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Biogeosciences Discussions

Interactive comment on "Temporal variability of the anthropogenic CO_2storage in the Irminger Sea" *by* F. F. Pérez et al.

Anonymous Referee #3

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Review of “Temporal variability of the anthropogenic CO2 storage in the Irminger Sea” by F.F. Perez, M. Vázquez-Rodríguez, E. Louarn, X.A. Pádin, H. Mercier, and A.F. Rios.

The paper by Perez et al. uses a set of high quality data to decipher the inventory of anthropogenic carbon dioxide (Cant) in the Irminger Sea. This area of the North Atlantic is characterized by high variability, but also by relatively high concentrations of Cant, and is therefore interesting in a global carbon budget perspective. The paper is clearly written and concise, and the authors are presenting the data in a clear way. The conclusions are well funded by the data and the conclusions are of interest for the field, and the paper is well worth publishing in Biogeosciences. However, there are a few points that should be addressed before publication.

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General comments: Page 1589, introduction: It is worth to point out that although the study by Sarmiento and Le Quéré (Science, 2001) found profound decrease of Cant uptake in a reduced (shut-down scenario) of the thermohaline circulation (THC) from a GCM, they noted that this might be offset by the downward flux of organic carbon. Now we know that the “new production” is significantly larger than the Cant flux, i.e. ˜ 11 PgC/y (Duce et al., 2008, Science, and references therein) for the new productions vs. the 2.2 PgC/y of Cant uptake. It is thus very likely that the transfer of carbon from the atmosphere to the ocean would increase significantly if the THC did shut down. Although this might not yet well quantified, I feel that the statement by Perez et al. should reflect this possibility.

Page 1591, Dataset and method: It is difficult to judge the validity of the method used to estimate the Cant, since the authors refer only to a manuscript in review (Vázquez-Rodrígez et al.), the authors should provide a more detailed description of the method.

Page 1591, Methods: The few lines on uncertainties leave the reader with some questions on how this was done (presumably this information is available in the ms. in press). Furthermore, the errorbars in figure 3 (and table 2) are much smaller than +/- 5.2 umol/kg. If the authors used a different method to calculate those uncertainties, then this should be described. I guess that the error-bars only refer to the uncertainties in the averaging procedure, which only reflect the variability of the water-column, not any real biasing errors due to the Cant inference method. The uncertainties should reflect the total uncertainty in the accuracy.

Page 1594, Results: The authors claim (without showing the figure with the relationship that they are referring to) that the AOU can be used with some confidence as a proxy for Cant, at least in the LSW, since there is a correlation between these two quantities. However, it could also be a direct result of the method used to calculate the Cant. It is difficult to judge such a causal relationship from the sparse description of the method that the authors provide. It might be a valid statement, but I would like to see some discussion on the impact on how the methodology to calculate Cant relates to the AOU

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/ Cant correlation. It would also be nice to show the figure of the AOU / Cant correlation.

The conclusions on the way the anthropogenic carbon inventory in the Irminger Sea has increased in a step-like fashion are very interesting, as are the speculations over the causes of the non-linear increase. It would also be interesting to see a number of the inventory of Cant in the whole Irminger Sea, i.e. an extrapolation of the data to cover the whole basin (admittedly, this would be a somewhat uncertain number, but not worse than any other basin wide extrapolation of Cant inventories). Although this is a regional study, it would be nice to see a little more discussion on the possible implications for the North Atlantic Cant sink based on the results from the Irminger Basin sections.

Specific comments: Figure 2: This figure comes out very small in the pdf -format, and is difficult to read.

Page 1593: The reference to Olsson 2001 could be complimented with these, more easily accessible, publications; [Olsson, et al., 2005; Tanhua, et al., 2008].

Page 1593: The increase of Cant in the DSOW and the NEADW does not necessarily imply more vigorous mixing south of the Greenland-Scotland sill, but might also reflect changes in the source waters (either as different source water composition, or that the increase in Cant is also reflected in the source waters themselves).

Table 2: There is a wealth of information in this table. It would be nice to see a figure similar to Figure 3, but for AOU (or even theta and salinity for that matter) in addition to the table. Such a figure might be easier to grasp than the table.

References: Olsson, K. A., E. Jeansson, T. Tanhua, and J.-C. Gascard (2005), The East Greenland Current studied with CFCs and released sulphur hexafluoride, J. Mar. Systems, 55, 77-95. Tanhua, T., K. A. Olsson, and E. Jeansson (2008), Tracer evidence of the origin and variability of Denmark Strait Overflow Water, in Arctic-Subarctic Ocean Fluxes: Defining the role of the Nordic Seas in Climate, edited by B. Dickson, et al., pp.

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