly are the regions defined over which mean storage rates are
shown?
The regions follow the boundary between western and eastern Atlantic given by the Mid-Atlantic Ridge. We add that to the figure legend.

Figure 8/9: What does the stippling mean? Most regions in the South Atlantic are stippled, are these changes all not significant over the time periods considered?
Yes, the stippled regions have insignificant changes.

H References
Tanhua et al., 2008, https://doi.org/10.1029/2007JC004416
Froeb et al., 2016, https://doi.org/10.1038/ncomms13244
Smith et al., 2011, https://doi.org/10.1029/2010JC006471
He et al., 2018, https://doi.org/10.1002/ 2017JC013504