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

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Interactive comment on "Trends of anthropogenic CO2 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 ÏŢCT° 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?).

REPLY: The definition of the Hurrell NAO winter index is now given in the introduction, and a link is also provided (www.cgd.ucar.edu/cas/jhurrell/indices.html) where readers can check the tables of NAO indexes relevant to our study that, as we state, correspond to a high phase from 1981-1996 and low phase from 1997-2006. We provide here a graph and table of such indexes, but decided not to include it in the paper because this data is publicly available at the above-cited website. The modified paragraph of the manuscript is given below.

Year NAO Hurrell 1980 0.56 1981 2.05 1982 0.8 1983 3.42 1984 1.6 1985 -0.63 1986 0.5 1987 -0.75 1988 0.72 1989 5.08 1990 3.96 1991 1.03 1992 3.28 1993 2.67 1994 3.03 1995 3.96 1996 -3.78 1997 -0.17 1998 0.72 1999 1.7 2000 2.8 2001 -1.89 2002 0.76 2003 0.2 2004 -0.07 2005 0.12 2006 -1.09

"The water column stratification and wind forcing intensity are determinant factors in the efficiency of convective processes (Dickson et al., 1996; Curry et al., 1998; Lazier et al., 2002). Convection activity in the Labrador Sea is related to the persistence and phase of the North Atlantic Oscillation (NAO). The Hurrell NAO winter index is computed as the difference of surface atmospheric pressure between Iceland and Azores. Historical records of the Hurrell NAO winter index values are available at www.cgd.ucar.edu/cas/jhurrell/indices.html. In the early 90s (1989-1995) the 5-year mean $\pm$ standard deviation of this index was 3.3 ± 0.8 indicating a high phase of the NAO. A low NAO phase period followed during the years 1996-2006, when the index value dropped to -0.1 ± 0.6. A positive NAO phase causes stronger winds and heat loss in the Labrador Sea, fostering convection. During the early 1990's, the highly positive phase of the NAO coincided with an impressive and exceptional convection activity down to more than 2000 m (Dickson et al., 1996; Lazier et al., 2002; Yashayaev et al., 2008). The enhanced convection provoked the formation of the thickest layer of classical LSW (cLSW) observed in the past 60 years (Curry et al., 1998). However, this high LSW formation period that started in 1988 (Sy et al., 1997) ended abruptly in 1996 with the shift from high to a low NAO phase. Nonetheless, weaker convection events (to less than 1000 m depth on average) continued to take place in the central Labrador Sea region and formed the less dense upper LSW (uLSW)."

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.

REPLY: A brief introduction to this NAO-MOC link, as suggested by models (Böning et al., 2006), is now also given in the introduction. This is what's been added in the first paragraph of the introduction:

"The observed weakening of the North Atlantic subpolar gyre (NASPG) during the 1990s seems to have been NAO-driven, i.e., caused by the changes in wind stress and heat flux as part of the decadal variability of the gyre transport (Häkkinen and

Rhines, 2004). Notably, such changes in the subpolar gyre reverberate in the strength of the MOC in the subtropical North Atlantic (Böning et al., 2006)."

Böning, C. W., M. Scheinert, J. Dengg, A. Biastoch, and A. Funk (2006), Decadal variability of subpolar gyre transport and its reverberation in the North Atlantic overturning, Geophys. Res. Lett., 33, L21S01, doi:10.1029/2006GL026906.

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?).

REPLY: You are right. We have corrected this in the text for clarity and consistency in every occurrence: years 1981-1995 correspond to the high NAO phase and 1996-2006 to low NAO phase. However, it must be noticed that since we have no cruise data available for 1996 we had to use data from cruises conducted in 1997 (closest year) to make calculations of Cant storage rate calculations for high and low NAO phases (more on next answer). We will also make more frequent references to figure 3 that, given the data distribution, supports those choices.

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 4 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?

REPLY: Same reason as the one given above, but in the case of the Iceland basin one extra point is taken because, as it can be seen from fig. 3b, if we stick to the 1996 year then we would have only three points (1981, 1991 and 1993) to calculate a storage rate, and that is rather short to calculate a decent linear regression. Hence, in order to reduce uncertainties in Cant storage calculations we need to include as much cruise data as possible. Therefore we decided to "extend" the high NAO year to 1998 for the sake of more robust calculations and, also, it gives some overlapping between the two contrasting NAO periods. This was not necessary in the low-NAO phase case, for which the storage rate calculations were made from 1997 onwards. A short note clarifying this issue has been added to the caption of new table 3:

"The Cant storage rates ±std. err. of the estimate for different NAO phases in the considered NASPG basins. The high NAO phase extends from 1989 to 1995 and the low NAO phase from 1996 to 2006. No cruise data for either 1989 or 1995 was available; therefore data from the closest available years (1990/1991 and 1997) was used for storage rate calculations for the high NAO phase. In the Iceland basin the high NAO interval was further extended to 1998 in order to reduce uncertainties in Cant storage rate calculations, given the shortage of data.".

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.

REPLY: We hope to have cleared this up in the current version and with the answers given above. The data from 1997 was normally used in both high and low NAO phase storage rate calculations to increase slightly the number of data points considered in the regressions.

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.

REPLY: Some discussion on this issue has now been added after the first paragraph of the "Results and discussion" section:

"The speed with which the influence of the NAO is transmitted into the properties of the various water masses varies. These "reaction times" can generally be established in less than 3 years for the water masses in the Irminger basin and about 5 years for upper and intermediate waters in the Iceland basin. According to the latest estimates, the salinity minimum of cLSW is transmitted to the Irminger Sea in 2-years time during high-NAO periods and 2.5 years during weak convection periods (Pickart et al., 2003; Yashayaev et al., 2007; Thierry et al., 2008). Also, Yashayaev et al. (2008) suggest

that it takes about 5 years for LSW to reach the Iceland basin. According to Johnson and Gruber (2007) and Thierry et al. (2008) the NAO leads by 2-3 years the water mass properties of SPMW, mainly due to NAO wind-driven changes in circulation and advection. Regarding the SAIW, Sarafanov et al. (2008) have pointed out that the recent increase in salinity of the intermediate waters is the result of the contraction of the subpolar gyre following the 1996 NAO drop that causes the northward advance of the subtropical water masses. The strong correlations between the salinity values of intermediate waters and the 5-year running mean of the NAO index suggest that there exists a 2-year lag between the two phenomena (Sarafanov et al., 2008)."

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.

REPLY: As it stands right now, the only published version of the method can be found in the paper from Vázquez-Rodríguez et al., 2009 published in Biogeosciences (please, see full reference in the manuscript). The corresponding handling editor of Biogeosciences decided to leave the method description in "Biogeosciences discussions" because he considered that the method had already been applied in Pérez et al., 2008 and described well-enough in Vázquez-Rodríguez et al., 2009 (although no method equations were provided in either of these two published papers). Therefore, the method description remains for now in "Biogeosciences discussions" (http://www.biogeosciences-discuss.net/6/4527/2009/bgd-6-4527-2009-discussion.html.).

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 CT eq term)? In Gruber et al., they used a linearization of temperature, salinity and alkalinity.

REPLY: To calculate Ceq(fCO2=278) we used the thermodynamic relationships of the carbon system (many software bundles and computer routines can do this, like the CO2SYS or the SEACARB. We used a macro that we personally developed and implemented using visual basic), i.e., we calculated CT as a function of θ, S, pCO2, AT and nutrients using the thermodynamic constants proposed by Mehrbach refitted by Dickson & Millero (1987).

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 calculations, but could some of this 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.

REPLY: Thank you for this comment. The methodology section has been revised and made more synthetic now in the revised version of the manuscript. Also, the details of the layer thickness calculations are now better described in the newly added Appendix I.

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?

REPLY: Yes. The climatological data serves as a necessary reference to calculate the factor by which observed layer thickness compares to the climatological one in the case of the Irminger and Iceland basins, which have a fairly good sampling coverage (Eqs. A1, A2 and A3 in Appendix I). Such factors are further used to extrapolate observed layer thickness under cruise "c" to basin scale. In the case of the ENA, which has by far the largest extension of all three basins, the WOA05 data is given another use in order to make up for the spatial coverage and scarcity of data in Cant calculation itself (now explained also on Appendix I).

Accordingly, if we had better data coverage in the ENA then the recommended practice would have been to sub-divide the basin in different provinces/regions (perhaps no more than two, like northern and southern) of more homogeneous biogeochemical characteristics. This would largely help to obtain better and more representative parameterizations. Also, if we had more observations in the ENA basin it could have also been feasible to aim for specific high-low NAO estimates of the Cant storage rates, and not a single rate for the whole 1981-2006 period referred to an average climatological reference for the entire ENA, as we had to do.

We hope that the revised version of Section 3 and the added Apendix I will clarify your doubt to future readers.

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?

REPLY: All properties in Table 2 (including the observed layer thickness) were indeed vertically and horizontally integrated. We went through the text trying to spot the sentence that gave you the impression this was not done for the layer thickness and we concluded it was maybe because of what's said in the table's caption: The STD (and not the standard error of the mean, as for the rest of properties) is given for the variable "Thickness". We have made this clear in the caption of Table 2:

"Table 2 Temporal evolution between 1981 and 2006 of the average ($\pm$ standard error of the estimate, i.e., $\pm$, where N= number of data) salinity (S), potential temperature (θ), apparent oxygen utilization (AOU), silicate (Si(OH)4) and Cant for the water masses considered in the Irminger basin (2a), the Iceland basin (2b) and the ENA basin (2c). In the case of Thickness (Th), the standard deviation is given instead of the standard error of the estimate. The acronyms "l" and "c" stand for "layer" and "cruise", respectively. The "Obs." acronym in the second "Thickness" column stands for "observed" from cruise measurements. The WOA05 climatological values are given as references. The

WOA05 value in the “Cruise-Year” column represents the climatological basin conditions calculated from WOA05 data (details given in Appendix I). Whenever a year is listed more than once it is because more than one cruise was conducted in this particular basin, for example, the 1997 cruises FOUREX and MET97 in the Irminger basin (Table 2a). All values here listed were obtained by vertically and horizontally integrating each property within each layer.”

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?

REPLY: Yes, they have an impact indeed and are coherent with the reduction of Cant storage rates here found. The concentration of Cant in the Irminger Sea changes over time due to the atmospheric xCO2 increase that, once it enters into the water column, is later transported into the ocean interior thanks to the deep-convection processes. Thus, the strength of such convection events in the Irminger is also a determining factor for the Cant that ultimately goes into these particular waters. Pérez et al. (2008) showed that in the Irminger basin the % of the Cant saturation concentration varies (it is actually inversely correlated) with AOU, which is a proxy for ventilation (NB: using %Cant sat. “removes” the contribution of the temporal atm. xCO2 increase from this relationship, since the % saturation concentration is always relative to the corresponding atmospheric xCO2, i.e., the relationship %Cant sat-AOU establishes the direct dependence between Cant content and convection). Since AOU is controlled by the natural cycles of ventilation plus the remineralization of organic matter, and it is not affected by the anthropogenic effect (it assumes 100% saturation of oxygen at the air-sea interface), this means that in the subpolar gyre the natural cycles and the entrainment of the anthropogenic signal are directly linked. This relationship is driven by the variability of winter convection. The authors of the present manuscript therefore maintain that the changes observed in the uptake of natural and anthropogenic CO2 in this region are indeed linked and that, consequently, the observed decrease in air-sea CO2 exchange

over the last decade (Omar and Olsen, 2006; Corbiere et al., 2007; Schuster et al., 2007) runs parallel to the weakening of Cant storage in the NASPG. As discussed in previous answers, this weakening stems from the NAO-driven changes in stratification and convection.

The sentence has now been modified and reads as follows:

“The changes in Cant storage rates here obtained are consistent with the results in Omar and Olsen (2006), Corbière et al. (2007) and Schuster and Watson (2007), who found analogous decreasing rates in the air-sea CO2 exchanges from surface fCO2 measurements in the North Atlantic that, overall, contribute to the decrease of Cant storage rates in the NASPG. Such air-sea CO2 exchange results can be legitimately compared to the ones here obtained for Cant storage rates since, according to Pérez et al. (2008), the cycles and uptake of natural and anthropogenic CO2 in the NASPG are linked. Consequently, the observed decrease in air-sea CO2 exchange over the last decade (Omar and Olsen, 2006; Corbière et al., 2007; Schuster et al., 2007) must have occurred simultaneously (and most probably linked) to the weakening of Cant storage in the NASPG that, as shown here, stems from NAO-driven changes of stratification and convection intensity.”.

Pérez, F.F., Vázquez-Rodríguez, M., Louarn, E., Padin, X.A., Mercier, H., Ríos, A.F., Temporal variability of the anthropogenic CO2 storage in the Irminger Sea, Biogeosciences, 5, 1669–1679, 2008.

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 airsea CO2 disequilibrium and its rate of change in the eastern subpolar North Atlantic, 1972–1989, Geophys. Res. Lett., 33, L04602, doi:10.1029/2005GL025425.

REPLY: The reference has been added (see previous answer).

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?

REPLY: We have revised the text for consistency so that, for instance, when μmol kg-1 yr-1 is used, we talk about "rate of change of Cant concentration", Gt C yr-1 refers to "storage rates" and mol C m-2 yr-1 stands for "Cant specific inventory rates". In the case of table 4, information was redundant and we have now deleted the column where the storage rate was given in kmolÂůs-1.

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.

REPLY: Thank you for noticing. This part of the manuscript has also been reviewed for consistency according to the definitions given above:

"A high-quality inorganic carbon system database spanning over three decades (1981-2006) and comprising 13 cruises has allowed applying the ÏŢCT° method and coming up with estimates of the anthropogenic CO2 (Cant) stored in the main water masses of the North Atlantic. In the studied region, strong convective processes convey surface properties, like Cant, into deeper ocean layers and confer this region an added oceanographic interest from the point of view of air-sea CO2 exchanges. Commonly, a tendency for decreasing Cant storage rates towards the deep layers has been observed. In the Iberian Basin, the North Atlantic Deep Water has low Cant concentrations and negligible storage rates, while the North Atlantic Central Water in the upper layers shows the largest Cant values and largest annual increase of its average concentration (1.13±0.14 μmol kg-1 yr-1). This unmatched rate of change in the Cant concentration of the warm upper limb of the Meridional Overturning Circulation decreases towards the Irminger basin (0.68±0.06 μmol kg-1 yr-1) due to the lowering of

the buffering capacity. The mid and deep waters in the Irminger Sea show rather similar Cant concentration rates of increase (between 0.33 and 0.45 μmol kg-1 yr-1), whereas in the Iceland basin these layers seem to have been less affected by Cant. Overall, the Cant storage rates in the North Atlantic subpolar gyre during the first half of the 1990s, when a high North Atlantic Oscillation (NAO) phase was dominant, are âĹij48% higher than during the 1997-2006 low NAO phase that followed. This result suggests that a net decrease in the strength of the North Atlantic sink of atmospheric CO2 has taken place during the present decade. The changes in deep-water ventilation are the main driving processes causing this weakening of the North Atlantic CO2 sink."

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 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?

REPLY: You are right. We have now included these slopes in the appropriate paragraphs of the discussion section (Section 4). Indeed, all these rates are calculated from the data in table 2.

I'm confused by the last sentence, isn't the detrimental renewal of main water masses due to the changes in the ventilation?

REPLY: You are right. This sentence was redundant and has now been corrected:

"The changes in deep-water ventilation are the main driving processes causing this weakening of the North Atlantic CO2 sink.".

Methodology :

Row 22 on page 171: I thought the method gave similar inventories in this area?

REPLY: According to the Vázquez-Rodríguez et al. (2009) paper comparing various Cant methodologies in the Atlantic, the areas with the largest discrepancies are the Southern Ocean and the NASPG. In our current study region the methods that yield more similar inventories are the ÏŢCT$^{\circ}$, TTD and TrOCA also. The ΔC* (as in Lee et al., 2003) tends to underestimate inventories in this region compared to most other methods. The ÏŢCT$^{\circ}$ approach can be said to represent roughly the average of all five Cant methodologies that were compared in the above-cited study, over the whole latitudinal range of the Atlantic.

The following is now stated in the manuscript (section 3.1):

"...The work from Vázquez-Rodríguez et al. (2009b) compared five independent estimation methodologies of Cant in the Atlantic Ocean. According to this study, the ÏŢCT$^{\circ}$ approach consistently yielded the closest values to the average of all five Cant methodologies over the whole latitudinal range of the Atlantic. Appendix II discusses further the choice of the ÏŢCT$^{\circ}$ method with respect to other methodologies, and a comparison of results is made with the TrOCA approach (Touratier et al., 2007)."

Row 10 on page 172: Repeated words

REPLY: Corrected.

Row 22 on page 173: Is Fb,l,c explained earlier? Eq 6 on page 175: What happened to the density in this equation?

REPLY: Yes, Fb,l,c was explained earlier (page 173, line 1, where equation 2 was first introduced), but was not named as such. It is now explained at this point of first occurrence in Appendix I. Also, density is now included in old Eq. 6, which is Eq. (1) in the revised version.

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.

REPLY: The "clear" increase in salinity refers to the core of 34.9 of the LSW body, which extension diminishes as the surrounding waters increase their salinity as a result of the convection weakening in the Irminger after 1996. Regarding the temperature minimum, you are right, it is not early but mid 90s. This descriptive part of Section 4 has now been removed in order to focus more on results dealing with Cant. The distribution of the rest of tracers used is only mentioned to clarify/account for observed Cant patterns and tendencies.

Row 11 on page 179: Why wouldn't the 15-20 umol isopleth deepen over time?

REPLY: This sentence has been made clearer now: "This isopleth deepens over time (Fig. 2) as an indicator of the larger entrainment of Cant towards the ocean interior".

Row 19 on page 179: AR7E is named AR07E in table 1.

REPLY: Thanks for spotting this typo. Table 1 has been corrected to "AR7E".

Row 21 on page 180: This should be Iceland basin and not Irminger basin.

REPLY: Right. It's been corrected.

Row 6 on page 185: Is Jutterstrom et al the correct reference? Did they calculate storage rates?

REPLY: They did not calculate rates, but did provide an inventory estimate of 1.2 Gt C for the Nordic Seas as of Spring 2002. In order to calculate the storage rate from here, we followed a practice that was used by Tanhua et al. (2007) (see their page 3038): They calculated storage rates as ΔCant / Δt from two cruises (TTO-NAS 1981 and M60/5 in 2004). In order to calculate inventories (I) from ΔCant data they propose the following expression: I=3.0ÂůΔCant. Therefore, since the storage rate (S.R.) is defined as S.R.=ΔCant / Δt = I/(ΔtÂů3.0). Also, since they referred their calculations to a time span from 1981-2004 and Jutterstrom et al. did it for 2002, we calculated a correction factor (1.037) to rescale the inventory in the latter publication, considering what the Cant saturation concentration was in 2002 and 2004, respectively, from the different atmospheric pCO2 levels in each moment. Thus, we obtained the reported value for the storage rate of the Nordic Seas of 0.018 Gt CÂůyr-1 = (1.2 Gt C/ 3.0Âů23 yr)Âů1.037.

A short footnote has been added in the manuscript summarizing this explanation for readers.

“In order to calculate the storage rate from the inventory estimate of 1.2 Gt C provided by Jutterström et al. (2008) for the Nordic Seas, the expression proposed in in Tanhua et al. (2007) was applied, namely: Inventory=3.0ÂůΔCant. Hence, Storage rate=ΔCant / Δt = I/(ΔtÂů3.0). The calculations in Tanhua et al. (2007) were referenced to 1981-2004, while the ones in Jutterström et al. (2008) referred to 2002. A correction factor of 1.037 was calculated from the corresponding Cant saturation concentrations in 2002 and 2004. Thus, the value of 0.018 Gt CÂůyr-1 for the storage rate of the Nordic Seas here reported was calculated as [I/(ΔtÂů3.0)]Âů1.037= [1.2 Gt C/ (3.0Âů23 yr)]Âů1.037”

Tanhua, T., A. Körtzinger, K. Friis, D.W. Waugh and D.W.R. Wallace, An estimate of anthropogenic CO2 inventory from decadal changes in oceanic carbon content, PNAS, vol. 104 no. 9, 3037–3042, 2007.

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.

REPLY: In order not to enlarge the table too much with yet an additional column, we

have included the name and year of the cruise separated with a dash in the same column.

In table 2c for the uNADW there is a missing tab in the WOA05-columns.

REPLY: Thank you for noticing, but I cannot see where this missing tab is according to your indications on my MS Word version of the manuscript.

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?

REPLY: Thank you for noticing this. We apologize for this typo. The right R2 in this case is 0.41, which is indeed significant. In any case, the reported R2 are regular ones and not the adjusted ones. However, as you can see in the table below, these two statistics are almost the same in our case. Notice that this table is now Table A1 (Appendix I) in the revised manuscript.

Eastern North Atlantic (ENA) Basin Layer R2 adj. R2 a1 (AOU ; kg μmol-1) a2 (θ ; °C-1) a3 (S) a4 (xCO2 ; ppm-1) NACW 0.97 0.97 n.s. n.s. n.s. 0.47 ± 0.03 MW 0.96 0.95 -0.31 ± 0.05 n.s. 17± 2 0.18 ± 0.02 LSW 0.90 0.86 -0.38 ± 0.20 -7.1±3.3 n.s. 0.19 ± 0.02 uNADW 0.71 0.59 n.s. -61± 18 (0.7± 0.2)Âů103 0.06 ± 0.03 lNADW 0.41 0.36 -0.54 ± 0.21 n.s. n.s. n.s.

Table 4: Just a minor detail, but the numbers of the table are rounded differently than in figure 3.

REPLY: Thanks for noticing. We have modified the number of decimal places in the table for consistency and in such a way that others can reproduce the result we obtained for the Ovide Box of 1.18±0.12 mol C m-2 yr-1.

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.

REPLY: Done. We have included the basin separations (and names) and the bottle-depths as white dots in all subplots of Fig. 2.

Figure 4: Is there any specific reason for fitting the temporal evolution of layer thickness with polynomials?

REPLY: Not really. It was the simplest way to adjust fairly accurately a set of data that showed a large dispersion and an apparently cyclic pattern. Also, this solution yielded a very similar pattern in the variability of the LSW thickness to the one obtained by Kieke et al. 2006 in their Fig. 12 (included below).

Kieke, D., et al., Changes in the CFC inventories and formation rates of Upper Labrador Sea Water, 1997–2001, J. Phys. Oceanogr., 36, 64–86, 2006.

References:

Häkkinen et al in the text, but is Häkkinen and Rhines in the ref. list. Corrected.

In Yashayaev et al., 2008, Penny Holliday, N. should be Holliday, N. P. Corrected.

Is it Pierot et al., 2009 should be Pierrot et al., 2010? Corrected. It is Pierrot et al., 2010.

Is it Azetsu-Scott or Azetsu? Corrected. It is Azetsu-Scott.

Difference in text and reference list Schuster et al in the text, but is Schuster and Watson in the ref. list. Corrected.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/7/C758/2010/bgd-7-C758-2010-supplement.pdf

Interactive comment on Biogeosciences Discuss., 7, 165, 2010.