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 SF_6 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 CO_2 concentrations. The results are extensively discussed in reference to other techniques estimating anthropogenic 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.

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

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.

The cruise data in GLODAPv2 is extensively bias corrected through the crossover analysis 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.

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 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.

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 SF_6 (and CFC-11); also a constant degree of saturation over time? The choice of saturation of CFCs and SF_6 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 SF_6. There is also an indication that the saturation degree for CFC-12 and SF_6 differs during water mass formation, shown in Fröb et al., 2016, who actually find excess SF_6 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.

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?

Further, parameterized preformed alkalinity in the ϕC_T method accounts for temporal and spatial changes in the ocean air-sea CO_2 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).

Overall, the estimated uncertainty of the anthropogenic carbon inventory estimates and storage rates should be added throughout the manuscript.

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.

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

Line 18: Reference? Is this the total or natural variability?

Line 61: TTD has also a steady state assumption

Line 90: Do you correct for atmospheric CO_2 concentraion increase when calculating a climatology for C_{ant} based on data between 1982-2014?

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 than others?

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?

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?

Line 179: please rephrase statement, unclear

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

Line 205: Is it possible to use a ratio of CFC-11/CFC-12 or CFC12-SF_6, thereby constructing a different atmospheric history, a consequently different tracer source function at surface and TTD - could that prolong the potential use of CFCs beyond their peak concentration in the atmosphere?

Line 210: What are the temporal, spatial and depth boundaries to exclude SF_6 data due to the tracer release experiment?

Line 213: reference?

Line 236-255: Better start new section. This also mixes results and method.

Line 268: The threshold of 100 years seems like an arbitrary choice.

Line 281-298: again, are these not results?

Line 303: How do you validate the dilution factor?

Line 320: Why $\sqrt{4}$? I am not sure I fully understand this paragraph, can you please revise/rephrase?

Line 398: section 3.2.

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.

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.

Line 637: Denmark Strait

Line 690: Missing description for figure in appendix D.

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? Is

it possible to highlight where CFC-11, CFC-12, CFC-113, Tritium, and SF_6 data are available?

Figure 2: Light and dark grey lines are not distinguishable. Avoid rainbow color scale (applies to all figures).

Figure 4a, b: Can the fraction be between 0 - 0.25?

Figure 8: How exactly are the regions defined over which mean storage rates are shown?

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?

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