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Biogeosciences Discussions

Interactive comment on "Changes in column inventories of carbon and oxygen in the Atlantic Ocean" *by* T. Tanhua and R. F. Keeling

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1 Summary

Tanhua and Keeling determine the change in the column inventories of carbon and oxygen by comparing the vertically integrated amount of these quantities for station pairs that closely located to each other in space, but are separated substantially in time. By merging two data set for the Atlantic ocean (CARINA and GLODAP), they were able to identify a total of more than 2300 station pairs across the basin, for which the change in storage could be computed. They find a mean storage rate of 0.7 mol m^{-2} yr^{-1} for DIC, with a 9 to 95% confidence interval of 0.65 to 0.78 mol m^{-2} yr^{-1} . For oxygen, a mean loss rate was identified of -0.5 mol m^{-2} yr^{-1} with a confidence interval

of -0.64 to -0.45 mol m^{-2} yr^{-1} . The authors emphasize the importance of changes in carbon storage beyond the uptake of anthropogenic CO_2 from the atmosphere.

2 Evaluation

The accurate determination of the net oceanic sink for atmospheric CO_2 is one of the most important challenges driving ocean carbon cycle research. The net uptake and ultimately the change in ocean carbon storage is the sum of two fluxes with different drivers: The uptake flux of anthropogenic CO_2 that is driven by the increase in atmospheric CO_2 and the net flux of natural CO_2 that is driven by changes in the oceanic carbon cycle and its controlling factors. Globally and over longer timescales, the uptake flux of anthropogenic CO_2 clearly dominates, but at regional scales and when investigated over shorter timeperiod, the natural CO_2 component can be as important. The same conclusions apply for the changes in carbon storage.

So far, nearly all methods developed to determine the change in ocean carbon storage through time have focused on the anthropogenic component. The identification of this component requires a number of assumptions, many of which are not well tested or fully understood. This leads to a substantial amount of uncertainty in the estimates, and also capture only one part of the ultimate question, i.e., how large is the net oceanic CO_2 sink?

Tanhua and Keeling now propose and apply a new and conceptually very simple approach to tackle this problem. They forgo the separation into the natural and anthropogenic CO_2 components and instead focus directly on the changes in their sum, i.e., the total change in inorganic carbon. They also forgo an analysis of the depth distribution and instead look at the vertical integral. These two simplifications make the challenge quite tractable, and permit them to estimate the change in storage with a reasonable amount of confidence. Thus is a very valuable and important contribution to our field and will help us to better determine the net carbon sink by the oceans.

The paper is generally well written, properly illustrated, and the discussion and conclusion build solidly on the presented results. The topic is highly relevant and fits well into the purview of Biogeosciences. I am overall strongly in favor of this manuscript and support its publication, but have a couple of major comments that I think the authors need to consider in their revision.

Implications of assumptions: While conceptually simple and attractive, the method makes a number of (explicit and implicit) assumptions, whose implications are not well identified and discussed in the current version of the manuscript. The most important one is that the stations are randomly distributed so that the averaging across a larger region leads to a proper characterization of the mean trend in that region. The station pairs are anything but randomly distributed. This is a source of concern and needs to be more thoroughly investigated. I consider this a much more serious issue compared to methods that focus on the storage of anthropogenic CO_2, since model simulations and theoretical arguments predict that changes in storage in total carbon will be much more spatially variable than changes in storage in anthropogenic CO_2. This is because the former is very sensitive to lateral changes in the position of major currents and watermasses, which mostly do not cause a change in oceanic net storage, but simply a lateral redistribution. Such changes have a much smaller impact on anthropogenic CO_2. Therefore, by forgoing the separation, Tanhua and Keeling are able to remove the assumptions associated with the separation, but they have become much more sensitive to the spatial distribution of their data.

A good indicator for the potential scale of this problem is the change in the oceanic heat storage, where there is a large amount of spatial structure in the data (see e.g. Figure 5.2 in IPCC's AR4), and only after averaging across the globe, the net uptake clearly emerges. Also the substantial sensitivity of the changes in heat storage to the distribution of the observations should remind us how difficult it is to determine the

change by comparing storage at two different points in time. I therefore recommend the authors (i) to discuss this issue much more up front in their ms, and (ii) to undertake an effort to assess this uncertainty. Potential approaches include the use of temperature as a proxy (i.e., can they retrieve the change in heat content seen from the much more dense temperature network) or using artificial data from a model.

Depth interpolation: A second major source of uncertainty and bias is the vertical interpolation. The authors permitted quite a large vertical separation in the bottle data, making them rather vulnerable to vertical interpolation biases. With DIC increasing in places by more than 100 μmol kg-1 over a few 100 m, a small error in interpolation translates very quickly into a large error in the integral. This can be illustrated well by putting numbers behind the inventories. The typical column inventory of carbon in the North Atlantic down to 2000 m is 4000 mol m-2. The signal that the authors want to detect is about 5 mol (0.5 mol m-2 yr-1 over 10 years), i.e., they need to determine each column inventory to better than 2 in 4000 or 0.5 permil. This is a great challenge given that there are typically less than 20 bottles in the top 2000 m! I recommend that the authors also test more thoroughly for this source of error. In addition, I suggest to make the maximally permitted spacing smaller and/or make it dependent on the vertical gradient, i.e., the quantity is not permitted to change more than x between the two depths.

Computation of confidence intervals: I very much appreciate the author's efforts to be transparent with the uncertainties, but I am not convinced that their computation of the 5 to 95% confidence intervals was done correctly. I unfortunately couldn't identify exactly how this was done, but I assume that they computed these confidence intervals on the basis of the assumption that that the individual estimates are independent and that they represent samples from the same distribution. With these assumptions, the uncertainty of the mean decreases with the square root of the number of observations. Neither of these two assumptions are really justified. First, the estimates from the different sites are not really independent (or uncorrelated), and second, the samples stem from different distributions (as seen by the non Gaussian distribution of the storage rates). I unfortunately don't have a straightforward recommendation how to come up with a better estimate (short of using a Monte Carlo type approach), but I do believe that the provided confidence interval is overconfident.

Cant versus DIC storage: I fully agree with the author's emphasis that the two storages are different, but the authors themselves blur the line every once in while, particularly when comparing to other estimates. Unless one makes the explicit assumption that the storage rate of natural carbon is small, one cannot compare the storage rate of total carbon (or that of abiotic carbon) to an estimate of anthropogenic CO_2.

3 Recommendation:

I recommend acceptance of this manuscript after moderate revision. I particularly recommend that the authors investigate the sensitivity of their results to their (implicit and explicit) assumptions and discuss the implications in the manuscript.

4 Minor comments:

p8040, line 1: replace "is" with "are".

p8040, line 8: "with few assumptions". perhaps write "fewer", to reflect the fact that "few" assumptions doesn't mean by necessity higher certainty.

p8040, lines 14-17: "where the uncertainty", "standard deviation". I had to read these two sentences many times in order to understand what is meant. I suggest to clarify this by using just one expression for the $\pm$ part, i.e., standard deviation. I also think that these two sentences could be simplified.

p8040, line 14: "trend in O_2". As written, one gets the impression as if all of the Atlantic is losing oxygen, yet the data show only a significant trend in the high latitude North Atlantic. I think this needs to be rewritten.

p8041, line 13, "hard to test assumptions". The two most widely accepted methods, i.e., the empirical Green Function Method by Khatiwala et al. and the DC* approach of Gruber et al. have actually been tested quite extensively and their error structures and weak points are by now well established (Matsumoto and Gruber, 2005, Wang et al., 2012). So, I don't think that one can make this statement across all methods, particularly not when the more established methods are not even explicitly mentioned.

p8041, first paragraph. In my opinion, most of this first paragraph could be cut. It puts the reader on a wrong track, since it talks largely about the methods that estimate the total amount of ant. CO_2 that the ocean has taken up since the preindustrial era, while the focus of this ms is on the change in storage. So, the reader is first drawn in one direction, and then has to be reoriented towards another direction. Why not start from the beginning with the change in Cant/DIC?

p8042, line 25. Levine et al. (2008 (JGR), 2011 (GBC)) investigated some of the assumptions inherent in the MLR and eMLR methods and studied the impact on the estimated changes in Cant. They seem highly relevant for the discussion here and ought to be cited.

p8047, equation 1: I consider the normalization of DIC with AOU as problematic. The normalization with phosphate is a much better choice. As defined, DIC_abio reflects the uptake (and release) of both carbon and (some) oxygen, so this is confounding the issue rather than giving a proper separation of the biological from the non-biological effects. Since phosphate is conserved globally within the ocean, a global normalization using a tracer equivalent to C* (Gruber and Sarmiento, 2002) or Cgasex properly reflect the net change in oceanic carbon.

p8047, vertical interpolation. See main comment above. These are very wide spacings.

p8048, line 5 "see below". I suggest to move the details from "below" to here. I found the chosen sequence somewhat distracting.

p8049/8050, results section. The issue of time span is given relatively little consideration until much later in the paper. The station pairs cover quite different periods and are likely not rather evenly distributed in time. At least, the authors should show the temporal distribution of the station pairs.

p8050, line 27: "together with the 95% confidence interval". How was this computed? See also my main comment above.

p8051, entire page: The authors mix apples and oranges here, as they compare their storage rates without proper caveating with previous estimates that mostly just consider the storage rate of anthropogenic CO_2.

p8052, line 1: "should reflect, in principle, the increasing uptake of CO_2 at the ocean surface due to increasing uptake of CO_2". This is not true. This just compensates for anomalous changes in DIC due to respiration since the water left the surface, but it does not compensate for the exchange of natural CO_2 across the air-sea interface.

p8053, lines 4-5: "for regions outside the SPNA, no significant trend in the column inventory of oxygen or AOU can be detected". In my opinion, this needs to be considered in the way the abstract is presented. See comment above.

p8054, first paragraph. Is this needed? I suggest to delete it. This would help to tighten the ms.

p8057, comparison with Stendardo and "neglecting temporal changes in the thickness of these water masses". An updated estimate will soon be available in the "in press" section of JGR. In there, Stendardo and Gruber estimated the oxygen changes taking also the change in water masses into account. A copy of that paper can be made available on request.

Figures 6-9: I found these figures difficult to read and interpret. I would recommend to represent them differently, so that the quantitative aspects are more discernible. My preference would be to plot the data against latitude, with different colors indicating different regions. If the authors decide to keep them as they are, I suggest to show just the North Atlantic, and to combine the data into one Figure. If they decide to keep the plots separate, the figure captions of Figures 7 through 9 should be corrected. It should read "as Fig. 6".

Nicolas Gruber August 22, 2012

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