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Biogeosciences Discussions

Interactive comment on "An upgraded carbon-based method to estimate the anthropogenic fraction of dissolved CO_2 in the Atlantic Ocean" *by* M. Vázquez-Rodríguez et al.

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Dear reviewer,

The authors would like to thank you for your constructive and positive comments. It is very rewarding to learn that a scientist that is not a frequent "back-calculation" user has found that the description of the method was clear and that the brief review of the methodology given in the introduction was equally useful. On the following, we would like to address the most important points you raised in your review letter.

You have pointed out how the choice of the subsurface layer should ideally not be fixed to a closed depth interval for the whole latitudinal extension of the Atlantic and different basin depths. In our work, the 100-200 m depth range is used for the whole Atlantic basin. The choice of this range was not arbitrary though. Several ranges were tried, from 50-150 to 150-250 m, but the 100-200 m range proved to work particularly well in the Atlantic, showing the lowest seasonal variability and having the closest average values to surface late wintertime ones, when water masses typically form in high latitudes. The challenge in selecting an appropriate range relied in the fact that the selected layer had to be as free as possible from the direct influence of surface seasonal and short-term variability, and yet be not too influenced by underlying older waters. Adding to this, the availability of at least four levels of bottle data from the GLODAP dataset for this particular range represented an added value so as to establish the 100-200 m boundaries for the subsurface Atlantic layer. In doing so, the parameterizations benefited from the higher number of data and spatial coverage from the numerous spring and summer cruises in GLODAP. Finally, there is yet another convenience to having a single depth range work out effectively, i.e., it largely simplifies the calculation of the A_T^o and ΔC_{dis} parameterizations (it has no implications for future users, since the parameters of the fits have already been calculated).

Should it occur that, for example, the winter mixed layer depth (WMLD) was only 100 m, then it might be argued that the selected subsurface range would not represent accurately late wintertime formation properties. We know that the WMLD generally increases polewards from the Equator, with known exceptions like in the Southern Ocean. In the Equator, for instance, where the strong upwelling brings up to the surface waters slightly older than 20 years, there are no significant water mass formation processes. Even if there were some, Figs. 3a, 3b and 3e show how in this region (and in the Southern Ocean) the vertical variability of relevant tracers for the parameterizations of A_T^o or ΔC_{dis} in the 100-200 m layer is rather negligible, and the distributions are quite homogeneous, contrary to the ΔC_{dis} computed from subsurface C_T data (Fig. 3f), which actually corresponds quite well to the age distribution in the Southern Ocean (Fig. 3d). In the Equator, the obtained ΔC_{dis} subsurface values close to zero (Fig. 3f) are due to the strong upwelling of very old, C_T enriched waters that would therefore be close to C_T saturation with respect to the present atmospheric pCO_2, while the highest disequilibrium in Southern ocean waters is mostly due to ice cap hindering of air-sea fluxes and the way oxygen disequilibrium interferes with the way ΔC_{dis} is defined and calculated in back-calculation approaches (Lo Monaco et al., 2005).

Although the general C_{ant} concentration fields certainly share some general trends and similarities (Fig. 6), the discrepancies are important enough so as to generate differences of up to 8 Gt C in terms of Atlantic C_{ant} inventory (about 15% of the inventory) (Gerber et al., 2009; Vázquez-Rodríguez et al., 2009), and this is not only with respect to the old ΔC^* method but also with respect to methods more recently proposed like the one from Lo Monaco et al. (2005) or the TTD (Waugh et al., 2006). In addition, it must be said that the differences described in Fig. 6 are the sum of all the improvements included in the φC_T^o method, and they are not due only to the use of a particular set of (modern) Atlantic cruises.

Concerning the comment on method uncertainties, the overall uncertainty in C_{ant} determination for the φC_T^o method is 5.2 μmol kg^{-1}, compared to the 7.9 μmol kg^{-1}of the ΔC^* reported in recent applications of the latter (Lee et al., 2003). This apparently minor reduction in the estimation uncertainty is quite remarkable taking into account that the analytical uncertainties in A_T and $C_T$are around 3 μmol kg^{-1}. The resulting lower uncertainty, compared to older C_{ant} estimation methods, comes from the higher quality of the modern A_T data (CRM calibrated) and the better A_T^o fit here proposed. Most importantly, rather than having lowered the uncertainty in C_{ant} determination, the point is that the consistent ΔC^* biases found in the high Atlantic latitudes (Southern Ocean and Nordic Seas) have been largely corrected, mostly from having used the sub-surface data as the only reference for parameterizations.

You have also suggested extending the discussion of the article to the Western Atlantic basin by applying the method directly there. From the way the parameterizations have been derived (using data from the whole Atlantic extension –Fig. 1) we know the method can be satisfactorily and safely applied throughout the entire Atlantic domain.

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The scope of the manuscript was not to make an exhaustive re-evaluation of Atlantic C_{ant} inventories, rather upgrading the calculation method. The calculation of the Atlantic inventory is just an aftermath of the latter, and we took advantage from the fact that Lee et al. 2003 had provided separated Eastern and Western Atlantic inventories to compare with. However, since this manuscript was submitted for review we have applied the φC_T^o method to a south-western Atlantic section (WOCE A17) to compare the results with other C_{ant} estimation methods (please, refer to the supplementary material provided with this reply letter). The results of this intercomparison work are analogous to the ones obtained here for the Southern Ocean and confer robustness to the results here presented.

Lastly, you have suggested checking the applicability of the φC_T^o method in the Pacific and Indian oceans. This is a work that is currently under progress and has been quite fruitful so far. For the time being we can say that preliminary results are quite satisfactory and very promising. We just have broached the workload of upgrading the back-calculation methods one-step-at-a-time. This is because the magnitude of the problem is too large to be dealt with in one single article. It would have otherwise resulted in an excessively long-winded manuscript that would have been rather difficult to follow, given the *ad hoc* particularities of the three major ocean basins.

Once again, we would like to thank you for your comments which were found very constructive and encouraging to continue with our on-going research and work of implementing the φC_T^o method in the global ocean.

Please also note the Supplement to this comment.

Interactive comment on Biogeosciences Discuss., 6, 4527, 2009.