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Biogeosciences Discussions

Interactive comment on "Anthropogenic carbon distributions in the Atlantic Ocean: data-based estimates from the Arctic to the Antarctic" *by* M. Vázquez-Rodríguez et al.

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Interactive comment on “Anthropogenic carbon distributions in the Atlantic Ocean: data-based estimates from the Arctic to the Antarctic” by M. Vázquez-Rodríguez et al.

Anonymous Referee #1 Received and published: 25 May 2008

Summary

Vazquez-Rodriguez et al. present the results of an intercomparison of 5 recently developed methods (or sub-methods) to reconstruct the concentration of anthropogenic CO2 in the ocean. On the basis of 4 representative cruises in the Atlantic Ocean, they

find that while these methods give relatively similar overall inventories, the spatial distribution of the reconstructed anthropogenic CO2 differs substantially. The Southern Ocean emerges as the region with the largest differences, but also substantial depth dependent differences were identified in many other regions.

Evaluation

With only two of the four major terms of the global anthropogenic carbon budget since the beginning of the industrial period being well established, i.e. the fossil-fuel emissions and the atmospheric CO2 accumulation, any additional constraint on the remaining two components, i.e. ocean and land uptake is of particular relevance. Sabine et al. (2004) provided such a constraint for the global ocean on the basis of a ΔC* based reconstruction of the global distribution of anthropogenic CO2 in the ocean. In the last few years, several additional methods have been developed, but so far, no systematic intercomparison of the various methods has been conducted.

In this manuscript, Vazquez-Rodriguez and co-workers undertake a first attempt at such a systematic intercomparison, thereby filling in a clearly identified need. As such, this paper is fundamentally well suited for publication in Biogeosciences and likely will attract good readership. But before I can recommend acceptance of this manuscript, the paper needs to be much improved and expanded. As it stands right now, the manuscript is little else but a presentation of a few key results. This is clearly insufficient for a publication in Biogeosciences, as the manuscript does not contain a discussion with substance nor does it provide a clear roadmap for how the identified differences can be reconciled and improved upon. I am fully aware that this is a difficult task for any intercomparison paper, but this is not an excuse for not making an attempt.

I have the following four specific recommendations for how the paper can be improved:

i) Provide detailed statistics on the distribution. For example, plot the Cant estimates of the various estimates against each other and discuss where the largest differences occur. Compute correlations etc, offsets, etc.

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Rather than using Cant-Cant scatter plots we have a set of three complementary statistics, that are normally used to build Taylor diagrams (Taylor, 2001). The three statistics: a) the ratio of the variances of the two fields (γ2 = σ2mod/σ2obs); b) the correlation coefficient (R) between the two fields; c) the root mean square difference (RMSD) of the two fields. One particular aspect to anthropogenic CO2 is that there is no “observed” field against which Cant reconstruction models can be compared, and this is why this tabular option was preferred to actually plotting a Taylor diagram. Still, we tried to present our results in the most visual way possible by adding the color codes. The results concerning new Fig. 5 have been included in section 3. They complement and support the main results already given in the manuscript, and add to the information given in Figs 3 and 4.

Taylor, K.E., Summarizing multiple aspects of model performance in single diagram, J. Geophys. Res., 106, D7, 7183-7192, 2001.

ii) Evaluate the different estimates in comparison to other tracers, particularly pCFC (don’t plot against [CFC], but against pCFC!), but consider also temperature.

New Figure 3 compares the average Cant estimates from all methods in the considered regions while plotted in order of increasing pCFC12. The general trend is as expected: Cant estimates and pCFC12 are positively well correlated for most methods. When this is not the case (ΔC* in the Southern Ocean, for instance), a small discussion on why this happens is introduced in the text (please, refer to the regional results given in section 3.1).

iii) Discuss the reasons for why the different methods arrive at these various estimates. What are the particular assumptions that lead to these distributions?

A brief introduction to the methods and their main assumptions is now given in section 2. This is intended to provide the reader with a better background to understand the caveats that potentially stem from the various methods and that are later discussed on the basis of the presented results. The most likely causes producing a particular

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Cant distribution from one of the methods are not discussed into one common section. Rather, they are dealt with as the analysis of the data progresses and it gets more in-detail with the help from the figures. This way through sub-sections 3.1 and 3.3, mainly, the most likely causes to produce a given Cant distribution are given, either in the case of apparently discordant distributions or to explain the general common patterns observed.

iv) Provide a roadmap for how we can arrive at improved estimates of anthropogenic CO2 in the ocean. For example, it is particularly intriguing to use the change in Cant, Delta Cant (for example by differencing two occupations) as a constraint, since model simulations as well as theoretical approaches (see for example the discussion in Tan-hua et al.) clearly show that Delta Cant and Cant are rather strongly related to each other.

The absence of absolute references against which Cant estimates can be checked represents indeed one of the highest handicaps when it comes to assess the accuracy of the predictions. Given this fact, any constraint that is imposed to Cant fields has to be based on either statistics or in waters that are presumably untouched by the anthropogenic CO2 intrusion. The present study addresses both issues: it shows a complete array of statistics (Fig. 5) and vertical profiles (Fig. 4) in the regions of particular oceanographic interest from the point of view of the carbon system and it pays attention to the Deep South Atlantic, where old water masses can be reasonably assumed to be free on Cant, also based on age and pCFC12 data (Fig. 3).

As said in iii), the potential weaknesses of the methods and the most likely causes to produce a given Cant distribution are given in the manuscript. They actually represent the “adjustment” that would be necessary in each method to yield results that would be more in accordance with the rest of approaches or with a theoretical Cant reference (if available at all). Cant from a given method is strongly correlated with ΔCant and with any other Cant distribution from a different method (Fig. 5).

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Previous comparison works have avoided drawing a conclusion as to which approach leads to the most reasonable results (Friis et al., 2006). However, there is consensus about a general direction: “…To decrease the uncertainty in future estimates of anthropogenic CO2 in the ocean, a variety of geochemical and empirical techniques, and models will have to be used in a synergistic manner (Wanninkhof et al., 1999)”.

Friis, K., A review of marine anthropogenic CO2 definitions: introducing a thermodynamic approach based on observations, Tellus 58B, 2-15, doi:10.1111/j.1600-0889.2005.00173.x, 2006.

Wanninkhof, R., Doney, S., Peng, T.-H., Bullister, J. L., Lee, K. And Feely, R. A., Comparison of methods to determine the anthropogenic CO2 invasion into the Atlantic Ocean. Tellus 51B, 511–530, 1999.

I could think of more analyses (e.g. plots of Cant versus (pCFC) age on isopycnals surfaces) etc. that would help to understand similarities and differences.

A second major comment I have concerns the English language. There is much room for improvement as well!

The sentence construction and coherence has been revised in this version. We hope you will find the writing satisfactory and easier to understand this time.

Recommendation

This is a potentially interesting paper deserving publication, but it is currently too weakly developed to merit a positive recommendation. I therefore can recommend acceptance only after a major revision.

Detailed comments

Abstract and elsewhere: Extrapolation: I find it overly ambitious to extrapolate estimates that pertain to a single set of sections to the entire Atlantic. I am particularly

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concerned in the subpolar North Atlantic and the Southern Ocean, where there are substantial east-west differences. I am aware that the authors attempted to take this into consideration, but what is the basis for the argument that all methods have the same scaling to the ΔC* based estimates? Isn’t it very feasible that an overestimate in one part of the ocean corresponds to an underestimate in another part? I therefore think that the total inventories need to be presented with much more caveat and uncertainty than they are presently associated with.

Thank you for this remark. In the revised version we have stressed the intention with which this extrapolation is made and the assumptions behind it (first paragraph in sub-section 3.2). The manuscript now states: “… Given the similarities here found in the general Cant distributions it is assumed that the scaling obtained for the ∆C* from Lee et al., 2003 can be applied to the rest of methods. Knowingly of the caveats attached to this practice, this assumption allows to calculate Atlantic inventories from the presented Eastern Atlantic basin results”.

Introduction and elsewhere (lines 17-21). The writing needs to be much improved. I take the following two sentences as an example:

"International effort has been focused to investigate the evolution of the oceanic sink of atmospheric CO2, and to understand how human activities interfere in this air-sea coupled system. The endeavour aims at gaining insight on the assessment of the future possible scenarios proposed by the Intergovernmental Panel on Climate Change (IPCC Fourth Assessment Report: Climate Change 2007)"

i) "has been focused": wrong tense. ii) second part of sentence is not well linked with first part of sentence. iii) "insight on": should be "insight into" iv) "endevaour aims at gaining insight on the assessment of": this can be shortened to " The goal is to assess" etc, etc.

All of the above have been corrected after your suggestions, and as for the rest of the manuscript, the writing has been revised and amended.

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Introduction, p1423, line 17: If I recall correctly, the fraction is 45% not 50%.

This has been corrected.

Introduction, p1423, line 25: It might be worth mentioning that the oceanic inventory does not only provide a constraint for (forward) ocean models, but also constitutes a key input for inverse estimates of the ocean fluxes of anthropogenic CO2 (e.g. Gloor et al., 2003, Mikaloff Fletcher et al. 2006; Gerber et al., submitted) as well as for global carbon cycle budgets as presented, for example, by IPCC.

Done.

Introduction, p1424, line 21: "validate". I doubt that such reconstructions can be used to "validate" models. However, they serve as useful estimates to "evaluate" the models.

Thank you for this observation. The word “validate” has been changed to “evaluate”.

Method, p1426: I suspect that more details are needed here in order to have the background needed to discuss the reasons for why the estimates differ.

A 200-300 words briefing describing each method and their main assumptions has been included now in this section.

Results, p1426, line 13: adjustment to common year: Please specify whether this was done for each method separately, i.e. using the Cant estimate of that method, or whether the same adjustment was used for all estimates?

First of all, please notice that after some reorganization of the paragraphs in the revised manuscript, this information is now given in the first paragraph from section 2 (Methods). It reads as follows: “…The selected cruises correspond to different years and thus Cant results had to be referred to the common year 1994 (GLODAP canonical year) to eliminate biases introduced by the effect of increasing atmospheric fCO2. This was done using data from time series of CO2 molar fractions (xCO2) and

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calculating from here the ratio of Cant saturation concentrations for the year of the cruise and the preindustrial era. The correction typically varied between 1-7 μmolůkg-1 of Cant depending on the sampling year, the potential temperature and salinity of the samples…”.

Results, p1426, line 22: setting conc. to zero: I am concerned with this procedure, as it will lead to biases in the inventories. I highly recommend to consider all observations (negative and positive).

When computing inventories, negative concentrations cannot be subtracted from the water column integral and they are therefore set to zero. This is a common practice in Cant inventory computation; see for instance Tanhua et al., 2007 (please, refer to their Fig. 5 caption).

Tanhua, T., A. Körtzinger, K. Friis, D.W. Waugh and D.W.R. Wallace, An estimate of anthropogenic CO2 inventory from decadal changes in oceanic carbon content, PNAS, vol. 104, no. 9, 3037–3042, 2007.

Atlantic inventories, p1431, line 17: As noted above, I am concerned with this extrapolation. It is not unreasonable, but it introduces a significant amount of uncertainty into the basin-wide inventories. These need to be properly acknowledged and discussed.

Please, refer to comment above (first point in the “Detailed comments” section).

Figures: The figures are of good quality (they will have to be printed fairly large), but they are all of qualitative nature. See my suggestions above for suggestions.

The newly added Figs. 3 and 5 are of a more quantitative nature.

Interactive comment on Biogeosciences Discuss., 5, 1421, 2008.