

Interactive comment on “Temporal variability of the anthropogenic CO₂ storage in the Irminger Sea” by F. F. Pérez et al.

F. F. Pérez et al.

Received and published: 17 October 2008

line 12: Please, add Rhein et al. (2007, GRL) as a further reference, since their analysis covered the years 2003-2005, while the analysis of Kieke et al.(2006) ended in 2003.

Done.

line 15: In contrast to this statement, AOU in the cLSW appears to be lower in 2006 compared to 2004.

We do not agree. Please, refer to data in Table 2 (40.5 ± 0.2 and $40.6 \pm 0.2 \mu\text{mol}\cdot\text{kg}^{-1}$ in 2004 and 2006, respectively). All that could be said in any case is that the rate of increase of AOU seems to reach a plateau in 2006.

line 24: 'errors' should be replaced by 'uncertainties'. Please add isopycnals to figure S2000

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2, so the reader can easily infer water mass boundaries.

Done.

lines 26-27: Please, clarify whether averages of layer thickness, salinity and temperature have been derived from bottle data or CTD data. At least for the DSOW observed in 1997 it is striking that the average temperature is quite lower than one can infer from the temperature distribution shown in figure 2.

As discussed previously (and stated now in Fig. 2 caption), only bottle data has been used in this study. This DSOW issue has already been clarified in the general comments section.

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lines 14-15: Please, explain why DSOW is excluded from the correlation analysis.

line 21: It should be noted that strong entrainment close to the Denmark Strait sill can result in direct exchange between the LSW layers and DSOW. I cannot see to what extent this approach is further exploited. If one does actually plot AOU versus %C_{ant_sat}, which is listed in table 2 but not shown by the authors, it turns out that cLSW and DSOW occupy similar ranges of AOU at very different values of %C_{ant_sat}. This seems to be in contrast with the statement on lines 17-18. Please, clarify this section.

To answer these two remarks: This relationship is intended to show that there exists a clear relationship between the ventilation of water masses and C_{ant} content, rather than to be able to calculate C_{ant} solely from AOU data. We do not have a sound theory to explain why DSOW falls out from this linear relationship. Azetsu-Scott et al. 2003 show that the CFC content of DSOW is lower than that of the LSW. Similarly, the C_{ant} content of DSOW is lower than in LSW. In any case, the DSOW represents only 5% of volume of the water column. This fact is now given in the text.

line 26: Which C_{ant} estimates are displayed here? The ones estimated using the

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method of Vazquez-Rodriguez et al. or the ones estimated from AOU - %C_{ant}_sat?

The ones obtained with the ϕ -Ct^o method, as always. This is now made clear in the text. The following line has been deleted to avoid confusion in the reader:

“Knowing the %C_{ant}^{sat} and the atmospheric pCO₂ for the sampling year, C_{ant} might thereby be estimated.”

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lines 3-4: Please, add a reference to table 2 since many of the properties which are discussed in this context are not visualized in figure 3 but are listed in table 2 (DONE). The temporal resolution of the time series is rather coarse, but many more CTD sections are available and could be used to generate annual time series of water mass layer thicknesses. It might be the case that the maximum layer thickness of cLSW occurred already earlier than 1997, but this cannot be resolved with the presented data set.

This point has been already discussed in the general comments section. Even though it is true that there are more CTD sections available than the selected ones (like the proposed Meteor cruises), not all of them have the necessary carbon system parameters to calculate C_{ant}.

lines 5-6: Since the density distribution is not provided for each particular section (Now it is. Please, see Fig. 2), the reader cannot infer to what degree the layer thicknesses have changed over time. It is therefore not easy to deduce from the provided material that cLSW has doubled its thickness throughout 1981-1997 in the Irminger Sea. But it seems that your refer to your table 2. Please, clarify this.

Done.

lines 12-13: What is "a very small portion"?

In this particular case, it is 2-3%. This data was presented in Table 2. This percentage

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and a reference to Table 2 are now included in the text.

line 18: See comment to figure 3.

OK.

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line 8: Please, add references of some of these studies. What patterns do these studies describe?

Done. The following references have now been included: Azetsu-Scott et al. 2003; Kieke, et al. 2007, Rhein, et al. 2007. The patterns were described in the lines immediately after this very sentence.

lines 9-10: Add a reference.

Done (please see previous comment).

lines 17-18: Azetsu-Scott et al. (2003) did not analyze data from the Irminger basin, they focused on the Labrador Sea.

OK. The specific mention to the Irminger basin has been removed

line 19: 2001 should be 2000, since the analysis of Azetsu-Scott et al. (2003) covered the years 1991-2000.

Corrected.

lines 25-26: Again, Azetsu-Scott et al. (2003) focused on the Labrador Sea and not on the Irminger Sea.

Corrected.

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line 13: Also the method applied by Alvarez et al. (2003) is an indirect approach.

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OK. The Álvarez et al. 2003 reference is now given along the Mikaloff-Fletcher et al., 2006

lines 17-18: It is not clear to me, why the MPD should be considered as an index of the convection activity in the Irminger Sea. The estimates vary only by about +/- 200m. What is the measure to distinguish between weak and strong convection? Earlier it was stated that the TTO section was conducted in a period of low convection activity, but the MPD value from 1981 is as high as in 1997. Several studies present indications that the wintery convection in 1997 reached down to 900-1000m, which is quite deep for the Irminger Sea (see Falina et al. (2007), Bacon et al. (2003)) and might be considered as that particular year with best knowledge concerning local 'intense' convection activity. In contrast, the OVIDE period was considered as a period of weaker convection.

You are right. The text has been modified for clarity sake:

"... Assuming a transient steady state (TSS, Keeling and Bolin, 1967) for C_{ant} , the MPD is defined as the quotient between the specific inventory of C_{ant} in the water column and the C_{ant} concentration in the mixed layer (C_{ant}^{ml}). The model results presented in Tanhua et al., (2006) demonstrate that the TSS assumption is indeed valid for C_{ant} in this part of the North Atlantic Ocean. A high MPD normally indicates that large amounts of C_{ant} have penetrated in the water column following strong vertical convection processes (> 1000 m depth) generated in the considered region, and vice versa. Álvarez et al., (2003) calculated an average and constant MPD for the Irminger basin of 1739 ± 381 m by approximating $C_{ant}^{ml} \approx C_{ant}^{sat}$ in the corresponding sampling years."

In addition, earlier in the manuscript the 1000 meter depth horizon is given to the reader as a rough reference to distinguish between weak and strong convection processes in the Irminger Sea.

Regarding your argument comparing the MPDs from TTO and FOUREX, you are right. We do not have a clear explanation for this contradiction. A plausible justification for this could rest on the fact that the TTO is the oldest of all cruises considered, and the

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quality of data is not as good as in more modern sections. This is patent by the higher TTO MPD uncertainties (1835 ± 140) and C_{ant} storage uncertainties (Fig. 4) compared to the rest of sections.

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lines 16-22: Please note that the annual decrease of the uptake as presented by Schuster et al. (2007) refers to a much larger area than the Irminger Sea. Furthermore, the results by Corbiere et al. (2007) were included in their estimate.

OK. We have made this point explicit in the text:

“Our observations in the Irminger Sea can also be compared with other works on the secular variation of sea surface pCO_2 that cover larger areas.”

line 22: Earlier on page 1595 it was stated that the decrease during 1997-2006 was only $-1.5 \text{ mol C/m}^2\text{yr}^{-1}$

Thank you for noticing this typo mistake. Now all rates of decrease have been corrected to $-1.6 \text{ mol C}\cdot\text{m}^{-2}\cdot\text{yr}^{-1}$.

References, pages 1599-1601

All references have been updated. Thank you for noticing it.

The references of Böning et al (2006), Canadell et al. (2007), and Drijfhout et al. (2006) are missing.

The reference of Yashayaev et al. (2008) should be listed at the end of this section.

The reference of Wallace (2001) is incomplete. Editors and publisher should be added.

Tables

Table 1, page 1602: The name of the cruise referred to as 'AR7E' should be '91/1', AR7E is the name of the respective WOCE-line. Column indicating date: for reasons of consistency please

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add missing zeros when indicating months.

Done.

Table 2, page 1603: At least for the DSOW observed in 1997 it is striking that the average temperature is quite lower than one can infer from the temperature distribution shown in figure 2 (Solved. Please, refer to the general comments section for the full reply to this issue). Inventories are only given as percentages. How large is the actual inventory and how it is defined? (The inventory for each year can be read from figure 4. This is also clarified in Table 2 caption. It is defined as the vertical integral of C_{ant} in $\text{mol}\cdot\text{m}^{-2}$. This is identical to the sum of the products of the average thickness layers times the C_{ant} concentrations in the corresponding layer). The standard deviation of the average layer thickness should be included as well for reasons of completeness (The STDs for the layer thicknesses are now included in Table 2). As one can deduce from figure 1b, the layer thickness close to the continental slope or Reykjanes Ridge can differ from the thickness in the interior Irminger Sea. The %Inventory of the sub-surface layer in 1991 is strikingly lower compared to all other years but this not further discussed. How about outcropping effects that were observable in the data from this section?

With respect to the %Inventory of the sub-surface layer in 1991, this is a direct consequence of the low average thickness of the layer in this year (Table 2; Fig. 2) and the high convection activity in those years. This is now made explicit in the first paragraph of the Results section:

"... Since the AR7E cruise was carried out shortly after the winter season, the sub-surface layer thickness is seen to decrease substantially (Table 2)"

Figures

Figure 1: 1a) It cannot clearly be seen whether all sections cover the boundary current area (The symbols of the OVIDE sections have been modified to facilitate the view

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of the full extension of the sections). At least the AR7E cruise does not come very close to the shelf break. The TTO-leg is actually leg 6 with only one station from leg 4 (the easternmost) and not leg 4 as indicated in the figure (Corrected). It looks like the authors only have chosen a selection of profiles. The reason might be that only those profiles provided the necessary CO₂-relevant data (Right. This was the main selection criteria). But there are more S/T profiles available, even CTD profiles (This has been already discussed). In figure 2 which appears later, the authors only present those FOUREX stations from west of 34_W. Those stations which are not used for the analysis can therefore be omitted from figure 1a (Done). Otherwise, the reader might be confused. 1b) As was indicated already earlier, it should be mentioned whether the presented density distribution is from one particular section (which?) (Done. It corresponds to OVIDE 2004 and is now clearly posted in the figure).

Figure 2: The quality of figure 2 is quite low, and details are difficult to examine without magnification to several hundreds of percent. The density distribution should be included. Also ticks indicating the station position are necessary, since the horizontal distribution of the sections is quite different. Though the authors commented on their choice of visualized data in the figure caption, CTD profiles of all sections are available, and it is suggested to use these at least for the salinity and temperature distributions. T/S averages of the particular layers can be estimated much more accurately from the CTD data (Already discussed). It is furthermore suggested to decrease drastically the number of contour labels. Currently, they rather make it difficult to deduce any details. Instead, it is recommended to insert dots which indicate the horizontal and vertical distribution of water samples. It should be mentioned in the figure caption that the western and eastern ends of the sections are not necessarily identical.

Done.

Figure 3: The box indicating the mean penetration depth is hardly readable and should be considerably enlarged or removed from the figure (Done. This information is now given in the text). As is given in the figure caption, the right axis indicates the inventory.

Alvarez et al. (2003) define the storage as the change of the inventory with time, which is probably the same as the 'storage rate' in the present study. This is quite confusing and should be revised (It has been corrected).

Interactive comment on Biogeosciences Discuss., 5, 1587, 2008.

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