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Discussions

Interactive comment on "Trends of anthropogenic CO_2 storage in North Atlantic water masses" *by* F. F. Pérez et al.

Anonymous Referee #1

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The aim of the paper is to evaluate the rate of anthropogenic carbon increase in the main North Atlantic water masses. The authors use quality controlled data from 1981-2006 and a method of estimating anthropogenic carbon, the φCT° method, developed previously by the authors. Their main findings include a decrease in storage rates of anthropogenic carbon in the study region at the same time as a switch occurs in the NAO phase, from high to low. The decrease in storage rate is mainly attributed to a weakening of the convective activity in the region in the low NAO phase.

As the North Atlantic is considered a very important sink of anthropogenic carbon and studies have pointed to a decrease in the surface water uptake capacity in the region, an investigation such as this is certainly of interest. I found the paper interesting and suitable for publication after some issues have been dealt with.

General comments:

One of the main conclusions of the paper is the difference in storage rates of anthropogenic carbon in different NAO modes. Therefore I would like to have some more information regarding NAO in the paper, e.g. it would be helpful with either a small table or a plot of the NAO-evolution over the time-period discussed and what NAO definition is used (I'm assuming the winter-index?). Even though the MOC is mentioned in the introduction, there is no discussion of the MOC in connection with the NAO until just before the conclusion, I think this information would be useful in the introduction. At the moment it is a bit confusing in reading the paper when the high/low shift occurs. In the abstract the high NAO phase is stated as 1991-1997 and the low phase 1997-2006 (why is 1997 included in both?). In the text on line 23 on page 167 it states that "...ended abruptly in 1996 with the shift from high to a low NAO phase". Also in Table 3 it gets a bit confusing regarding the NAO phase in the Iceland basin where the high phase is from 1991-1998 and the low phase is from 1997 to 2006. What is the reason for this? I understand that there is not cruise data available for every year, but at the moment there is no clear reasoning for the different sets of years or the overlap. It would also be helpful if there was a discussion on how fast changes in NAO would reasonably be seen in the different waters defined and in the different basins.

I will not go into detail when discussing the anthropogenic carbon estimation method used, but I am wondering if the cited method description paper will be published. After reading the reviews, the main concern seemed to be not that the method did not work, but that it was much like already existing methods and got essentially the same results. Therefore it should not be a problem using the method in this context and I will not evaluate the method in itself. Just out of interest, how is Ceq(fCO2=280) estimated in this method (it would be the CπTeq term)? In Gruber et al., they used a linearization of temperature, salinity and alkalinity.

When calculating the inventory, much emphasis is put on calculating the layer for each water mass. I think the authors have been very detailed and thorough in these calcula-

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tions, but could some of this could perhaps be moved to an appendix or supplementary material? As it is now it takes up a quite a large part of a paper that is not specifically aimed at improving inventory calculations per se. Initially I wondered why climatological data was used for calculating layer thickness along the observed cruise track, but it was later explained that the ENA basin was scarcely sampled and the climatological data was used. If all basins had been densely sampled would the climatological data still have been used? The properties listed in table 2 were vertically and horizontally integrated within each layer, but if I understood it correctly this was not done for the layer thickness itself so I wondered why not?

Regarding the last sentence in the conclusion, shouldn't the decreasing uptake rates in the surface waters have an impact on the storage rates in addition to the impact of the decreasing ventilation and renewal of water masses? Apart from Corbiere et al and Schuster and Watson, a study that also should be mentioned is Omar and Olsen (2006): Omar, A. M., and A. Olsen (2006), Reconstructing the time history of the air-sea CO2 disequilibrium and its rate of change in the eastern subpolar North Atlantic, 1972–1989, Geophys. Res. Lett., 33, L04602, doi:10.1029/2005GL025425.

The term storage rate is used for rates expressed in both μmol kg-1 yr-1 (e.g. row 18, page 181 and row 16 in the abstract) and Gt yr-1 (e.g. row 11, page 184) in the text. This is a bit confusing. In table 4 storage rate is reported in kmol s-1 and Gt yr-1 (Gt C yr-1), why in two different units since kmol s-1 isn't mentioned in the text? In the abstract, the term storage capacity seems to be used as meaning the same thing as storage rate. I am not sure that I think of storage rate and storage capacity as the same thing so a clarification is needed.

Specific comments:

Abstract: The rates mentioned in rows 14 and 16, where do they come from? I found that the storage capacity/rate of 1.13 in the abstract later in the text was referenced to table 2 so I assume it is calculated from the values there as the others likely are, but

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they are not mentioned in the results and discussion. If they are important enough to be mentioned in the abstract, shouldn't they be in the discussion? I'm confused by the last sentence, isn't the detrimental renewal of main water masses due to the changes in the ventilation?

Methodology : Row 22 on page 171: I thought the method gave similar inventories in this area? Row 10 on page 172: Repeated words Row 22 on page 173: Is Fb,l,c explained earlier? Eq 6 on page 175: What happened to the density in this equation?

Results and discussion : Rows 15-20 on page 178: I have a bit of a problem seeing the clear increases in salinity for the Iceland basin waters in the averages in table 2, also the temperature minimum seem to be in 1997. Row 11 on page 179: Why wouldn't the 15-20 umol isopleth deepen over time? Row 19 on page 179: AR7E is named AR07E in table 1. Row 21 on page 180: This should be Iceland basin and not Irminger basin. Row 6 on page 185: Is Jutterstrom et al the correct reference? Did they calculate storage rates?

Tables and figures: Table 2: I would suggest adding a column naming the cruise. It would be easier since often the cruise names are used in the text and when you go and check the table, there are only years. In table 2c for the uNADW there is a missing tab in the WOA05-columns. Table 3: Just a minor detail, but with a R2 of 0.02 is really AOU significant? Is R2 the adjusted R2 which takes into consideration the number of predictive parameters or the regular R2? Table 4: Just a minor detail, but the numbers of the table are rounded differently than in figure 3. Figure 2: I can understand that it might be a bit messy, but I still think it is a good idea to have the separate basins as in figure 1b. Also, when presenting sections I like to see the bottle-depths for the data used. Figure 4: Is there any specific reason for fitting the temporal evolution of layer thickness with polynomials?

References: Häkkinen et al in the text, but is Häkkinen and Rhines in the ref. list In Yashayaev et al., 2008, Penny Holliday, N. should be Holliday, N. P. Is it Pierot et al.,

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2009 should be Pierrot et al., 2010? Is it Azetsu-Scott or Azetsu? Difference in text and reference list Schuster et al in the text, but is Schuster and Watson in the ref. list

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