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

Anonymous Referee #2

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General comments:

The present study is developed in a very important oceanic region in terms of the carbon system. The results obtained are based on high quality measurements and demonstrate the importance of maintaining the effort of the international community in carrying on high-quality measurements on world-wide repeat lines. In general, the manuscript is well written and the results well presented. Besides, the results add insight into the changes in DIC by analysing the changes in its natural and anthropogenic components. Nevertheless, there are some comments on the manuscript that need to be clarified before the manuscript can be accepted for publication in Biogeosciences.

Major comments:

I do not understand very well the use of the thickness of the water mass layers as a driving mechanism to the changes in the carbon storage rates. As far as I know, the authors are not exactly computing the thickness of a layer. The computation of the layer thickness (Eq. 5, page 8) implies that there are points that "share thickness" between SWTs, isn't it? I understand that the authors are computing how thick in the water column the distribution of the SWTs are. I would like the authors to clarify this in the text. Thus, the dependence/not-dependence of on the storage rates of natural and anthropogenic DIC on the SWT-layer thickness is intrinsic to the computation of the layer thickness and the use of the OMP to estimate the anthropogenic DIC. I do not think that the decomposition into layer thickness driven changes and concentration driven changes (Figures 9 and 10) is really needed. The results of the OMP should not be used as a driving mechanism. The increase/decrease of the layer thickness is the solution of the OMP to the mixing between the water masses. The authors found the layer thickness a driving mechanism because the OMP results are used to establish the amount of anthropogenic carbon in the interior ocean.

Minor comments:

Abstract: Page 1, line 6. It should be mention that the distribution of the main water masses is based on the results from an OMP analysis.

4.1 Anthropogenic CO2 calculation: Page 5. The authors should mention how AT0 (preformed alkalinity?) and ΔCdis are estimated in the interior ocean, i.e., using the OMP (Vazquez-Rodriguez et al., 2009).

5 Results: Page 9, lines 30-32. I am not sure about the comparison between the increase in the Cant inventory from 2012 to 2015, which I also think could be real, and the peak in 2005. Between 2012 and 2015 there are only two cruises that were measured in summer and spring, respectively, when the active convection is not as deep as in winter. Nevertheless, the peak in 2005 pops up in the group of cruises of 2004, 2005 and 2006 and the cruise of 2005 was measured in October, closer to winter

and more likely to have some episode of active convection (even though a general situation was a neutral NAO state). Could the peak of 2005 be due to interannual variability?

5.3 DIC storage rate decomposition: Pages 11-12. See major comments.

6 Discussion: Page 13, line 21. pm should be $\pm$.

6 Discussion: Page 14, lines 11-27. These two paragraphs do not need Figure 10 for the discussion of the results. Graphs similar to Figure 6 would be better. It could also be interesting and complete more the discussion to relate the changes in oxygen, AOU to the changes in natural DIC. Some hints of it had been said in 5.2 section, page 11, lines 22-23 but not enough.

7 Conclusions: Page 15, lines 5-8. See major comments.

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