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Biogeosciences Discussions

Interactive comment on "Temporal variability of the anthropogenic CO_2 storage in the Irminger Sea" *by* F. F. Pérez et al.

F. F. Pérez et al.

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Dear Reviewer:

In the first place, I am very grateful for your sensible comments and for the time you have committed to make them. They have greatly contributed to improve the paper in terms of clarity and conciseness. It is the desire of all co-authors that you find the reading of the revised version more straightforward and concise than the previous one. The way we have structured the present reply letter is as follows: The original comments and remarks are included in black font type, and answers are in blue.

After reading and working on both general and the detailed comments sections we realized the most important comments were all synthesized in the general comments section (which is a very long paragraph addressing a large variety of issues). While all comments have been considered and answered we thought it was unnecessary giving the same answer to the same issue twice in a single document. Thus, the reply to the detailed comments section sometimes refers to the answers and points made in the general comments section. We hope you will find the revised manuscript and this reply letter satisfactory.

Yours truly

The authors

General remarks

The presented manuscript deals with estimating the anthropogenic carbon (C_ant) in the Irminger Sea from oceanic sections spanning a period of 25 years. The focus is on those water masses that contribute to the North Atlantic Deep Water, and, as e.g. Labrador Sea Water, are affected by changes in remote or local water mass formation. The authors aim at quantifying changes in the C_ant inventories which they address as changes in the oceanic C_ant storage. While facing changes in the global climate, it is highly desirable to have time series which indicate changes in the oceanic uptake and storage of C_ant. These interesting issues are addressed in the manuscript and make it certainly suitable for considering publication in Biogeosciences. However, a revision of the manuscript is definitely necessary. Details are given in the following.

The abstract should be filled up with more results rather than broad and general comments. Relevant key points should be worked out more properly. Some repetitions especially in the 'Discussion' section could be removed (DONE). Essentially, the section about 'Methods' is too short. First of all, I would expect some more details about the source of data, at least those, which have not been measured by the group of authors (DONE). Additional data from 1991 (Meteor cruise M18) as well as 1994 (Meteor cruise M30/3) and 1997 (Meteor cruise M39/4, M39/5) are certainly publicly available either as part of the CARINA data set or at the former WOCE data server, and to my knowledge these cruises include carbon related measurements. Wouldn't it be worth

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to include these data in the analysis since it would yield another data point for 1994 and it would provide additional estimates for 1991 and 1997 which can be used for direct comparison with the presented estimates?

Out of the cruises suggested, only the M39/4 has both C_T and A_T (we need at least two variables from the oceanic carbon system to be able to calculate C_{ant}). We have considered very seriously including the Meteor M39/4 data after your suggestion to add additional estimates to the FOUREX section in 1997. However, given the similarity of the obtained C_{ant} storage rates results for the FOUREX and MET39/4 cruises and the fact that the MET 39/4 cruise has less data points (by half in most cases) than FOUREX (table below), we have decided to stick to the original dataset. This internal exercise has served, however to validate our results with other cruises, which you can check out in the tables and graph below (analogous to Fig. 4 in the revised manuscript). In the case of the Meteor 18 / A01 cruise, the number of data in the water column is inferior compared to the AR7E cruise and also the quality of the carbon system data is not fully satisfactory (the MET 18/A01 cruise has not been considered in the second quality control phase of the CARINA database).

Did the authors only analyze bottle data or did they also look at CTD data? Layer averages of S or Theta can be estimated much more accurately from CTD data. The missing DSOW temperature signal in 1997 as discussed in the text seems to stem from focusing on bottle data. Statements concerning differences in the horizontal and vertical resolution of the particular sections are missing.

We have checked the CTD and bottle data and the obtained profiles are pretty much identical, so using one dataset or the other will not change results significantly with regard to DSOW or any other water mass (see table below).

The fact that the DSOW signal does not seem to weaken in the corresponding θ contour plot in Fig. 2 in 1997 stems from the gridding resolution used for that particular plot, which we agree it was not too appropriate given the values presented in Table 2. The

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contour plots have been redone. We have also modified the corresponding sentences in the manuscript to give a sense of “thinning” rather than “extinction”.

Concerning the information on the horizontal and vertical resolutions, please notice that Fig. 2 has been re-designed attending your suggestions.

The cruise from 1991 termed ’AR7E’ was carried out shortly after the winter season has ended. The 27.68 isopycnal outcropped in the Irminger Sea at this time. The section also did not cover the boundary current region. Any effect resulting from these issues is not discussed. Does this mean, it does not have any effect?

The boundary current region represents a very small portion of the Irminger (roughly 3.5%). Even if our sections had covered this region, the inventory results (both specific and total inventories) for each year would not change significantly. Since the horizontal C_{ant} concentration gradient between the boundary current region and the immediately adjacent areas is very small (as it can be deduced from the AR7E 91/1 plots in Fig. 2, for instance), the effect on the average inventories would be trifling.

The method which was applied to deduce C_ant is presented in a manuscript which is not yet available to the public. It is therefore necessary to outline at least the basic ideas, assumptions, and improvements with respect to established methods. In later sections of the manuscript, further methodological approaches are introduced which might be gathered together in section 2. Concepts like the mean penetration depth (MPD) have been applied similar to the studies of Alvarez et al. (2003). The reader of the present manuscript might not easily understand these concepts without going back to the earlier paper and some statements related to MPD are not really convincing (see Specific Comments).

The main features of the C_{ant} reconstruction method used (φC_T^o) are now summarised in Appendix 1 and in a paper available from the BGD site (http://www.biogeosciences-discuss.net/5/1421/2008/bgd-5-1421-2008.pdf). The paper is currently under revision.

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With respect to the MPD issue, we did not consider this element of the paper as relevant as others so as to refer specifically to it in the Methods section. The definition of MPD is now given immediately after its first occurrence in the text, namely:

". . . 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^{ml}_{ant}*). 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″*

Details should be provided with respect to the calculation of the C_ant inventories. Regarding the fact that authors only have section data along more or less the same line, should this be considered representative for the entire Irminger Sea. Azetsu-Scott et al. (2003) scaled their section-derived inventories with a basin-wide survey. Have similar approaches been undertaken here as well? At least, this needs some discussion.

The main physical mechanisms operating in the Irminger Sea have to do primarily with convection. The convection processes in the Irminger and Labrador Seas are rapidly ($\sim$2 yr) and intimately correlated (Falina et al. 2007; Yashayaev et al., 2008), so we can assume that the results here presented might not only be representative of the Irminger basin based on a more or less narrow line, but of the Subpolar North Atlantic (SPNA).

Based on the fact that the average water mass renewal times in the Irminger ($\sim$5 yr) is considerably smaller than the C_{ant} rate of increase in the ocean ($\sim$60 yr for C_{ant} to double its concentration, according to Toste et al. 2006), it can be reasonably assumed that the C_{ant} concentrations in the different layers found in the studied section can be extrapolated to the whole Irminger basin. This is now included in the manuscript (last paragraph of the results section).

Falina, A., A. Sokov, and A. Sarafanov, Variability and renewal of Labrador Sea Water in the Irminger Basin in 1991–2004, J. Geophys. Res., 112, C01006, doi:10.1029/2005JC003348, 2007.

Tanhua, T., A. Biastoch, A. Körtzinger, H. Lüger, C. Böning, and D. W. R. Wallace, Changes of anthropogenic CO2 and CFCs in the North Atlantic between 1981 and 2004, Global Biogeochem. Cycles, 20, GB4017, doi:10.1029/2006GB002695, 2006.

The structure of section 3 (Results) should be better elaborated. There are several back-and-forth jumps in the discussion, from one water mass to another, from one parameter to another. This might lead to confusion. Many aspects concerning changes in T and S of different deep water components have already been discussed by e.g. Yashayaev and Dickson (2008, ASOF book), Yashayaev et al. (2008, ASOF book) or earlier by Falina et al. (2007, JGR). Based on their own T/S data, basically, the present authors come to similar conclusions. Of course, they need these to interpret the carbon-related results, but one might shorten this section a bit and strengthen the focus (please, refer to comment above on the MPD). The section dealing with Csatant or its percentage is not really clear and needs to be revised. Some material is presented as a table (e.g. table 2). Increase versus decrease of certain properties as discussed in the text is, however, easier to infer, if it is presented as figures rather than as data in a table (PLEASE, SEE NEW FIGURE 3 which includes all of these suggestions). I also strongly recommend adding isopycnals to figure 2, so the reader can detect changes in the stratification and layer thicknesses of the particular water masses (Figure 2 has now largely improved following your suggestions. Thank you.). In some occasions I found it a bit disappointing how the authors use their citations. In some cases, references are missing or are incomplete in the reference section. In other cases, citations are used to corroborate statements made by the authors, but they are not always appropriate and should be used more carefully (We beg you pardon for having skipped some references. Throughout the text, and following your comments,

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the citation irregularities have been fixed). In the following, specific remarks are listed and sorted by page numbers and then line numbers. These might help the authors to identify those statements which need clarification. After comments related to the text, comments related to tables and figures are added.

Specific remarks

page 1588

line 15: The reference "Canadell et al. (2007)" is entirely missing in the reference section.

Done

lines 16-18: At first, the uptake of CO2 is governed by the air-sea gas exchange. The deep water formation as one of the key processes of the MOC is then the gateway for CO2 to enter the deep ocean. Following Wallace (2001) storage and uptake should not be confused. This is is used interchangeably throughout large parts of the manuscript and should be clarified.

Thank you for having noticed this. We have gone through the whole manuscript and have used the most appropriate ad hoc term.

lines 23-25: It should be noted that there is on-going discussion concerning the interpretation of the results presented by Bryden et al. (2005). The presented time series has a very coarse temporal resolution (5 particular estimates in 5 decades) and the slowing signal is comparable to the uncertainty (see Kerr, 2005, Science.

This is the information and results that we have available at the moment. The shut-down scenario is unlikely, according to the last IPCC report (2007), and the revised manuscript includes now the message of the MOC slowdown being quite probable, instead of the unlikely shut-down:

“Recent studies on the variability of the MOC point towards a possible decrease in its

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intensity during the second half of the twentieth century (Bryden et al., 2005). The causes for it are mainly attributed to the greenhouse-enhanced temperature rise and precipitation in high latitudes, where water mass formation processes abound. Based on multi-model simulations, an average MOC reduction of 25% (0-50%) by year 2100 under a SRES emission scenario A1B is very likely to occur (IPCC report, 2007).”

lines 25-26: Citing McManus et al. (2004) in this context is probably not quite correct, since they did not pose a warning related to a possible future shutdown of the MOC as a response to global warming.

OK. The reference has been removed.

page 1589

line 2: Reference "Drijfhout et al. (2006)" is missing entirely.

Solved

line 5: Reference "Böning et al. (2006)" is entirely missing in the reference section. Several OGCMs are mentioned. Please, be more precise. If you do not simply refer to Böning et al. (2006), please, list further studies since Böning et al. (2006) basically analyzed a set of three different OGCMS from the same family of models which differ in their horizontal resolution.

Done. The Böning et al., 2006 reference has been added to the references section. Also the following references have been added: Cubasch et al, 2001 and IPCC report, 2007.

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Year	Cruise	Layer	Cant	N^o data (N)
1991	MET 18 / A01	subsurface	33.6 ± 1.3	33
1991	AR7E	subsurface	34.6 ± 1.8	13
1997	MET 39 / 4	subsurface	38.5 ± 1.5	27
1997	FOUREX	subsurface	38.8 ± 0.8	100
1991	MET 18 / A01	uLSW	30.2 ± 1.0	37
1991	AR7E	uLSW	31.3 ± 0.9	46
1997	MET 39 / 4	uLSW	31.2 ± 1.2	27
1997	FOUREX	uLSW	32.1 ± 0.7	79
1991	MET 18 / A01	LSW	27.3 ± 1.0	49
1991	AR7E	LSW	24.6 ± 0.9	62
1997	MET 39 / 4	LSW	29.6 ± 1.0	47
1997	FOUREX	LSW	29.9 ± 0.7	92
1991	MET 18 / A01	ISOW	18.4 ± 1.0	46
1991	AR7E	ISOW	12.9 ± 0.8	69
1997	MET 39 / 4	ISOW	20.0 ± 1.2	28
1997	FOUREX	ISOW	18.5 ± 1.0	43
1997	MET 39 / 4	DSOW	15.4 ± 1.8	11
1997	FOUREX	DSOW	12.7 ± 1.2	26
1997	MET 39 / 4	DSOW	20.5 ± 2.4	11
1997	FOUREX	DSOW	19.8 ± 2.2	13

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Longitude	Latitude	Station	Max. CTD	In Situ Temp.	Salinity CTD	Theta CTD	Theta bottle	$Sigma_0$	DSOW
(deg. East	**(deg. North)**	**No.**	**press. (dbar)**	**CTD (^oC)**		**(^oC)**	**(^oC)**	**CTD**	**Presence**
-42.1342	59.1317	86	2247	2.1801	34.8762	2.0185	2.0192	27.8711	NO
-41.9208	58.9922	85	2513	1.6739	34.8784	1.4976	1.4977	27.9129	YES
-41.7423	58.8578	84	2773	1.4021	34.8813	1.2076	1.2075	27.9359	YES
-41.4773	58.6888	83	3001	1.4286	34.885	1.2122	1.2121	27.9386	YES
-41.0638	58.407	82	3177	1.4384	34.8905	1.2048	1.2060	27.9435	YES
-40.6222	58.084	81	3239	1.768	34.8912	1.5206	1.5250	27.9214	YES
-40.0462	57.7033	80	3273	1.9636	34.8943	1.7081	1.7082	27.9099	YES
-39.4662	57.307	79	3269	1.8956	34.893	1.6421	1.6428	27.9138	YES
-38.9073	56.9255	78	3317	2.5165	34.9169	2.2432	2.2421	27.8853	NO

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