Biogeosciences Discuss.,
doi:10.5194/bg-2017-27-RC1, 2017

Biogeosciences
Discussions
Open Access

Interactive comment on "Inorganic Carbon and Water Masses in the Irminger Sea since 1991" *by* Friederike Fröb et al.

Anonymous Referee #1

Received and published: 9 March 2017

I applaud the authors for a thorough treatment of a large series of hydrographic and carbon data collected over more than two decades. This is a nice contribution to the understanding of the marine carbon cycle in a changing climate in a region where substantial ventilation of the oceans occurs. In general the manuscript is well organized and easy to read, with one exception; there are too many abbreviations used. I have no problems with using abbreviations for water masses as well as currents, but don't see the point of doing this for regions like the "subpolar North Atlantic" or the "subpolar gyre". It cost very little to spell these out. Additionally I have some minor comments that the authors can consider before this manuscript is suitable for publication in Biogeosciences.

P 2, L 32. Delete last word, "conditions".

Printer-friendly version

Discussion paper

P 5, L20 -. You apply the data from 100-200 m to determine the disequilibrium of DIC as well as preformed TA. Some of the waters within the water column left the surface at very different regions, e.g. the DSOW. How well does the “observed” parameters represent these waters and does this aspect impact the computations? Some words on this would be nice.

P 5, L 30. SWT needs to be identified in the text, not only in Table 2.

P 7, L 7. Ideally all hydrographic parameters should be independent if one wants the best quality of a eOMP analysis, and this would be worth noting. That is a further reason why it is wise to limit the number of water masses in each analysis, as is done.

P 7, L 27. Here the SWT concentrations of the different parameters are referred to, thus relying on the disequilibrium of DIC and preformed TA as mentioned above. It would be nice for the reader to have these concentrations specified for each SWT.

P8, L6. It reads “multiplied by the bottom depth”. However, the computations are performed in a set of density layers, see P 7, L 11 -, so why by bottom depth. Please expand text to address this.

P 9, L 25 and continuation. There are two reasons why DIC-nat can change, either a change in salinity (i.e. mixing or other water mass) or in primary production / remineralization of organic matter. The salinity effect is well discussed in respect to the layer thickness aspect. But the mineralization aspect is not much discussed. It would be nice to see some discussions of this in relation to the convection, e.g. on L 30 same page. The newly ventilated water has higher DIC-anthro but the water that is expelled likely had more remineralized DIC. Similar aspects relate to the text on P 11, paragraph starting L 15. Here it reads that all SWTs except cLSW has a constant DIC-nat, while the cLSW gets accumulation of DIC during ageing, presumably be remineralization. That such an increase is not seen in the other SWTs is that explained by a steady state situation, or? What is then the difference for the cLSW. Is there a difference between the SWTs or is this a result of the uncertainty of the method? Some discussions on this would be valuable, more than now on P 13, L 5.

P 11, L 25. This sentence needs some rewriting. I can guess what is meant, but “The decomposition of the inventory rate of change. . .” is not very informative.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2017-27, 2017.

Printer-friendly version

Discussion paper